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AN

ELEMENTARY SYSTEM

OF

PHYSIOLOGY.

COMPLETE IN ONE VOLUME.

BY JOHN BOSTOCK, MD,
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OF THE LITERARY AND HISTORICAL SOCIETY
OF QUEBEC, ETC., ETC.

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PREFACE

TO THE FIRST VOLUME OF THE FIRST EDITION.

WHEN we reflect upon the distinguished share which our countrymen have borne in improving the knowledge of the animal œconomy, it may appear not a little remarkable, that we have no original work in our language, which contains a systematic and connected view of modern physiology[*]. My object in the following pages is to endeavour to supply this deficiency, by presenting the student with a concise view of the present state of the science, embracing an account of the most important facts and observations, as well as of those theories and hypotheses which have been the most generally received, or have been sanctioned by the authority of the most eminent names. As my design is to furnish an elementary treatise, my first aim has been perspicuity, both in language and in arrangement; and although it has not been my intention to produce what may be styled a popular work, yet I conceive that it contains so little of what is strictly technical, as to be generally intelligible to those who are not conversant with the medical sciences. Viewing it however, in connexion with its appropriate object, I have endeavoured, in all cases, not merely to afford the student a digested abstract of the present state of our information on the various topics which it embraces, but by referring him to the sources whence I have derived my information, to enable him to examine for himself how far I have given a correct account of them, and to assist him in pursuing the investigation of any part in which he may feel more particularly interested. And I may here remark, that in no instance have I given a reference to any book which I have not myself examined, and that in order to facilitate the progress of those who may be disposed to follow me in this track, I have appended a list of the works that have been consulted, with the dates of the editions employed.

---

[*] Since the above was written, systematic works on physiology have been published by Mr. Mayo, Dr. Alison, and Dr. Elliotson; I cannot give a better proof of my estimation of their value, than by the frequent reference which will be made to them in this volume. To the above list I may add the elegant and philosophical Bridgewater Treatise of Dr. Roget, a treatise, which although written for a specific purpose, and in illustration of a particular train of reasoning, contains a correct and luminous account of most of the topics which fall under the notice of the physiologist.

With respect to references, the plan which I have proposed to myself has been to indicate, in all instances, the original sources of my materials, while, at the same time, I have avoided filling my pages with a multitude of quotations, which could afford no additional authority to the subject in question. On one point I profess myself to have exercised all the care of which I was capable, in scrupulously assigning to each individual the share of merit which justly belongs to him in the discovery of facts or the formation of theory. During the progress of information, and while knowledge is rapidly advancing, it not unfrequently happens that the same fact is discovered, or the same train of reasoning developed, by different individuals entirely independent of each other; but where the priority of publication is clearly proved, we ought to be cautious in admitting any claims that are subsequently brought forwards, while all instances of that disingenuousness which aims at suppressing the names of preceding writers, for the purpose of procuring a surreptitious celebrity, cannot be too severely reprobated. But to the credit of the English be it said, that such examples are among them extremely rare. It is impossible to peruse the various scientific and medical journals of Great Britain without observing, that so far from keeping back the various discoveries that are made in other countries, one of their first objects is to obtain priority of information on these topics, and it is truly gratifying to observe with what quickness and accuracy it is transmitted to us from the various parts of the continent.

I feel it incumbent upon me to make some observations upon the portion of the work which I have devoted to the consideration of physiological theories. When we call to mind the fate of those which have hitherto been given to the world, how even the most elaborate of them, and those which appeared to be the best founded, have been successively discarded, and given place to some new speculation, which has, in its turn, shared the fate of its predecessors, we might be tempted to regard the whole as undeserving of any share of our attention. But, although the truth of the foregoing statement cannot be denied, still, I apprehend, the subject possesses many claims upon us. It might be sufficient to allege that the theories of eminent physiologists form a curious part of the history of the science, that they mark the progress of knowledge, and exhibit, in an interesting point of view, the operations of the mind in its attempts to arrive at truth. It can surely never be considered as an unimportant or trifling pursuit to inquire how such men as Haller and Hunter reasoned, or by what mode of investigation they were led to the discovery of the truths which immortalize their names.

And further, although no one in the present day can be insensible to the comparative value which ought to be attached to facts and to opinions, still they are often so intimately blended together that it is extremely difficult to separate them, and this is more especially the case in the study of physiology, where, from obvious causes, it is much less easy to make observations, or to perform experiments that may lead to unexceptionable results, than when we operate upon inanimate matter. Even the most unfounded, and, as we now conceive them, the most absurd theories of the chemical and mathematical physiologists of the seventeenth century, were, to a certain extent, derived from what, at that period, was called experiment, and which satisfied the minds of the learned men of the age; yet if we except the simple detail of anatomical observation, there is perhaps scarcely a single statement against which we should not now be disposed to offer some objections. And there is another circumstance which renders it necessary for the student to be made acquainted with the most noted theories of the older writers, that without this knowledge their works would be unintelligible; for such complete possession had these topics taken of their minds, that we can scarcely peruse a single page without meeting with some theory, either expressed or implied, which is intimately connected no less with their observations than with their reasoning.

And I shall further advocate the cause of theory, as being a direct means, and that a very important one, of acquiring a knowledge of facts. In prosecuting our investigations we are necessarily guided by some object, and this is, in most cases, a preconceived hypothesis, which we wish either to put to the test of experiment, or to inquire how far it can be reconciled with the structure of the body or the operation of its functions. This is the natural process by which information is acquired; the errors into which we have so frequently fallen do not consist in the legitimate employment of hypothesis, but in our being so much influenced by it as to "mistake the scaffold for the pile," to regard a train of reasoning in the same light with a deduction from facts. Who that has attended to the state of chemical science for the last 50 years can reasonably doubt, that its progress was prodigiously accelerated by the formation of the Lavoisierian theory, by classing the insulated facts in a systematic form, by generalizing the conclusions that appeared to be fairly derived from them, by introducing a uniform nomenclature, and by discarding a mass of antiquated opinions and phraseology? Yet this edifice, so beautiful in its separate parts, and which seemed so consistent as a whole, and so firmly connected together, appears destined to fall before the powerful genius of Davy; its strongholds have been assailed, and it totters to its very base.

One of the prevailing errors of the present day I conceive to be a fondness for constructing new arrangements, and for introducing new terms into all the physical sciences. This is, no doubt, partly owing to the rapid increase of information, which, to a certain extent, produces a necessity for a change both of system and of language; but the innovations have been carried to a most unreasonable length. Indeed this is so much the case, that in some departments, the attention is almost entirely engrossed by the study of nomenclature, while in consequence of the variety of denominations that are given to the same object, and the number of technical terms with which the memory becomes charged, we defeat our very end and object, which is to produce a uniformity of names, and a greater simplicity in our designation of things. In the following work it has been my aim to form an arrangement, which shall be no further technical than is absolutely necessary for announcing the subjects as they successively fall under our notice; scarcely a single new term is introduced, while, with a very few exceptions, I have employed the old terms in their most generally received acceptation, and have endeavoured always to use them precisely in the same sense.

Although I am not sensible that I have omitted any reasonable means for rendering my work complete, yet I am fully aware that, after all the pains that I have bestowed upon it, it must contain many parts that require correction, both with respect to the statement of facts and the inferences that are deduced from them. It is, in a great measure, from this consideration that I have determined to publish it in separate volumes, in order that I might be able, in the second volume, to correct the errors and supply the deficiencies which should be pointed out as occurring in the first. And I beg to remark upon this subject, that I shall pay every attention to the criticisms that are made upon the work, and shall thankfully avail myself of all the information that I can obtain from this source.

I cannot conclude these remarks, without adverting to the great advantage which I have derived in the prosecution of this work, from the very extensive and valuable library of the Medical and Chirurgical Society. When we reflect upon its recent date, and bear in mind, that it was established by a few private individuals, depending for its support solely upon the sense of its utility, we cannot estimate too highly the public spirit of those who were the most active in its original formation. To the survivors it is a sufficient reward to witness the success of their labours, but I trust that I shall be pardoned for introducing in this connexion the name of Dr. Marcet, among whose claims to the gratitude of his profession, perhaps the very considerable share which he had in the establishment of the Medical and Chirurgical Society, must be considered as the most important[1].

Upper Bedford Place,
Dec. 4, 1823.

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[1] I am aware that it is seldom proper to obtrude upon the public feelings of a personal nature, yet I shall hope for the indulgence of my readers if I offer my testimony to the distinguished merits of my much valued and ever to be lamented friend. His character has been so justly delineated by the elegant and correct pen of Dr. Roget, that I can do no more than give my warm assent to the sentiments expressed in his memoir, an assent sanctioned by an unreserved and confidential intimacy of twenty-eight years.

TO

ARTHUR AIKIN, ESQ., F.L.S. & G.S.,

SECRETARY TO THE SOCIETY OF ARTS, &c.

MY DEAR SIR,

THE first volume of this Treatise I had proposed to have inscribed to our much valued friend, Dr. Marcet, under whose inspection the work was commenced, and from whom I received many important suggestions, respecting both the plan and the mode of execution. His premature death prevented me from fulfilling my design. To the present volume I beg leave to prefix your name. A friendship, which may be said to have been transmitted to us from our parents, which commenced in our childhood, and which has continued to the present period, without the slightest interruption, might alone be sufficient to justify my choice. But, independently of any private feelings of this description, I am anxious to embrace this opportunity of giving my public testimony to your excellent moral qualities, and to your varied scientific acquirements; qualities which are the more esteemed the more they are known; and acquirements which have been uniformly employed in the improvement of the useful arts, or in the advancement of knowledge.

Believe me, my dear Sir,
Your very sincere and faithful Friend,
J. BOSTOCK.

Upper Bedford Place,
March 5th, 1826.

PREFACE

TO THE SECOND VOLUME OF THE FIRST EDITION.

THE reception which the first volume of this treatise experienced, could leave no doubt in my mind as to the necessity of completing the work; and I lament that certain circumstances, which were unavoidable, have delayed for so long a period the publication of the second part. As, however, the circumstances to which I allude no longer exist, I may indulge a hope that the third volume will succeed the present at a considerably shorter interval. I have, as nearly as possible, pursued the same method of giving an account of the best established facts, and the most approved hypotheses; freely offering my remarks upon them, and pointing out any part which appeared to be objectionable. I have, however, found it a very difficult task to arrive at any satisfactory conclusion, with respect to many of the topics that are discussed in this volume. For singular as it may appear, although most of them profess to be established on the basis of direct experiments, and such as would appear not to be of very difficult execution, yet we shall find that every step of the track through which I have had to pass is on debateable ground. On this account, I have frequently felt it necessary to dissent from the opinions of the most eminent physiologists of the age; but when I have done this I have given my reason for the dissent; and I trust that I have, in no instance, gone beyond that candid criticism, which it is necessary to exercise on all scientific topics.

I beg to repeat the request with which I concluded the preface to my former volume, that my readers will use towards me the same liberty which I have used towards others; that they will, without reserve, point out all the errors and imperfections which may be found in the work. I have thought it, upon the whole, more advisable to defer the notice of these remarks until the publication of my third and concluding volume; as by this means I shall have the advantage of another year's experience, an advantage which is of no small moment in a science so rapidly progressive as that of Physiology.

TO

WILLIAM BABINGTON, M.D. F.R.S. F.G.S.

&c. &c.

DEAR SIR,

I TRUST that you will excuse the liberty which I have taken in prefixing your name to my third volume. The friendship with which you have honoured me, since my residence in the metropolis, might plead my excuse; but I will acknowledge, *that I have been principally* induced to make use of your name from the kind interest which you have taken in the progress of my work, and the approbation which you have bestowed upon the former parts of it. It is impossible not to feel the value of commendation when it proceeds from such a quarter, from one who is so thoroughly conversant with all the topics which form the subject of my treatise. The profound knowledge and the ample experience which you possess of the morbid actions of the animal œconomy enable you to form a correct judgment of the degree in which the practice of medicine may be expected to derive benefit, by investigating the functions of the living body in their healthy state, and the interest which I know you attach to these inquiries affords the best warrant of their importance.

I am, dear Sir,
With every feeling of esteem and respect,
Your obliged friend and obedient servant,
J. BOSTOCK.

Upper Bedford Place,
May 23d, 1827.

PREFACE

TO THE THIRD VOLUME OF THE FIRST EDITION.

THE present volume completes the plan which I originally proposed, of giving a summary view of the present state of physiological science. I am far from supposing that the method which I have pursued is the best which could have been adopted, or that it is perfect in its execution; but I may be permitted to say, that the deficiencies of my work do not arise from any want of care or attention on my part, and I believe that no material improvement would have arisen by longer deferring its publication. Many of the topics that are treated of in this volume are such as do not always fall under the cognizance of the physiologist; yet I consider them as bearing so intimate a connexion with the animal frame, as to afford sufficient ground for taking at least a cursory view of them. We have observed in almost every branch of the subject that has fallen under our examination, the greatest diversity of opinion to prevail, and we have found that this is the case even on topics which seem to admit of being decided by a direct appeal to experiment. It cannot, therefore, excite surprise that obstacles almost innumerable should assail us at every point, when we attempt to penetrate the intricacies of metaphysics, where we have to treat upon subjects, the very conception of which is difficult to attain, and where we have nothing to guide our researches but imperfect deductions and doubtful analogies. It is, however, not a little remarkable, that it is on these dubious points that mankind have shown the most pertinacity of opinion, and have been the least disposed to manifest a spirit of candour towards those who have differed from themselves. On such subjects the utmost that I can expect to have accomplished, is to have endeavoured to free my mind from prejudice, and to state my opinion with that cautious moderation, which is fitted to the uncertain nature of the evidence on which it is necessarily founded.

Considering that three years have elapsed since the publication of my first volume, I have found occasion for less alteration and correction in the former part of the work than might perhaps have been expected. My readers will perceive that I have taken the opportunity of introducing in various parts of the notes some of the necessary corrections and additions; the remainder I have placed in an appendix. I will not venture to presume, that these are all the deficiencies and inaccuracies which the work may contain, but they are all that can be ascribed to inadvertency. As to the errors of judgment, or the defects of information, I must leave them to be rectified and supplied by my successors, being satisfied with the reflection, that my attempt may have the effect of smoothing the path to one of the most interesting and ennobling pursuits that can possibly occupy the human mind.

ADVERTISEMENT

TO

THE THIRD EDITION.

IN presenting to the public a third edition of my System of Physiology, I shall beg to state, that I have endeavoured to render it worthy of their patronage, by inserting into it an account of the numerous additions which the science has recently experienced. This I have done in as brief a manner as was consistent with the object of the work, and in order that it might not be unnecessarily extended, I have omitted some parts and re-modelled others, in conformity with the present state of our information on the various topics which it embraces. I may farther remark, that I have, in most cases, preferred appending the new matter in the form of notes to interweaving it into the text, in order that those who are in possession of the former editions, may be enabled more easily to distinguish the additions that have been made to the present.

I must not omit to express my grateful acknowledgements to the President and Council of the Royal College of Surgeons, for the use of their library, which was most liberally granted to me; a library not unworthy of the noble institution with which it is connected. I am also happy to have an opportunity of expressing my sense of the kind attention and valuable assistance which I have, at all times, received from their intelligent and excellent librarian, Dr. Willis.

Upper Bedford Place,
Nov. 1st, 1836.

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INTRODUCTORY OBSERVATIONS.

The term Physiology, according to its original meaning, is nearly synonymous with Natural Philosophy; but it has, for a long time, been always used in a more limited sense, and restricted to that branch of science, which treats of the functions of the living animal body, and of the powers by which these functions are exercised[1].

Notwithstanding the value which must have been, at all times, attached to the study of the animal body, both as holding the first rank in the scale of natural objects, and as being intimately connected with the various departments of medicine, its functions were seldom made a distinct object of investigation, until the beginning of the last century. Although the writings of the ancient physicians, and of the earlier among the moderns, abound in physiological speculations, they are rarely brought forwards in a connected or systematic form; so that we are obliged to collect our knowledge of their tenets, more from a number of scattered fragments, that are dispersed through works on medicine and pathology, than from treatises expressly devoted to the subject.

Hippocrates may be regarded as the father of physiology as well as of medicine, although from the more complicated nature of the former, the actual advances which he made in it were probably not very considerable. We observe in his writings many traces of the Pythagorean philosophy, but, at the same time, we meet with a large proportion of what is original, or, at least, what has not been traced to any other source. One of his leading tenets is the existence of a principle, which he styles nature, (φυσις) and to which he ascribes the direction and superintendence of all our corporeal actions and movements. To this principle he attributes a species of intelligence, and conceives that one of its most important offices is to attach to the body what is beneficial, and to reject from it what would prove injurious; an hypothesis which, although expressed in different ways, and clothed in a more or less mysterious form, has continued to be a popular doctrine to the present day. Besides this nature, which is regarded as the prime agent, there are other subordinate principles or faculties, (δυναμιες) which especially operate in the production of the various functions.

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[1] Some of the continental writers, as Treviranus and Fodera, have lately employed the term Biology, as designating the science, which essentially consists in the knowledge of those properties which distinguish animate from inanimate matter; this, however, may be considered as applicable rather to the general principles, which constitute the theory of the science, than to the descriptive part from which these principles are deduced.

With respect to the body, he conceives it to be composed of three kinds of substances, solids, fluids, and spirits, which are themselves formed by the combination of the four primary elements. The nature of the body is supposed to be materially affected by the nature of the four elements which enter into its composition; as well as by the four qualities of hot, cold, moist, and dry; which, by their respective combinations and proportions, produce the four temperaments. These are considered as original predispositions existing in the body, influencing both its mental and corporeal character, and laying a foundation for the diseases to which the individual is more especially liable. In his account of the different functions of the body, although we observe many marks of sagacity and acuteness, yet there is much that is inaccurate and erroneous. His acquaintance with the minute structure of parts was limited; but little was known of the nature of the external agents which affect the corporeal organs; while the use of the organs themselves was derived from vague conjecture or false analogy. Hippocrates also adopted the mysterious opinions of Pythagoras respecting the occult power of particular numbers; and he believed that the stars exercise an influence over the operations of the body[1].

Little or no advance was made in the science of Physiology from the time of Hippocrates to that of Aristotle. The genius of Aristotle and the course in which it was directed were, in many respects, well adapted for the improvement of this science. He appears to have been the first among the ancients who advanced comparative anatomy and natural history to any considerable degree of perfection; and while he enjoyed great advantages for obtaining information on these topics, he cultivated these advantages with much assiduity. Hence he acquired an extensive acquaintance with natural objects, and may be considered as having made an actual advance in our knowledge of the animal œconomy, although perhaps less than might have been expected from the means of information which he enjoyed, and the powers of mind which he displayed on other topics.

After the death of Aristotle we have another long interval, during which no progress was made in physiological science, when it received a new impulse from the genius of Galen. A considerable share of the celebrity which this extraordinary character attained is derived from his physiology. When we compare his treatise "On the Use of the Parts of the Body," with any work on the same subject published before his time we cannot but admire the superiority of his information, and the ingenuity with which he applies it to the explanation of the animal œconomy. Yet on both these points he is not without considerable deficiencies and inaccuracies; his physiology is frequently founded upon fallacious principles; and in his application of such as are more correct, he displays more of what may be termed ingenuity, than of that cautious discretion, which is so necessary in the investigation of any intricate point, connected with the actions of vitality.

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[1] The best view of the Physiology and Pathology of Hippocrates, which are every where blended together, is contained in his treatise "De Natura Hominis;" Opera a Fœsio, t. i. p. 224..231. For a more detailed view of the character and writings of Hippocrates, I shall beg to refer to the second chapter of my History of Medicine.

Galen was a warm admirer and encomiast of Hippocrates; he professed to agree with him in all his fundamental doctrines, and to aim at little more than to elucidate and amplify his principles. But although he sets out from the same point, he soon deviates into a more intricate path; and he becomes so involved in abstruse and complicated hypotheses, that we are no longer able to trace the simplicity of the original in the refined speculations of his commentator. He assumes the four elements, and the four qualities; but in his application of them either to physiology or to pathology, he introduces so many minute distinctions and intricate combinations, as to give a new aspect to the doctrine. The real merit of Galen, however, consists in his knowledge of anatomy and in his acquaintance with the minute structure of parts, in which he made very considerable advances upon his contemporaries. The diligence which he displayed on these points is worthy of our warmest applause; yet the encomiastic flattery of his followers, by the excess to which they carried their admiration, has perhaps somewhat tended to diminish his reputation. From many circumstances connected with the history of the age, as well as from the candid confession of Galen himself, we may conclude that he rarely, if ever, dissected the human subject, but that he examined the bodies of apes, and of other animals the most nearly resembling it; and from these, by making what he deemed the proper allowances, he draws up his descriptions[1]. But his zealous disciples would not admit of what they thought an imperfection in the works of their master; and to such an extent did they carry this principle, that they even considered it as more probable that the human body should have undergone a permanent change in its anatomical structure, than that Galen could have committed an error.

The superior talents of Galen, and the unrivalled reputation which he obtained, seemed to repress all further efforts for the improvement of physiological science; and his immediate successors, regarding him as beyond the reach of competition, were satisfied with implicitly adopting his opinions, without attempting to inquire into their correctness, or to extend their application. The spirit of the times but too powerfully coincided with this feeling. The Roman empire began to exhibit unequivocal marks of decline: in every department of literature there was a deficiency of genius, and nothing more was now attempted than to imitate the standards of excellence which had adorned the preceding age. But Rome, even in its most splendid period, had bestowed little attention upon the physical sciences; and the efforts which were now made were altogether imperfect and unavailing. Nor was the revival of letters in the 15th century, which roused the intellectual powers, after a dead repose of nearly 1000 years, productive of the same benefit to physiology as to many other departments of science. From a variety of causes, partly perhaps of an incidental nature, and partly depending upon the limited knowledge which was then possessed of the powers and properties of natural bodies, the physiologists of that period fell into the error of ascribing the phenomena of life to the operation of the laws which influence inanimate matter. Hence arose the contending sects of the chemists and the mathematicians; the former accounting for all the operations of the animal œconomy by the chemical action of the components of the body upon each other, the latter by the principles of mechanics. It is not necessary, in the present day, to enlarge upon the waste of genius and the misapplication of experimental research, which originated from this fatal error; it may be sufficient to remark, that although important facts were occasionally brought to light, and many elaborate investigations were instituted, from which some valuable information may be deduced, yet that not one single hypothesis was proved, nor one single principle established, of all those upon which so much labour and learning were bestowed.

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[1] Hist. of Medicine, p. 87.

While the chemical and mechanical sects were thus dividing the opinions of the most learned men of the age, a new doctrine was gradually rising up, which, although in the first instance it was equally remote from the principles of true science, yet after having received a number of successive purifications, it at length appeared in a more correct form, and occasioned the complete overthrow of both the contending parties. For this revolution we are indebted principally to Stahl. This distinguished character was brought up in the school of the chemists; but being possessed of a powerful understanding, he soon deserted the tenets of his preceptors, from a full conviction of their futility. He was forcibly impressed with the difference between the changes which the components of the body experience during life, and what would take place in the same substances under other circumstances. Hence he concluded that when they form a part of the living system, they must be possessed of some additional principle which counteracts the effects that would otherwise be produced. To the agent which thus opposes the physical powers of matter, and to which the body owes its vital properties, he gave the name of *anima*. He conceived it to possess powers of a specific nature, and he especially attributed to it a species of intelligence, which enables it to act the part of a rational agent, and to superintend all our corporeal operations[1].

To Stahl, therefore, we must ascribe the merit of clearly perceiving the inadequacy of the actions of either chemical or mechanical causes to explain the phenomena of life, a truth which we now regard as incontrovertible, and which, obvious as it appears, had been overlooked or disregarded by the most acute and learned of his predecessors. But after having thus established a firm basis for his hypothesis, he was led astray by the fashionable metaphysics of the day. Instead of investigating the nature of the animal functions, and ascertaining the laws which direct them, he deemed it sufficient to refer them all to an hypothetical principle, which he invested with powers accommodated to his purpose. In its general aspect the *anima* of Stahl may seem to bear a near relation to the φυσις of Hippocrates; but there is this essential difference between them, that Hippocrates employs his term merely as a general expression of the facts, whereas Stahl considers his hypothetical principle as something distinct from the body, which actually produces its powers and faculties. But although the hypothesis of the *anima* was in itself gratuitous and altogether objectionable, it had the good effect of turning the attention to the phenomena more immediately connected with life, of enabling us to trace their connexion with the other operations of nature, and ascertaining more correctly the laws by which they are respectively governed. This progress was indeed attended with much difficulty, and the advances which were made in it were very gradual; but it is the correct plan of proceeding, and that which must eventually lead to the true theory of animal life, and to the just principles of physiology[2].

When knowledge is acquired by slow degrees, and truth is not elicited until after many unsuccessful efforts, it is not easy to assign to each individual the exact share which he contributed to the progress of improvement, or to ascertain precisely in what proportion his exertions may have conspired to the final result. In the present instance I am disposed to attribute a considerable share of merit to Hoffmann, who although a hasty and multifarious, rather than a correct and consistent writer, seems to have been one of the earliest who entertained correct notions respecting the general principles and objects of physiology. He had the sagacity to perceive that much of what Stahl ascribed to the operation of his *anima*, might be more correctly attributed to the action of the nervous system; and he appears to have been among the first who duly estimated the importance of this part of our frame in the vital operations[1].

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[1] Theor. Med. ver. Physiol. sect. 1. mem. 3. § 13. It may afford a topic for literary discussion, how far Stahl borrowed his notions from Vanhelmont, whose hypothetical agent, which he named *archeus*, bears a near resemblance to the *anima*. But I should be disposed to refer Stahl's doctrine to Hippocrates, with whose writings he must have been conversant. I may also remark, that opinions very similar to those maintained by Stahl may be found in Aristotle's treatise De Anima.

[2] A judicious summary of the physiology of Stahl, Hoffmann, Boerhaave, Haller, and Cullen, is contained in Dr. Thomson's learned and elaborate Life of Cullen; a work which may be regarded as a philosophical history of medical science during the beginning and middle of the 18th century. I shall also beg to refer to my History of Medicine for some remarks on the respective merits of these writers.

Contemporary with Stahl and Hoffmann was Boerhaave, a man perhaps equal to them in the general powers of his mind, or, if he possessed less genius and originality, he was superior in judgment and information. No one ever enjoyed greater fame as a teacher; and from this circumstance, as well as from their intrinsic merit, his doctrines acquired a degree of ascendancy over the public mind which had, perhaps, not been equalled since the time of Galen. But the genius of the age was not favourable to the continued dominion of any hypothesis; and as the theory of Boerhaave wanted the substantial support of facts, its celebrity did not long survive its founder. He was a professed eclectic; he selected from all preceding writers what appeared to be valuable in their respective systems, and endeavoured to mould the materials thus collected into one harmonious whole. Hence his system was the result rather of learning than of information; and although it indirectly tended to the detection of error, it induced a state of mind which led to its own downfal.

But whatever advances may have been made until this period in physiological science, they will appear of small amount when compared with the mass of knowledge which burst upon us about the middle of the last century, and for which we are principally indebted to Haller. This celebrated man is, in every point of view, entitled to the appellation of the father of modern physiology, whether we regard the unremitting assiduity with which he cultivated the science, or the actual advancement which he effected. Every circumstance of talent, character, and situation, conspired to promote his great object. In learning, in industry, in discrimination, he has seldom been excelled; he devoted a large portion of his life to the cultivation of physiology, while his rank and fortune gave every facility to his exertions. What, however, more especially entitles him to the highest commendation, is the method which he introduced and established, of investigating the pheuomena of the living body solely by observation and experiment, and keeping hypothesis entirely in subjection to these two leading principles. So powerful an effect indeed have his influence and example produced, that, since his time, the science has assumed altogether a new aspect: and from the publication of his "Elements," we may date the commencement of a new era in physiology.

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[1] Hoffmann's writings occupy no less than six large folios, and as he is generally deficient in arrangement, it is no easy task to select those parts which may exhibit the clearest view of his doctrines. His great work, "Medicina Rationalis Systematica," contains many remarks upon the nervous system, which show how much importance he attached to it; see particularly lib. i. sec. 3.

This great monument of learning and industry was still in progress when Cullen entered upon his career; a man of a very different turn of mind, yet one who was eminently useful in this department of knowledge. He excelled in general views rather than in minute researches; and, without adding many new facts to our previous stock of information, he arranged into a very beautiful and interesting system those of which we were already in possession. Few persons have contributed more than Cullen to sweep away the useless rubbish of antiquity; and there is a spirit of philosophical scepticism that pervades his writings, which happily coincided with the inquiring genius of the age in which he flourished.

Among the authors who have been most successful in the cultivation of physiology, we must class John Hunter. He possessed a remarkable share of boldness and originality of conception; his mind was equally ardent and acute; and to these qualities he added the most patient industry in the investigation of nature, under every aspect in which she presents herself to our notice. The high situation which he held in this metropolis, both as a practitioner and a teacher, and the noble memorial of his talents, which is deposited in the College of Surgeons, have conspired to raise his reputation to the highest pitch of celebrity. In the explanation of the operations of life, he professed to proceed entirely upon the result of observation and experiment; but, in this respect, he exhibited a singular example of self-deception, for his writings are, in fact, full of hypothesis and abound with theories expressed or implied; hence he has unhappily introduced into physiology a kind of metaphysical language, which has certainly tended to impede the progress of science, by substituting new expressions for new ideas; thus leading us to suppose that we had gained an addition to our knowledge, when in fact, we were only employing new forms of speech. There is, however, no one, since the time of Haller, to whom the science is more indebted for new facts than to Hunter; and upon these his fame will be amply supported when his speculations are forgotten In his physiological hypotheses, Hunter makes perpetual reference to the existence and operations of what he calls the vital principle. It is not easy, on many occasions, to determine how far his expressions are to be received in a literal, or how far in a metaphorical sense, but many of them strongly resemble the Stahlian doctrine, of an intelligent principle, connected with the body, directing its motions, and preserving it from injury or destruction. In his explanation of the functions and operations of the living animal, he not unfrequently confounds physical with final causes, and attributes to the specific effects of life, actions that ought to be referred to the powers belonging to inanimate matter.

Among the modern physiologists there is no one who has more just claim to our attention that Bichat, whether we regard him as an observer of facts, or an improver of theory. In the course of a short life he acquired an accurate and extensive knowledge of anatomy, and made many discoveries in this department of science, which seemed to have been so entirely pre-occupied by his predecessors. In his views of the animal œconomy, he proceeded upon the principles of correct philosophy; he regarded the vital functions as of a description essentially different from any other natural phenomena, and diligently applied himself to obtain an accurate knowledge of them, to observe their relation to each other, and to arrange them accordingly. His classification will, indeed, in many of its parts, appear too refined, and his speculations to savour too much of metaphysical subtilty; but we must regard him as having possessed an unusual share of genius and acuteness, and as having made very considerable additions to the stock of physiological knowledge.

I shall close my account of the individuals, who have been eminently successful in the cultivation of physiology, by the honoured and lamented name of Cuvier. There are few persons, either in ancient or modern times, whose minds were better adapted by nature for the improvement of this science, or whose pursuits were more calculated to promote it in its various branches. He was as much distinguished for his industry and correctness, as for his genius and originality. His information, in every department of natural history, was so extensive, that it is difficult to decide with which he was the most intimately acquainted. In addition to these qualifications, he was perfectly candid and ingenuous, without prejudice or bigotry, ready to do justice to all his contemporaries, to whatever nation they belonged, or to whatever system they were attached. With respect to his physiology, he may be considered as an eclectic rather than as a professed systematic. Having before his eyes the whole range of nature, he watched all her operations with singular sagacity, and with the hand of a master, traced their connexion with each other, and the relation which they bore to the living animal body. The services which he rendered to physiology were rather derived from his profound knowledge of natural history, and from the correctness of his observations, than from any new theory which he broached, or from any experimental researches which he performed. His reputation rested not upon any single position which he established, or any peculiar doctrine which he defended, but upon the light which he threw over the whole science; and we may rest assured, that the more his works are studied, the more will his talents be admired and his character venerated.

This brief sketch of the labours of preceding physiologists will tend to point out both the cause of the imperfection of the science and the method of advancing it. We find that for a long course of years, every one who attempted to explain the operations of the animal œconomy, employed only those powers which belong to inanimate matter; and that, at a later period, when the inadequacy of this mode became apparent, instead of inquiring into the actual nature of the specific powers of vitality, it was deemed sufficient to have recourse to certain hypothetical principles, derived from false analogies or from the mistaken philosophy of the age. The more correct opinions of the present day, for which we are in a great measure indebted to the sagacity of Haller, have led us to conclude that all the appropriate actions of the living system may be referred to the two classes of motion and feeling; and that these depend upon two principles inherent in the body, contractility and sensibility, the one seated in the muscular fibre, the other in the nervous matter [1]. To the action of one or other of these principles, every corporeal change may be ultimately referred; and it is through their immediate operation that all the functions are performed. Hence we have a foundation for an arrangement of the functions into contractile and sensitive, to which I propose to adhere in the following work; and for reasons which will be hereafter more fully detailed, I shall begin with the former class. The functions which belong to this division are, the circulation of the blood, respiration, animal temperature, secretion, digestion, assimilation, absorption, and generation.

But, before I proceed to the individual functions, it will be necessary to give an account of the nature of the two powers of contractility and sensibility, and of the organs by which they are exercised; the muscles and the nerves. It will be also found advantageous to premise a description of membrane and bone, because these substances constitute, as it were, the basis of the body; and without some knowledge of their nature, it would be difficult to comprehend the operation of the muscles and the nerves, or the connexion which they have with the system at large.

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[1] Adelon employs the terms *locomotilité* and *sensibilité*; Physiol. t. i. p. 34; but it may be objected to the former that it is not sufficiently extensive in its application; on this point, and on the subject generally, the sixth part of his Physiology may be perused with advantage; t. iv. p. 531 et seq. Dumeril, in his "Zoologie analytique," defines an animal a being capable of digestion, feeling, and motion. Bourdon likewise characterizes animal life as consisting in feeling, spontaneous motion, and digestion; Principes de Physiol. p. 34. Dr. Elliotson extends the properties of sensibility and contractility to vegetables, including them both under the general denomination of excitability; Physiol. p. 3; this, however, I cannot but regard as a premature generalization. We have some judicious remarks on the subject by Blandin, in his notes to Bichat's Anat. Gén., "Consid. Gén." See also the Art. "Animal," by Cuvier, Dict. de Sc. Méd. t. ii. p. 142 et seq.; and the same by H. Cloquet, Dict. de Méd. t. ii. p. 411 et seq. The introductory part of Prof. Tiedemann's Physiology contains an ample account of every thing that is connected with this subject; but I must acknowledge, that his remarks appear to me not unfrequently to savour too much of metaphysical subtilty. We have a clear and correct view of the distinction between animals and vegetables, in the Art. "Animal," by Dr. Willis, Cyc. of Anat., v. i. p. 124 et seq.

After reviewing in succession the various contractile functions, I shall proceed to the other great division, the sensitive. These comprehend what are commonly styled the five senses; sight, hearing, smell, taste, and touch; and besides these there are other classes of sensations, which appear equally specific, although from the mode in which they operate, or the organ by means of which they are exercised, their distinct nature has not been so generally recognized. Of these, some of the most important are the sensation that attends muscular contraction, that of heat and cold, and that of hunger.

The connexion between the corporeal and mental part of our frame is so intimate, that it is impossible to acquire a complete knowledge of the one, without paying some attention to the other. There is a very important class of phenomena of an intermediate, or perhaps, more properly, of a compound nature, where an effect upon either a contractile or a sensitive function is succeeded by some intellectual operation, or where an intellectual operation produces a change in the action of the corporeal organs. Some of the more important of these will be briefly noticed; and although it will be my object to encroach as little as possible upon the province of the metaphysician, I shall be unavoidably led to consider some of those topics which are only indirectly connected with physiology; of this description are the effects of association, habit, imagination, sympathy, and volition.

A very curious subject connected with the animal œconomy which must engage a share of our attention, respects the causes which produce the differences between individuals, both those which more immediately affect the external form, giving rise to the varieties of the human species, as they are termed, and those which seem to depend more upon the internal actions of the system, constituting the temperaments. This subject will naturally lead us to notice the curious topic of craniology; and, connected with this, I shall venture to offer some observations upon the much controverted question, of the nature of the connexion between the intellectual faculties, and the organ by which they are exercised. In the last place, I shall make some remarks upon the natural progress of the animal body, from the commencement of its existence through its state of maturity, to its decline and final dissolution, by which its component parts fall into decay, and its appropriate powers are at first impaired and ultimately destroyed.

ELEMENTS

OF

PHYSIOLOGY.

CHAPTER I.

OF MEMBRANE.

When we examine the structure and composition of the animal body, the most obvious division of its component parts is into solids and fluids; the first being fixed and permanent in their nature, and affording the basis by which the general form is determined; while the latter are lodged in appropriate receptacles, formed by the solids, are generally in motion, or are undergoing some obvious changes in their quantity or quality. The science of anatomy, which professes to describe the mechanical structure of the body, and the physical relation which its parts bear to each other, is principally concerned with the solids; while both the solids and the fluids are equally the province of the physiologist, whose business it is to study the nature of all the substances that enter into the animal frame. The solids, as being the most durable part of the fabric, and as forming the organs necessary to prepare the fluids, and to apply them when prepared to their different uses, seem to offer themselves as the first objects of our attention; although, upon a more minute examination, we may find the fluids to be of no less importance in our œconomy, as either by the intervention of external agents, or by the action of their components upon each other, they are, in most cases, the media through which those operations are effected, which are essential to life. It must, however, be remarked, that the terms solid and fluid, as applied to the components of the body, are rather relative than positive; there is scarcely any part so solid, which may not, by desiccation or by mechanical compression, be rendered still more compact; and most of what have been called animal fluids are composed of water, containing different species of solid matter imperfectly dissolved, or merely in a state of mechanical diffusion. The principal varieties of solids, considered in relation to their form and structure[1], are the bones, with their appendages, the cartilages and the ligaments, the muscles with the tendons, the membranes of all descriptions, the various kinds of sacs and vessels, the fat, and the cerebral matter. If we arrange the solids of the body with regard to their chemical composition, and to the uses which they serve in the animal œconomy, we may place them under five divisions; the osseous matter, the membranous, the muscular, the adipose, and the cerebral[2]; and we may say, in general terms, that the comparative degrees of their solidity or fixedness are in the above order. I shall not, however, follow this arrangement in the description of the solids, as the plan which I propose to adopt in the following pages is founded rather upon the functions which the body exercises, than upon its composition. I propose to give an account of both the solids and the fluids, as I successively treat of the functions of those parts in which they exist in the greatest quantity or the most perfect state; in consequence, however, of its general diffusion through all the organs of the body, it will be found convenient to commence with the membranous matter[1].

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[1] The method of viewing the animal body as composed not merely of a number of organs, but each organ as itself composed of a variety of textures, which are more or less common to the different organs, appears to have originated with Dr. C. Smyth, as illustrative of the phenomena of inflammation. It was carried to a much greater degree of minuteness by Bichat, who extended it to all parts of the body, and employed it as a leading principle of his systematic arrangement. It must no doubt be regarded as one of the greatest improvements that has been introduced into our science; but I have not formally adopted it in the following pages, because the elementary nature of this work seemed scarcely to render it necessary; while, at the same time, it might have led to useless and tedious repetitions. The reader may, however, observe that the general principle is always held in view, and is, in many cases, directly referred to. Smyth's paper is contained in the 2d vol. of the Med. Commun. p. 175. About the same time that it was read to the Society in 1788, but previous to its publication, which was in 1790, an arrangement of a somewhat similar kind was proposed by Pinel, in his Nosog. Philos. t. i. p. 135, 6. Bichat's arrangement was first published about the year 1800; it forms the foundation of his treatise on membrane. On the question of originality I may refer to the remarks of Dr. Milligan, in his translation of Magendie, p. 529 . . . 2.

[2] The number of textures proposed by Bichat is twenty-one; Anat. Gén. t. i. p. 36; they were reduced by his editor, Maingault, to eleven; Ibid. Bourdon has reduced them still farther to four, the cellular, the muscular, the osseous, and the fibrous; Prin. de Physiol. p. 43; and Beclard to three, the cellular, the nervous, and the muscular; his division of the cellular comprising the osseous, the membranous, and the adipose of the former arrangement; Add. à Bichat, p. 2. This is likewise the arrangement of Cuvier; Dict. des Sc. Nat. "Animal," t. ii. p. 158, and it is adopted in its essential parts by Mr. Quain, Anat. p. 24 et seq. Cloquet again extends the number to fifteen: Anat. Descrip. p. 5..8, and Knox's Trans. Ibid. Adelon makes the number of textures twelve, founding his arrangement partly on the mechanical structure, and partly on the uses of the parts; Physiol. t. i. p. 81. Rudolphi, proceeding more on their mechanical relations, fixes the number of the proximate solids at eight, the cellular, horny, cartilaginous, osseous, tendinous, vascular, muscular, and nervous; Physiol. by How, b. 2. ch. 1. Raspail in this, as in most other cases, takes a view of the subject which differs considerably from that of his predecessors; he assumes only three textures, the adipose, the albuminous, and the membranous, but of the latter he makes nine species; the muscular, nervous, bony, horny, cellular, respiratory, embryonal, parasitical, and spontaneous; this arrangement is an attempt to combine the anatomical, chemical, and physiological relations of the parts; see the arrangement of organized animal substances in the "Tabular View," p. 76. We have some judicious observations on the primary textures by Blandin; notes to Bichat; Anat. Gén. t. i. lxvii. See also Dr. Copland's account of various proposed arrangements of the primary solids and compound textures of the body, in his appendix to the Trans. of Richerand, p. 535 et seq. Dr. Craigie, also, in the first chapter of his anatomy, entitled "Division of the textures," gives an account of what had been done by others, and proposes his own arrangement; I am disposed to think that his divisions are somewhat too minute.

SECT. 1. *Extent and Structure of Membrane.*

Membrane is the most simple in its structure of any of the organized parts of the body; it is the most extensively diffused, and exists in the greatest proportion. The coverings, not only of the body at large, but of each of its individual parts, both internal and external, are principally composed of membrane; and it lines all the cavities in which the different organs are situated. It constitutes the main bulk of the bones, and determines their figure; the earthy matter upon which their strength and hardness depend, being deposited in a tissue of membranous cells. Membrane also enters into the structure of muscles, not only affording them an external sheath, in which they are each of them enclosed, but the same matter is also interposed between their fibres, separating them into bundles, to which it, in like manner, affords a distinct covering, and these into still smaller bundles, until it appears at length to envelop each individual fibre. The membranous matter composes very nearly the whole bulk of the tendons, by which the muscles are attached to the bones; of the ligaments, by which the bones and other solid parts are connected to each other; and of the cartilages, which form the basis of many parts of the body, supplying the place of bone, and which also cover the ends of the bones, and assist in the formation of the joints. It also enters very largely into the composition of the hair, the nails, and other similar parts connected with the surface. It likewise composes what is called the cellular texture, a series of cells or interstices, which extends over a great portion of the body, fills up its intervals, and serves to unite the different parts to each other. Membranous matter is the chief ingredient in the glands, both those which are attached to the absorbent system, and those which are appropriated to the office of secretion. The brain is also enveloped in a covering of membrane; and it is probable that the nerves are composed of a series of fibres enclosed in membranous sheaths, analogous to those of the muscles. The pouches or sacs, which are found in different parts of the body, such as the stomach and the bladder, are almost entirely composed of membrane; and what perhaps must be regarded as the most important of all the purposes which it serves, this substance composes the principal part of the tubes or vessels, with which the animal body is so plentifully furnished.

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[1] I have not thought it expedient to adopt any of the arrangements of the subjects of physiology, which have been formed, with so much labour and ingenuity, by some of the modern French writers; those, for example, of Dumas, Bichat, Richerand, Adelon, and Magendie. The object of these authors appears to have been to produce a system which should be equally applicable to physiology and to anatomy; but it may be doubted whether their plans have not become complicated and unnatural, in proportion as they are rendered more comprehensive. For some useful remarks on this subject, see Ed. Med. Journ. v. xv. p. 565. In the same work, v. 45, p. 236 et seq., we have a valuable article on the modern German and Italian physiologists. I am happy to bear my testimony to the continued excellence of this valuable *journal*.

From this account of the extent and distribution of membrane, we find that it must exceed in quantity all the other solids of the body taken together, and that it enters as a principal ingredient into almost every part of the animal frame, the enamel of the teeth being, as we are informed, the only solid in which it cannot be detected[1]. This is indeed so completely the case, that were it possible to remove the earth of the bones, the muscular fibre, the nervous matter, and the fat from the soft parts, to empty the vessels, and to carry off the fluids generally, the size and figure of the body would remain nearly unchanged. Membrane may therefore be considered as the connecting medium between the different parts of the body, by which they are held together, the basis to which they are all attached, and the mould in which the particles of the other kinds of matter are deposited[2].

It will appear from these observations upon the extent of membrane, that I employ the term in rather a more comprehensive sense than ordinary; and that I include under it, not only what have been usually called membranes, but the whole of the substance which possesses the same mechanical structure and the same chemical properties. What I have styled membranous matter nearly coincides with the white parts of the older anatomists, the cellular texture of Haller, the tissu muqueux of Bordeu[3], the tela mucosa of Blumenbach[4], and the systême cellulaire of Bichat[5]; the first of these terms was, however, used in a vague manner, without any very distinct appropriation; while Haller's, Bordeu's, Blumenbach's, and Bichat's appear objectionable, as they are derived from hypothetical opinions respecting the nature of this substance, which are probably not altogether correct.

The mechanical structure of membrane, as it exists in the different parts of the body, has been minutely examined by various anatomists, and was particularly attended to by Haller. He described it as composed of a vast assemblage of lines or fibres, in their state of ultimate division too small to be perceived by the eye, but which, by the union of a sufficient number of them, are formed either into larger visible fibres, or into plates, according to the structure of the parts in which they are situated.

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[1] Blumenbach's Physiol. § 22. Hatchett, Phil. Trans. for 1799, p. 328.

[2] Cuvier, Tab. El. p. 25; Roget's Bridgewater Treat. p. 99, 0.

[3] Recherches sur le Tissu Muqueux. [4] Physiol. § 21.

[5] Anat. Gén. par Blandin, t. i. p. 14 et seq.; see some judicious remarks by the editor in loco. See also the art. "Membrane," by Monfalcon, in Dict. *Sc. Méd. t.* xxxii.

He was at much pains to detect this fibrous structure in all the organs of the body, and to show that the membranes, however differing in their apparent texture, or whatever degree of firmness they possessed, were all resolvable into the same substance. This he calls cellular web; and although his idea of its structure may not be entirely correct, yet he made considerable advances upon the knowledge of his predecessors [1].

All the solid parts of the body, he supposes, by mechanical division or by maceration in water, may be made to assume the fibrous appearance. In its most simple state the fibre is to be regarded as a straight line, and by the approximation of these lines, in different directions with respect to each other, all the various forms are produced that enter into the composition of the animal body. He further conceives, that the greatest part of the solids, in their primary state of aggregation, compose plates with interstices between them, and that the most compact membranous body consists of this texture in a condensed state. Whether the tendons and ligaments ever actually possessed the mechanical structure of this cellular texture may be reasonably doubted; but it appears that, by proper methods, a structure somewhat resembling it may be exhibited in parts that are naturally of the densest consistence.

The uninterrupted continuity of the membranous matter, all over the body, was one of the discoveries of Haller. He employed much accurate dissection, and instituted many experiments to prove this point; but it is now so generally admitted, as to render it unnecessary to adduce any arguments in its favour. It follows indeed as the direct consequence of the view which we have taken of membrane, regarding it as the basis of the whole body, into which all the other parts are moulded, by which they are at the same time enclosed, and which serves the purpose of connecting them together into one whole [2].

The idea which was entertained by Boerhaave respecting the structure of membrane, is in itself so improbable, and seems so contrary to the evidence of the senses, that it is surprising it should have been so generally received [3]; and may serve as one among other proofs of the unlimited confidence which was formerly placed in all his opinions. He supposed that the simple fibres, or the smallest into which it is possible to conceive them to be divided, by their union, compose a membrane of the first order or series, which, when coiled up, will form a vessel of the first order or series. These vessels, by being placed in contiguity or being interwoven together, form a membrane of the second order or series, which is again coiled up into a vessel of the second order; a third series of membranes and vessels is then formed, and others in succession, until they acquire a sufficient magnitude to be visible to the naked eye. According to this hypothesis it follows, that except the earth of the bones, no part of the body is properly solid but the coats of vessels; and that, all the fibres which are cognizable by the senses, are only a congeries of vessels arranged in these ascending orders[1]. It is scarcely necessary to observe, that this hypothesis is entirely gratuitous, that there is no foundation for these regular gradations of vessels and membranes, and that the actual degree of vascularity of the different parts is infinitely varied. Both from the effect of injections and from microscopical observations, we may conclude not only that membrane generally is much less vascular than the muscular parts, but that different membranes differ very much in this respect from each other; and it even appears, that there are large portions of membrane which are without vessels of any description.

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[1] El. Physiol. lib. 1. § 2.

[2] El. Physiol. lib. i. § 1, 2; and Prim. Lin. c. 1. § 1..13.

[3] See Haller's Phys. lib. i. *§ 3, for a long list of* authors who adopted it.

For the refutation of the hypothesis of Boerhaave, we are indebted to Albinus[2], and still more to Haller; but in accomplishing this object, Haller probably went too far into the opposite extreme; for he is disposed to regard some of the ultimate parts of which the solids are composed, as unorganized[3]. This opinion he seems to have adopted partly from an idea that a vascular structure is essential to organization, and partly, because, in the division of the larger fibres into those that are more minute, we must at length arrive at a fibre which is too small to admit of any further subdivision. But this latter objection is rather metaphysical than physiological, and refers more to the fineness of our instruments, than to the actual state of the parts, while the former is an assumption without proof, and will probably be found to be incorrect.

The idea that vascularity is essential to organization, or rather, that in descending from larger to smaller parts, organization and vascularity must cease at the same time, has been generally entertained by the most eminent modern physiologists. It seems to have originated, in a great measure, from the skilful injections of Ruysch[1], who proved by his preparations, that many of the white parts of the body, as they were termed, which were thought by the ancients to be entirely without blood, possessed numerous vessels demonstrable to the eye. William Hunter, in his strictures upon the doctrine of Haller, adopts this opinion: he always speaks of organization as synonymous with vascularity, and takes it for granted, in reference to the minute structure of the body, that where there is no circulation there is no life[2]. An error of a similar kind formerly prevailed respecting the distribution of the nerves; for as sensation was admitted to be an appropriate quality of the living body, it was assumed that no living part could be without sensation; and of course, that the smallest parts into which an organized body could be conceived to be divisible, were nervous filaments[3]. But there is reason to believe, that both these notions are incorrect; and although our views must be, to a certain extent, hypothetical, when we venture to describe the structure of parts that are too small to be visible, yet we may at least form a plausible conjecture upon the subject.

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[1] Although this may appear to be the fair deduction from the expressions that are employed by Boerhaave, it must be acknowledged that we do not find the doctrine laid down by him so explicitly as might be expected, from the statement of Haller, or even of Van Sweiten. See the references to Boerhaave, given by Haller; Van Sweiten's Com. on Aph. 39; also Gorter, Med. Compend. § 1..5, and fig. 1, 5, 14..21.

[2] Acad. Annot. lib. iii. c. 1.

[3] Prim. Lin. c. 1. § 16.

We may agree with Haller in conceiving, that there is an actual solid fibre, the basis of the whole animal frame, to which the vessels are superadded as distinct appendages, but we cannot admit the existence of the inorganic concrete, which he describes as filling up the spaces between the fibres[4]. The fibre itself, although not essentially vascular, we must suppose to be a regularly organized body, composed of particles bearing a certain relation to each other, and possessed of certain specific properties. By the conjunction of these fibres, membranes of all forms are produced, and among others the vessels; but the coats of these vessels are composed of fibres that are themselves without an internal cavity, and have no kind of circulation through their substance. With respect to the question, how these ultimate fibres, which are without vascularity, can be said to possess life, I conceive it to be a dispute about words. If a certain assemblage of parts, when taken as a whole, exhibits vital functions, we may say that every individual portion of it is alive, although the imagination may form a conception of its ultimate parts, as not being possessed of any characteristic of life.

In the present state of our knowledge, it is perhaps impossible to make any estimate of the size of the ultimate fibres of membrane. The current opinion among the physiologists of the last century was in favour of its being almost inconceivably minute;

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[1] See Albinus ubi supra.

[2] Med. Obs. and Inq. v. ii. p. 27.

[3] Boerhaave, Inst. §301; Prælect. § 440; see also Bell's Anat. v. i. p. 404.

[4] Prim. Lin. c. 1. *passim.*

an opinion which was partly founded upon the microscopical observations of Leeuwenhoek and others, and partly upon the hypothetical train of reasoning of which I have already given an account. Haller speaks of it as a geometrical line, or as possessing length without any sensible thickness; and to give an idea of its minuteness, he observes, that there are animals so small, that the most powerful glasses are barely sufficient to render them visible to the eye, yet these animals contain a complicated set of organs, each of which possesses a fibrous structure[1]. It may, however, be reasonably doubted, whether the ultimate fibre be of the same size in all animals; and, upon the whole, it is rather probable that, in the human subject, it possesses a magnitude, which is more within the limits of our comprehension.

We are indebted to Fontana for a number of microscopical observations on membrane; and although it is necessary to receive such observations with great caution, in consequence of the numerous errors and deceptions to which they are liable[2], yet his remarks are so candid, and what he describes is so credible, that I am disposed to place some confidence in them. By using glasses of moderate power, he found that a compact tendon was composed of a number of flattened plates, which he calls the primitive fasciæ, and which are connected together by cellular substance of a more lax texture. By maceration, or mechanical division, these fasciæ were found to be made up of cylinders, in the form of solid threads of a spiral or waved form[3]: these, we are expressly told, are neither hollow nor vascular; homogeneous in their consistence, and of the same size in all the parts of the same animal. These primitive cylinders or tendinous threads are about the $\frac{1}{13000}$ of an inch in diameter; they form a large portion of the substance of the whole body, and, according to the opinion which was prevalent when Fontana wrote, to which we have already alluded, he calls them non-organic[1]. These observations, as far as they can be depended upon, entirely oppose the vascular hypothesis of the Boerhaavian school, and present a much more rational and intelligible idea of the construction of the ultimate parts of the animal fabric.

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[1] El. Phys. lib. i. §. 1.

[2] If in this, or in other parts of the following work, I may appear to speak in a disparaging manner of the labours of those who have devoted their time to microscopical investigations into the minute structure of the components of the body, I beg to observe, that it is done, in no degree, upon individual, but entirely upon general, considerations. An historical detail of the errors into which this instrument has led even those who have been the most skilful in its application, would have the effect of inducing us to place but little confidence in hypotheses and speculations that are derived from objects, which can only be detected by the use of high magnifiers. I may refer to the observations of Raspail, which are contained in the first chapter of his New System of Organic Chemistry, in confirmation of my remarks on the use and value of the microscope in physiological researches. Raspail's work is one which must be read with deep interest by every cultivator of physiological and chemical science, but it is greatly to be regretted, that so much valuable and original information is conveyed in a style and manner, which is not, at all times, worthy of the dignity of the subject and the talents of the writer. We have a judicious analysis of the work in the Edinburgh Medical Journal, v. xlii. p. 440 et seq.

[3] The observations of Monro would, however, lead us to suppose that this spiral or waved appearance is an optical deception; Obs. on the Nervous *System*, c. xxii. pl. 35 et seq.

We are indebted to Dr. Milne Edwards for a series of microscopical observations on the principal textures which enter into the composition of the body, and among others, the different varieties of membrane. His observations may be considered as generally confirming those of Fontana, but by employing more powerful lenses, he was able to resolve the cylinders mentioned above into rows of globules, which had the uniform size of about $\frac{1}{7500}$ of an inch in diameter. He found the same structure in all the various kinds of membrane, so as to lead to the conclusion, that these rows of globules compose the ultimate texture of the basis of the body[2].

M. Dutrochet, has also given us the result of his microscopical observations on the elementary structure of the body. He advanced still farther into what may be termed the analysis of its mechanical composition, for he conceives that the globules themselves are resolvable into other globules of much smaller dimensions. His observations principally refer to the nervous and muscular parts, but they may be fairly applied to the other textures of the body[3]. Although there appears no obvious cause of inaccuracy in M. Dutrochet's observations, yet I may remark, that in proportion as they recede from the more ordinary opinions that are entertained on these topics, so do they require a greater weight of evidence for their establishment. In some points his system agrees with that of Dr. Edwards, but in many essential circumstances it differs widely from it. M. Dutrochet aims at a much more minute development of the system of organized bodies than Dr. Edwards, and must have required a much more powerful instrument for this purpose[4]. The account which Dr. Edwards gives us of his observations, is written in so clear, plain, and unassuming a style, while the appearances which he describes are so simple and intelligible, that the reader is scarcely disposed to entertain a doubt of their accuracy. It is, however, called in question by Dr. Hodgkin, who has more lately made a series of microscopical observations on various animal substances, and among others, on the different textures that were examined by Dr. Edwards. According to Dr. Hodgkin, the globular structure, which had been so fully made out and minutely examined, is all deceptive, and we are informed, that we must revert to the fibre, as the most minute component part of the cellular membrane, which can be detected by his most powerful microscope [1].

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[1] Sur les Poisons, t. ii. p. 222 et seq.; pl. 6. fig. 1, 2, 3, 4, 5; pl. 7. fig. 1, 2.

[2] Mem. sur la Structure Elém.; Recherches Micros. sur la Struct. des Tissus, in Ann. Sc. Nat. t. ix. p. 362 et seq.

[3] Recherches Anat. et Physiol. Sect. 5.

[4] M. Dutrochet informs us, that he employed in his researches the single microscope, which, he remarks, "seul peut procurer une vision très nette et très distincte." The little experience that I have myself had on the subject, induces me to coincide in this remark, when the examination of very minute objects is concerned.

SECT. 2. *Properties of Membrane.*

The properties which more especially belong to membrane, are cohesion, flexibility, extensibility, and elasticity. It will be unnecessary to enlarge upon the importance of these properties in a system like that of the living body, in which great strength is necessary, together with lightness and a capacity for free motion, and where the parts are perpetually varying in their bulk and relative position.

Of these properties, elasticity may, in some degree, be regarded as the specific quality of membranous matter; as it appears to be the only one of the constituents of the animal body which possesses it. The muscular fibre is flexible, and is capable of being extended, but it does not appear that it is properly elastic. As we advance in our subject, we shall be more able to estimate the advantages to be derived from this property: at present I shall briefly notice, that it is an essential agent in the action both of the arteries and of the thorax, so that it is of prime importance in the functions of circulation and of respiration. In some instances it co-operates with the muscles in the motion of the joints, and it is frequently employed to restore the situation of a part that has been previously moved by muscular contraction from its natural position.

Besides the above properties, which are universally admitted to belong to membranous matter, but which it possesses in common with other natural bodies, some physiologists have ascribed to it qualities of a more specific or appropriate nature. Many of the modern French writers, as Bichat [2] and Richerand [3], have supposed that the different forms of membrane have a degree of spontaneous contractility and sensibility connected with, or inherent in them; but this opinion seems to be derived from the idea that contractility and sensibility are necessarily attached to every part of a living body, and to membrane among the rest. As far as the opinion depends upon any general principle concerning the nature of a living body, I shall think it sufficient to refer to the observations which were made above, respecting the connexion between life and vascularity. And as to the facts which have been adduced to prove this point, I conceive them to be altogether inconclusive. The alleged instances, where pure membrane is supposed to exhibit marks of contractility, are either of a very dubious kind, and so obscure as to afford no sufficient ground for any theoretical deduction, or, when they are more obvious and decisive, they appear to be easily referable to the effects of elasticity. Of this kind are the shrinking of a cavity that has been preternaturally distended, the retraction of a tense membrane when suddenly cut across, and the collapse of tubes or sacs of various kinds, upon the removal of some extraneous force that had stretched them beyond their ordinary size; but in all these cases we see nothing more than the operation of the elastic power of the membranes, modified by their situation, or by the nature of the parts connected with them.

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[1] Phil. Mag. and Ann. Phil. v. ii. p. 136, and Appendix to the trans. of Edwards, p. 466, 7. See also the remarks of Raspail, § 484 et seq.

[2] Traité des Memb. p. 54; Anat. Gén. t. i. p. 116.

[3] *Elém.* de Physiol. t. i. p. 48.

Professor Blumenbach ascribes to membrane a specific power, which he calls contractility, or vis cellulosa; but he employs the term contractility, in a peculiar sense, and expressly distinguishes it from the moving power of the muscular fibre [1]. The contractility of Blumenbach consists in the contraction which membrane is occasionally observed to exercise, when it has been over-distended, and the stretching force is withdrawn, and like the cases mentioned above, may be referred to elasticity. The principal example of it which he adduces is the action of the cellular substance in propelling the serous exhalation into the lymphatic vessels, an operation which is very obscure, and with the nature of which we are too little acquainted for us to make it the basis of an hypothesis.

Under the title of tone or tonic power, the Stahlians formerly described a peculiar property as belonging to membrane; and this term has been lately employed in the same way by Bordeu [2] and Bichat [3]. Bordeu describes the tonic power as that property of the cellular, or, as he styles it, the mucous texture, by which each of the separate cavities or cells of which it is composed acts upon all those around it, and keeps them in a state of equilibrium or of uniform distension. Bichat expressly states, that what he terms the tonic power of membranes exists in all the three species into which he divides these bodies: the mucous, the serous, and the fibrous: the instances, however, which he adduces of its effects are not very explicit. This as well as the tone of Bordeu, appears to be very similar to the vis cellulosa of Blumenbach; and it seems probable, that as far as they depend upon any one power, they may all be referred to the action of elasticity.

With respect to the sensibility of membrane, considered as a matter of fact, independent of any speculation concerning the nature of organization or vitality, the opinions of physiologists have been various; and it was especially the subject of a warm controversy, about the middle of the last century, between Haller and Whytt[1]. As is often the case in points of this nature, the question has been decided by a kind of compromise; it is generally admitted with Haller, that simple membrane is insensible in its healthy and natural state, but that it is liable to inflammation, and that it then becomes sometimes exquisitely painful[2]. The cause of this fact, the excessive degree of pain, which is excited by disease in parts that are, at other times, without sensation, is perhaps not altogether understood. We may remark concerning it, that one effect of inflammation is to enlarge the bulk of the inflamed part, and the pain is generally in proportion to the difficulty with which the part admits of this extension. A high degree of inflammation may exist in loose cellular texture, and we may be scarcely sensible of its existence, while the inflammation of the periosteum of the smallest bone, as of a tooth, of the sclerotic coat of the eye, or of the tense membrane about the finger nail, will be almost intolerable. In these cases we shall probably always find, that even if the inflamed part be without nervous filaments, which give it sensibility, still that there are some branches of nerves immediately contiguous to it, which, in consequence of the firmness of all the neighbouring parts, are pressed upon and irritated, while the blood-vessels connected with them are in a state of plethora; for it seems to be a general law of the animal œconomy, that no cause is more powerful in producing pain than a certain degree of pressure upon a nerve, while its sensibility is augmented by an unusual determination of blood.

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[1] Instit. Physiol. § 40, 59. Tiedemann also maintains, that parts which are not muscular, may possess contractility; Physiol. § 577..2.

[2] Recherches sur le Tissu *Muqueux*, *§ 70.*

[3] Traité *des Membranes, p. 62, 101, 133.*

It is probable that much error and confusion took place on the subject of the sensibility of membrane, among the anatomists and physiologists, after the revival of letters, in consequence of their blind veneration for the ancients. Hippocrates, who had but an imperfect knowledge of the existence and use of nerves, confounded them, or at least placed them in the same class, with the tendons, from some similarity in their visible structure and appearance, and having observed very serious effects to ensue from injuries of the proper nerves, he laid it down as a maxim, that tendons and other membranous parts are among the most sensible organs of the body[3]. This erroneous opinion materially influenced, not only physiological speculations, but medical and surgical practice, even as late as the middle of the last century, long after the distinction between nerves and tendons was thoroughly understood. Even Boerhaave fully subscribed to this doctrine, in which he is warmly seconded by his learned, but obsequious commentator, Van Sweiten[1]; and the influence of the old hypothesis upon our language may still be observed in the present day[2].

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[1] Haller, Mém. sur la Nature Sens. et Irrit. des Part. and Op. Min. t. i.; Whytt's Essay on Sensibility and Irritability, and Appendix. We are informed by Wilson, Lectures on the Bones, p. 44, that the doctrine of the insensibility of the periosteum and various other membranous bodies, while in their healthy state, was taught by Wm. Hunter in his lectures, as early as the year 1746, which was previous to the publications of Haller on this subject.

[2] Bichat, Anat. Gén. t. i. p. 119; Blumenbach, Physiol. § 210.

[3] Fœsii, Œcon. Hipp. "*Νευρον*." See also Pliny, Hist. Nat. lib. vii. c. 20; Le Clerc, Hist. de la Médecine, liv. iii. c. 3. § 5; and Haller, Mém. sur les Part. Sens. et Irrit. t. i. p. 19; and Opera Minora, t. i. p. 411.

John Bell, in his usual animated and impressive manner, describes the dreadful effects which this opinion, concerning the great sensibility of membrane, formerly produced in the operation of lithotomy[3]. As the bladder principally consists of membrane, it was agreed by all the learned operators, for a succession of ages, that it would be improper to cut or divide any part of it; and, therefore, in order to extract the calculus, a variety of instruments were employed for the purpose, as it was said, of dilatation, but which, in fact, caused the most cruel laceration of the organ itself and of the neighbouring parts. It is truly astonishing to observe how the weight of authority bore down the clearest dictates of reason, and the most decisive results of experience; and how the most obvious facts were warped and misconstrued, before mankind would submit to prefer the evidence of their own senses to the mere hypothetical opinions of the ancients.

From these remarks it will appear, that except bone, membrane may be regarded as the most simple in its properties of all the organized parts of the body. By this expression, I must be understood to mean, that the properties which belong to it are likewise found in many other natural objects. Cohesion necessarily belongs to all solids, while flexibility, extensibility, and elasticity are possessed by many vegetable and some mineral substances, and also by dead animal matter; whereas spontaneous contractility and sensibility are the exclusive properties of the living body. We are, however, as much unacquainted with the intimate nature and immediate cause of the properties of membrane as of contractility and sensibility, only we are much more familiar with these operations.

As I have several times made use of the term organization, and shall frequently have occasion to employ it in the subsequent parts of this work, it may be desirable to give a clear explanation of it. In its most extensive acceptation it may be regarded as nearly synonymous with the word arrangement, signifying that the parts of the organized body are placed according to some specific structure visible to the eye. Thus the serum of the blood, when coagulated and dried, in its chemical and mechanical properties, almost entirely agrees with membranous matter, yet in its texture it is obviously different. We say that the serum is not organized, because its texture is perfectly homogeneous, it is cut or broken with equal facility in every direction; whereas, in a tendon, which is organized, there is a regular distribution of the particles in a specific form, and according to a determined arrangement. The term is not so generally applied to mineral substances, yet in reality crystallization seems to be analogous to this kind of physical organization. It is doubtful whether the term can with propriety be applied to any fluid; and if, for reasons which will hereafter appear, it should be applied to the fibrin of the blood, still the greater part of the animal fluids can have no title to it. It is a question which we cannot perhaps very easily determine, whether any of the solids are not organized. I have already considered this point with respect to the ultimate fibres of membrane, but it may still be supposed to be the case with some other of the components of the body; for example, with the earth of the bones, which has been conceived to be merely deposited in cells of membranous matter, upon which its form entirely depends. This point we shall examine more fully when I come to treat upon bone; and as we advance in the subject, we shall more accurately learn what substances are organized, and what are to be considered as composed of unarranged particles, or rather, if there be any which fall under this description.

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[1] Aphorismus 164, et comment. in eund.

[2] Stuart argues with much ingenuity to prove, that tendons and nerves are the continuation of the same substance, differing merely in the mechanical arrangement or disposition of their parts; Diss. de Struct. et Mot. Mus. C. vii.

[3] Surgery, v. *ii. sect. iii.*

But besides this kind of physical organization, the word is employed by physiologists in a more restricted, but perhaps in a more correct sense, when it is applied to a system, composed of a number of individual parts, possessing each of them appropriate powers and functions, but all conducive to the existence and preservation of the whole. An animal body is thus said to be organized, or to consist of a number of organs or instruments. A vegetable, in like manner, is an organized body, consisting of separate parts, as the roots, the sap-vessels, and the leaves, each of them constituting a distinct organ or instrument for performing some appropriate action, yet all composing one connected system. It is this species of physiological organization which properly distinguishes animate from inanimate matter; and where we are able to ascertain its existence, it may be regarded as a sufficient characteristic of the presence of life.

From these observations, it will appear that membrane has a perfect organization, although one which is more simple than that of some other parts of the body, as being possessed of fewer powers, and made up of fewer component parts. Some writers, especially of the French school, seem to regard organization as necessarily connected with contractility and sensibility; and to consider those parts which are neither contractile nor sensitive, as inorganic. I have already referred to the opinion of Fontana on this point, and a similar kind of doctrine is maintained by Dumas; he speaks of the cellular substance as being slightly organized, and even calls it a kind of inorganic sponge[1]; and a similar doctrine seems almost necessarily to follow from the view which Bordeu takes of the subject.

The opinion of Cuvier respecting the nature of organization, is, on the contrary, somewhat more restricted than the one which I have adopted. He conceives it to be essential to an organized body that it be composed of both solids and fluids, the latter being the media through which its functions are performed, and the former being necessary to contain the fluids[2]. This view of the subject is probably correct, so far as respects any organized being of which we are able to ascertain the independent existence; but when we attempt to conceive of the ultimate parts of which it consists, we must at length arrive at a solid fibre, which contains no fluids, and which is, however, composed of regularly arranged particles.

With respect to the essential distinction between organized and unorganized bodies, this author points out the following circumstances: their structure, the mode in which they are originally produced, that in which they are supported, and that by which they are finally destroyed. With respect to structure, I have already remarked, that it is essential to an organized body to be composed of separate parts, which are heterogeneous and dissimilar to each other, yet which all combine together to form one whole; whereas the parts of a simple unorganized body are homogeneous, so that into whatever number of portions it is divided, still each portion may retain every property of the whole mass, and constitute a perfect existence. Its individual parts have no relation to each other except those of cohesion and physical attraction; whereas the components of an organized body possess numerous relations of a more complicated nature, each having its appropriate and specific powers, which enable them to form a whole, to the perfection of which every individual part is necessary. As to the production, support, and destruction of organized bodies, and the way in which they differ from the same operations in those that are without organization, we shall be able to enter with more advantage upon these topics, when we have made ourselves acquainted with the functions to which they are respectively subservient[3].

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[1] Principes de Physiol. t. ii. p. 5.

[2] Regne Animal, t. i. p. 14, 15.

[3] We have some valuable observations on this subject in Adelon, Physiol. t. i. sub. init.; also in the art. "Organisation," by Chaussier and Adelon, in Dict. Sc. Méd. t. xxxviii. p. 205 et seq; by Dr. Roget, Bridgewater Treatise, v. i. p. 96 et seq.; and by Mr. Quain, Elem. of Anat. p. 16. .0. Raspail, in his late work, attempts to solve the question, by reducing the structure of all organized bodies to one homogeneous arrangement; viz. a vesicle, the internal surface of which is capable of aspiration and expiration; § 126 et seq. Before we can give our assent to so important a principle, it will be necessary to have *a very powerful weight of* direct evidence, as well

Having now considered the structure and physical properties of membrane, I must proceed to give an account of its chemical composition, and the effect of chemical re-agents upon it. So very imperfect was the knowledge of animal chemistry, even as late as the time of Haller and Cullen, that they supposed all the soft parts to consist of the same substance, differing only in its mechanical arrangement. Haller had an opinion, that membrane, being the least complicated part of the body, consisted principally of the simple fibres, which served as a kind of basis to the whole system, and that the fibre itself was composed of earthly particles cemented by gluten[1]. The discoveries of the pneumatic chemists, and especially of the French, who have assiduously cultivated this department of science, proved that Haller's opinion is fallacious, and that earth is not an essential constituent of membrane. His hypothesis of the connecting gluten is equally gratuitous, and is quite contrary to the more correct notions of modern chemistry. The particles of membrane, as well as those which compose any other solid, are held together by their attraction for each other, not by any connecting medium. It appears indeed that membrane acts mechanically in uniting the different parts of the body, and in maintaining the proper form of those substances, which are of so delicate a consistence, as not to be able to preserve themselves in a compact state, for want of a greater degree of cohesion between their particles. The soft pulp of the nerves, for example, and the adipose matter, seem to be retained in their present form, merely by the membrane in which they are imbedded; but this is quite independent of the consistence or structure of the membrane itself.

Although Cullen had no direct share in the great revolution in the doctrines of chemistry which commenced about sixty years ago, yet his notions on this, as on most other topics to which he paid any attention, were much more correct than those of his predecessors. He pointed out the mistake of Haller respecting the basis of the body being formed of solid particles united by a cementing material, and maintained, on the contrary, that the simple fibre is an homogeneous compound; but, with the exception of the bones, he conceives this compound, which he calls the animal mixt, to be of the same nature in all the different parts of the body[2]. So little indeed was he acquainted with the constitution of animal matter, that he expressly says, we know nothing of it, except that it consists of some concreting substance united to water, and that the differences which it

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as the concurring testimony of various observers. We have some excellent remarks by Dr. Willis on the "comparison of the organic and the inorganic world," in the Cyclop. of Anat. art. "Animal," v. i. p. 118 et seq., and especially the summary in p. 121; and in the "comparison between animals and vegetables," p. 124 et seq. See also Tiedemann's Comp. Physiol. B. 1.

[1] El. Phys. lib. i. sect. 1.

[2] *Inst. of Med.* § 10, 13.

exhibits in various parts of the body are merely owing to the proportion which the connecting matter bears to the water. When we recollect that Cullen formed his opinions on physiology before we were made acquainted with the gaseous bodies which enter into the composition of animal matter, and that the only method then employed for its analysis was simple combustion or destructive distillation, we ought not to be surprised at the incorrect opinions which he entertained upon the subject.

The experiments of Hales, who obtained large quantities of fixed air, as it was then termed, from urinary calculi, and afterwards those of Priestley, who procured azote from the muscular fibre, by means of the nitric acid, may be considered as among the earliest which threw any light upon the real nature of animal substances; but for the first regular analysis of them, and, especially, for the first attempt to distinguish their different species, and to show the nature and proportion, both of their primary compounds and of their ultimate elements, we are principally indebted to the French. Fourcroy early devoted himself to the department of animal chemistry, and enriched it with many important discoveries; but his opinions are not to be regarded as, in all instances, entirely correct. This is the case with respect to membrane. Finding that a large, and as it seemed, an indefinite quantity of jelly could be extracted by boiling from many membranous bodies, he concluded that all bodies of this description, which he classes together under the denomination of white parts, were either identical with jelly, or might be entirely resolved into it[1]. But although jelly, in a greater or less degree, may be obtained by boiling from all these bodies, yet they are none of them entirely composed of it, and are found to differ very considerably in the proportion of it which they contain[2].

We are indebted to Mr. Hatchett for a much more correct view of the subject; from his experiments we learn, that what may be considered as the basis of membranous matter is a substance, which, in its chemical properties, is identical with the albumen of the egg, when in a state of coagulation[3]. Albumen naturally exists in the form of an adhesive fluid, miscible in water, but when subjected to a temperature of about 165°, it experiences a remarkable change in its physical properties. By the operation of heat it is converted into a solid, which is no longer capable of being dissolved in water; and if after coagulation it be gradually exposed to a higher temperature, it is reduced to a firm semi-transparent body, very similar to some of the more compact varieties of membrane.

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[1] System of Chem. Knowledge, by Nicholson, v. ix. p. 319 et alibi.

[2] A good deal of confusion has arisen on this subject, in consequence of the indeterminate manner in which the terms albumen and gelatine have been employed by physiologists. In illustration of this remark, I may refer to Broussais' Traité de Physiol. t. i. p. 10 et alibi, a work, in many respects of considerable merit, but which is defective with regard to precision in the use of technical terms.

[3] Phil. Trans. for 1800, p. 399 et alibi. For an ample account of the physiological and chemical properties of albumen, I may refer to Raspail's New System, § 429..477. He conceives that the nitrogen, which is obtained when we decompose albumen, existed in it in combination with hydrogen, under the form of *ammonia, or a combination of* ammonia with an acid.

But although albumen appears to be the essential part of membrane, that which gives it its general form and determines its peculiar texture, yet it probably always contains jelly, and in some cases, even much more copiously than the albumen itself. Jelly is very soluble in water, especially when heated; it is thus separated from the albumen, and by the evaporation of the water may be obtained in a state of purity. One of the most striking characteristics of jelly is the property which it exclusively possesses, when united to a quantity of water, of being dissolved by heat, and again becoming concreted by cold, without, as it appears, undergoing any change in its chemical constitution.

Another substance which appears to enter into the constitution of membrane, or is at least frequently found connected with it, is animal mucus. This is not properly soluble in water, nor it does not possess the property of gelatinization, and it differs from jelly in many of its chemical relations. Animal mucus appears to be nearly related to albumen; and indeed the constituent upon which its characteristic properties principally depend would seem to be a mere modification of this substance.

A considerable proportion of both the bulk and weight of membrane, as well as of all the other soft parts, consists of water, and it has been supposed by many eminent physiologists that upon the relative quantity of the water and the solid matter depend many of the morbid changes of the body, as well as the natural varieties in the constitution and temperament of different individuals. Boerhaave entered largely into these speculations; they were refined upon, with much ingenuity, by his pupil and successor Gaubius[1], and were, to a certain extent, adopted even by Cullen[2]. Membrane, when no longer forming a part of the vital system, is capable of having its properties much affected by the quantity of water with which it is combined; and there are some facts connected with pathology and the practice of medicine, which would lead us to conceive, that the elasticity, and perhaps even the density, of some of the external parts of the body may be influenced by being exposed to warmth and moisture. But there is always great difficulty in applying these mechanical explanations to the living body; and when we reflect how many mistakes have occurred on the subject, we must feel the necessity of proceeding with the greatest caution.

With respect to the ultimate chemical elements of which membrane is composed, we find that, like other animal substances, it consists essentially of oxygen, hydrogen, carbon, and azote. It has been observed that membrane is less disposed to undergo the putrefactive fermentation than any of the soft parts of the animal body, a circumstance which may probably depend upon its containing but a very small proportion of either blood or fat, substances which are peculiarly disposed to spontaneous decomposition. The more dense varieties of membrane contain less water than many other of the solids of the body, and this may be one reason why they are less disposed to putrefy; as we invariably find that moisture favours decomposition.

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[1] Instit. Pathol. § 150..168.

[2] Inst. § 13.

By the assistance of heat membranous matter is soluble in the mineral acids; its combination with the sulphuric and muriatic acids is not attended with any peculiarly interesting phenomena, but the effects which are produced by the nitric are more remarkable; the membrane is partly dissolved and partly decomposed, and several new compounds are produced, which will be more fully described when I treat expressly upon the analysis of animal substances. Membrane is also soluble in the pure fixed alkalies, and a saponaceous fluid is formed, which has been employed, in some instances, instead of the coarser kinds of soap, made in the usual process, by the combination of alkali and oil. Membrane has a strong affinity for the tannic acid; by uniting with a portion of it a dense, flexible, elastic compound is formed, little susceptible of putrefaction, which constitutes the basis of leather.

The jelly, which enters so largely into the composition of some kinds of membrane, has also a strong attraction for the tannic acid, and forms with it a substance, which appears in its chemical properties to resemble that produced by tan and the proper basis of membrane, the albumen. But it necessarily differs in its mechanical consistence, as the jelly, at least in the form in which it is usually employed, is dissolved in water, and of course without any regular organization of its particles, or even any adhesion between them. The mineral acids and the pure alkalies, when assisted by heat, readily dissolve jelly, but nothing very interesting or important results from their combination; the compound of jelly and alkali does not appear to possess that saponaceous property which exists in the compound of albumen and alkali[1].

It appears from the analysis of MM. Gay-Lussac and Thenard, and of Dr. Prout[2], that jelly, as well as albumen, contains less

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[1] Phil. Trans. for 1800, p. 379; Turner's Chem. p. 936..0.

[2] Children's Thenard, p. 357; Thenard, Traité de Chimie, iv. 204.
The following are the results of Thenard's analysis:

	Carbon.	Oxygen.	Hydrogen.	Azote.
Fibrin	53.365	19.865	7.021	19.934
Albumen	52.883	23.872	7.540	15.705
Jelly	47.881	27.207	7.914	16.988
Albumen (by Dr. Prout)	50.	26.67	7.78	15.55

It will be seen, that *the analysis of albumen*, as given by Dr. Prout, differs azote than the muscular fibre; they also found that the proportion of oxygen in jelly is greater than in either of the other two substances, and that it also contains less carbon. It is probably upon this circumstance, as well as upon its less compact mechanical constitution, that the greater tendency of jelly to pass into the acid state depends; and hence it is that jelly has been generally considered to be less completely animalized than the other soft parts of the body; for one of the characters which distinguish animal from vegetable substances is, that the former are disposed to evolve an alkali, and the latter an acid, during their spontaneous decomposition. As, however, there are some animal bodies that become acid, so there are some vegetables that become alkaline, and are found to contain azote. These may be considered as forming, on each side, the connecting link between two of the great kingdoms of nature.

An interesting experiment was performed by Mr. Hatchett, which tends to illustrate the difference between the chemical constitution of albumen and jelly; he found that if coagulated albumen be immersed for some time in diluted nitric acid, at the temperature of the atmosphere, it is gradually converted into a substance resembling jelly[1]. We may suppose that, in this case, the nitric acid parts with a portion of its oxygen to the albumen, and consequently, that jelly is to be regarded as differing from albumen in containing a greater proportion of oxygen; an opinion which is supported by the analyses of Gay-Lussac and Thenard.

The physiological relation which subsists between jelly and albumen is no less deserving of our attention. It is ascertained, that if we examine the same membranous parts at different ages, those of the young animal will be found to contain a greater proportion of jelly, and those of the older of albumen. It is on this account that the parts of young animals, such as the foot of the calf, are principally employed in the preparation of jelly as an article of diet; and every one must be acquainted with the difference between the soups formed from veal and from beef, in the greater proportion of jelly contained in the former. We thus perceive that the young animal, not only in its physical and mental powers, but even in its chemical constitution, is less completely possessed of the characteristics of the animal than when it has arrived at a more mature age. As we advance in the subject, we shall find that the same observation applies to other parts of the corporeal frame besides the membranous matter.

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considerably from that of Thenard, with respect to the relative proportion of carbon and oxygen; Turner's Chem. p. 936.

[1] Phil. Trans. for 1800, p. 385.

SECT. 3. *Account of the different Species of Membrane.*

Having described the structure, properties, and composition of membranous matter in general, I now proceed to give a more particular account of some of the different forms under which it exists. I shall confine myself at present to those species of membrane which are dispersed in various situations through the body, and are connected with all the different parts of it; as it will be more convenient to reserve for their appropriate places the account of those bodies that contain some other ingredients superadded to the membrane, which gives them their distinguishing qualities, as the bones and the muscles, and likewise those that, in consequence of their peculiar organization, serve for the purpose of some specific function, as the blood-vessels and the glands.

The first species of membranous matter that I shall notice is the cellular texture, as it is the most extensively diffused, and is that which has been conceived to form the mechanical basis of all the rest, or to constitute, as it were, the original structure, from which the others have all been produced. Many of the modern physiologists have paid attention to this substance, but Bergen would appear to be the first author, who explicitly states the fact of its general diffusion [1], while we are indebted to Haller for a number of experiments and observations, which form the correct foundation of all that we now know respecting it. According to this author, the cellular texture is found in nearly every part of the body, being contiguous to each separate organ, frequently entering into their substance, and connecting their different parts with each other. It is, in short, the substance which fills up the spaces that exist between every other part, which preserves them all in their proper situation, prevents them from unduly pressing upon each other, or interfering with their motions or functions of any kind. The description of it which was given by Haller, and which has been generally admitted by subsequent anatomists, is, that it consists of an irregular assemblage of plates that cross each other in various directions, forming a kind of net-work, and leaving a series of irregular cells [2]. Many circumstances prove that these cells communicate with each other. Thus when air or fluid of any kind is introduced into any part of this texture, it may be diffused all over the body, without employing such a degree of force as can be supposed sufficient to produce a rupture of the membrane itself. It is this inflation of the cellular texture by air that produces the disease of emphysema, where, generally in consequence of an accidental injury, a preternatural opening having been formed between the vesicles of the lungs and some part of the cellular substances contiguous to them, a portion of the air which is received in inspiration passes into this texture[1]. In some of these cases the whole body has been puffed up in the most extraordinary degree, and the patient has actually been destroyed by suffocation. The fluid of anasarca, which is deposited in the same cells, although less moveable and less penetrable than air, is also liable to pass from one part of the body to another, in obedience to the laws of gravity, from the trunk to the extremities, and again from the extremities to the trunk, according as the posture of the patient has been erect or horizontal. And, to a certain degree, this is likewise the case with collections of pus, although, from well-known circumstances attending its formation, the transmission of this substance along the cells is less general and less extensive than in the former instances.

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[1] De Membrana Cellulosa, in Haller, Disp. Anat. t. iii. p. 79 et seq.; Bergen's Essay is dated 1732.

[2] El. Phys. lib. i. sect. 2.

A valuable addition to the knowledge which Haller gave us respecting the cellular texture was made by W. Hunter, who first clearly pointed out a difference in the nature of the cavities that are contained in this substance, both in respect to the communication with each other and to the uses for which they are destined[2]. The adipose matter, which is dispersed over most parts of the body, is contained in the cellular texture; and Hunter's observations go to establish the fact, that the fat is not lodged indiscriminately in all parts of it, but that it has peculiar cells destined for its reception, which do not communicate with each other, and which are distinct from those that contain the air in emphysema or the water of anasarca. These particular cells he calls, from their contents, adipose, in opposition to the general ones, which he calls reticulated[3]. It is not pretended that any difference can be detected in the appearance or visible structure of these two kinds of cells; but the difference between them is conceived to be sufficiently proved by their situation and their contents. The parts of the body, where the fat is principally accumulated, are not the same with those which are the most subject to anasarca; and it is also observed, that fat never passes from one cell to another in the way that air and water do, but that each portion of fat always remains stationary in the same cell in which it was originally lodged. The opinion of Hunter, although it can scarcely be said to be demonstrated by any direct anatomical proof, is generally admitted to be correct, as it coincides with all our pathological and physiological observations, and is most consonant to our notions of the nature of fat, and the relation which it bears to other parts of the system[1].

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[1] This effect is distinctly noticed by Bergen, p. 85, as a deception practised by beggars and by butchers.

[2] Med. Obs. et Inq. v. ii. p. 26 et seq.

[3] The adipose texture forms one of the genera into which Raspail divides animal substances; § 408..428. His account of it deserves a careful perusal, and contains much interesting matter; but, I apprehend, that some of his opinions require further elucidation or confirmation before they can be implicitly adopted. He resolves the adipose texture into a series of vesicles, enclosing each other, for several successive gradations, until we arrive at the ultimate granulations, which are themselves vesicles, containing the proper fatty matter; § 427. pl. 7. fig. 11. Dr. Craigie's Article "Adipose Tissue," in the Cyc. of Anatomy and Physiology, may be perused with much advantage. See also some remarks on this subject in the first number of the *British* and Foreign Quarterly Review, p. 153 et seq.

An account of the structure and properties of the cellular texture was made the subject of a separate publication by Bordeu, but his description does not possess that degree of accuracy which can give much weight to his opinions. He entitles it the mucous texture, from an hypothesis which he formed of its origin. He speaks of it as consisting of fibres that are enveloped in a stratum of mucus, so as to compose a spongy texture, without any regular figures; and it may be inferred, that he regards it as almost without organization[2].

The latest writer who has described the mechanical structure of the cellular substance is Bichat; and it is the more deserving of notice, as it professes to be the result of careful observation, and differs, in some respects, from that of his predecessors. He informs us that if we accurately examine a portion of the cellular texture, we shall perceive it to be composed of extremely thin plates, with a number of fine filaments crossing them; that the eye is not able to trace any thing further than these plates and filaments; that the plates do not exhibit any appearance of a fibrous structure, or, in fact, of any specific organization, and that the filaments seem incapable of more minute subdivision. It may be inferred from his expression, that this is an account of what we are able to detect by the naked eye. The filaments he supposes to be exhalent and absorbent vessels; but this conclusion is deduced, not from any thing of a vascular structure which was discoverable in them, but simply from the circumstance, that there must be an apparatus for exhalation and absorption, somewhere connected with these cells; and unless these filaments perform this office, we are ignorant by what means it is effected. Although the plates do not possess any visible organization, yet the author argues that they are organized, upon the general principle, that every part must be so which is connected with the living body[3].

It is perhaps to be regretted, that such an accurate observer as Bichat did not employ the microscope, before he had given so decided an opinion upon the structure of these parts; for if we are to form any speculations upon the subjects of minute anatomy, there can be no doubt that, by the cautious use of this instrument, we may be enabled to discover parts that cannot otherwise be detected. The necessity for an organ may be regarded as a proof of its existence, but it is a species of reasoning which we should employ with great reserve. It seems very certain, that the cavities of the cellular texture, at least those which William Hunter calls reticular, always contain more or less of an albuminous fluid, which is, in some way, separated from the blood; and it is equally certain, that this fluid would accumulate in them, were it not removed from time to time, by an operation, which can be performed by no method with which we are acquainted, except by the action of the absorbents. We may then say, that the cavities are provided with an exhaling and absorbing apparatus, but beyond this we are not able to proceed. Blood-vessels are seen passing through the cellular texture in various directions, part of which we may presume are expended in support of the substance itself; and there are also branches of nerves found in it, but they appear rather to be crossing it, in order to be distributed to some other parts, than to be destined for the use of the texture itself.

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[1] M. Béclard has offered some additional circumstances in favour of this opinion, p. 15. See his note to Bichat, t. i. p. 66..0.

[2] Recherches sur le Tissu Muqueux.

[3] Anat. Gén. t. i. p. 106 et seq.

With respect to the form of the cavities, we have no accurate knowledge. All the writers who have described them since Haller, speak of the cells as having an irregular or indeterminate figure, and generally compare them to those of a sponge. But this illustration, although it is commonly referred to, and is mentioned even by Cuvier[1], cannot be considered as very appropriate, except so far as regards their free communication with each other. The cavities of a sponge are of a tubular form, whereas it may be presumed that those of the substance in question are rather narrow spaces with acute angles, the sides of which are flattened, and when not forcibly expanded, we may suppose to be in contact.

Some physiologists have indeed gone so far as to deny altogether the existence of any cellular arrangement, and to assert that the appearance of cavities, which is produced by injecting air or fluid into this texture, depends not upon any cavities previously existing there, but upon the substance itself being so soft, and at the same time so tenacious, as to admit of having its parts distended and forced asunder, in the same way as if we inject air or fluid into a mass of softened glue[2]. But, notwithstanding the respectability of the authors who have advanced this opinion, I think it is clearly contrary to the obvious conclusion which would be drawn by an unbiassed observer of the various states of this texture, which appear to me totally different from what would take place upon the supposition of its being a uniform mass, not to insist upon the theoretical objection of any part of the body consisting of unorganized matter[1].

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[1] Tab. Elém. p. 25. This learned naturalist, in the Dict. Scien. Nat. article "Animal," t. ii., describes the cellular texture as consisting of a number of small plates irregularly thrown together, and forming small communicating cells.

[2] See Béclard, p. 19...23.

I have anticipated the greatest part of what might be said on the properties of the cellular texture in my remarks on membrane generally. Notwithstanding the opinion of many very eminent physiologists, I am not disposed to admit of its possessing either contractility or sensibility; but it exhibits more marks of vitality, or rather, it is more intimately connected with the functions of the system at large than some other species of membranous matter. It is especially liable to be the seat of inflammation, and in consequence of the laxity of its texture, is frequently the depository of large collections of pus. The theory of inflammation is a subject that must occupy our attention in a subsequent part of the work; at present it may be remarked, that an essential and obvious feature of this process consists in those vessels which generally contain only a colourless fluid, becoming so far enlarged as to admit of the red particles of the blood, that this change occurs in the vessels of the cellular texture, and that probably the seat of it is in the exhalents of the part. It is stated that the cellular texture possesses the power of speedily renewing itself, after it had been wounded or destroyed; and that in the process of the healing of wounds, or repairing injuries of any kind, it is this part which is first formed, or which first recovers itself; and it has also been supposed that it is the cellular substance which is first produced in the development of the embryo; but this opinion is derived from no very accurate discrimination of the nature of the substance, and is to be regarded as at least very problematical.

The chemical composition of the cellular texture, like that of membrane in general, has been thought to consist, in a great measure, of jelly. I have already referred to Fourcroy's account of it, but it appears that he had not a sufficiently correct idea of the nature of jelly, or at least of the distinction between jelly and the animal substances which the most nearly resemble it. Bichat says that boiling water first hardens and afterwards dissolves the cellular texture; but this statement is made in a loose manner, and it may be doubted how far it was the result of direct experiment[2]. Nearly a similar account of the chemical composition of the cellular texture is given by Cuvier[3]; but like that of Bichat, it is stated incidentally, and is to be considered rather as a commonly received opinion, than as one explicitly adopted by this learned naturalist.

The next species of membranous matter which I shall notice are the bodies that are especially denominated membranes, to which the generic term was originally applied. They consist of thin semi-transparent sheets or plates, which generally form the coats or coverings of some other parts, and which differ from the cellular texture in the greater continuity of their structure. Haller in conformity with his general doctrine on this subject, supposed that the proper membranes, like all the other white parts, consist merely of condensed cellular substance: and he was at much pains to prove this point, by examining them after they had been macerated in water for a long time, and were approaching to a state of decomposition[1]. To a certain extent this idea is well-founded, but, as a matter of fact, independent of any speculation on their origin or the mode of their formation, it does not appear that the whole substance of membranes can be resolved into this texture; for although there may be a considerable quantity of it attached to, or connected with them, still there appears to be a solid basis, which is no longer capable of further subdivision into more minute plates. The proper membranes form the subject of a very elaborate publication by Bichat, in which he has arranged them into different classes, has minutely examined the structure and functions of each, and pointed out their connexion with the other parts of the system. His work contains many interesting details, and deserves to be carefully studied; yet I think that his distinctions are sometimes too refined, and that his opinions are occasionally rather ingenious than just. But there is a real foundation for the division which he makes of membranes into the three kinds of mucous, serous, and fibrous; of these I shall give some account in succession.

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[1] We have a judicious summary of the properties of the cellular tissue by Dr. Willis, in the Cyc. of Anat. v. i. p. 126.

[2] Anat. Gén. t. i. p. 111.

[3] *Regne Anim.* t. i. p. 26.

The mucous membranes are named from the peculiar semi-fluid substance with which their surface is covered, proceeding from numerous small glands that are imbedded in them. This kind of membrane always lines those cavities which are disposed in the form of irregular passages, or canals, that open to the atmosphere, and are connected with the skin at their extremities. Of these, the principal are the mouth, the nostrils, the œsophagus, and the intestines, composing the great system of the digestive organs, and those connected with the urinary organs, and the uterine system. These membranes are attached to the parts which they cover by a smooth and dense surface, while their external surface is soft and pulpy, and generally irregular from numerous projections of various kinds, which differ according to the uses for which the part is destined. According to Bichat they are divisible into distinct layers[2]; but this seems scarcely compatible with the account which he gives of their organization, and with the idea which is commonly entertained of their texture; so that it is probable that the layers which have been supposed to be detached from these membranes were merely portions of condensed cellular substance, by which they were connected to the subjacent parts. The mucous membranes are the immediate seat of some important functions: in the mouth and nose, they constitute the organs of taste and smell; in the stomach, of digestion; and in the intestines, of the assimilation of the food, and the separation of the nutritive from the fæcal part. On this account they differ from most membranous bodies, in being plentifully supplied with blood-vessels and nerves, as well as in possessing an extensive apparatus of glands and absorbents. As I shall have occasion to enter more particularly upon these points, when I treat upon the different functions that are connected with the mucous membranes, I shall at present only remark concerning them, that, unlike most of the membranous bodies, they exhibit, in a high degree, the powers of vitality, and are intimately connected with all the general actions of the system [1].

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[1] *El. Phys.* lib. i. sect. 3.

[2] Traité des Mem. art. ii. § 3.

The second species of membranes, the serous, differ materially from the mucous in their seat, their texture, and their properties. They are always found in close cavities that do not communicate with the atmosphere, as those of the thorax and the abdomen; they form coats for most of the individual organs which are essential to the animal œconomy, as the heart, the lungs, and the different abdominal viscera, and they frequently afford an external covering for those parts which are lined internally with a mucous membrane. The serous membranes in their texture are dense, smooth, and compact, comparatively thin, but of considerable strength in proportion to their bulk, and are not divisible into any regular layers; they have their surface always moistened with a fluid which exhales from them, as is supposed, in the gaseous state. No glandular apparatus has been detected in them, and on this account the fluid has been ascribed rather to a kind of infiltration through small pores, than to what can properly be called secretion. Although in some states of disease the exhaled fluid accumulates in cavities that are lined with serous membranes, forming different species of dropsies, in health it is always removed as fast as it is generated, proving that the absorption exactly keeps pace with the exhalation. We have, therefore, a clear manifestation of the effect of these two processes; and yet we are not able to detect the apparatus by which they are carried on, a circumstance which, I conceive, is unfavourable to the opinion of Bichat, that the filaments of the cellular texture are a system of exhalent and absorbent vessels. The specific use of the serous membranes, as consisting of a smooth surface, which is always lubricated by an albuminous fluid, is to give a capacity for the free motion of the parts which they enclose upon each other, at the same time that they are prevented from adhering together, to which, from their frequently being in close contact, they would be liable, without the intervention of this fluid. The serous membranes have scarcely any vessels of sufficient size to convey red blood, and have very few, if any, nerves; they are therefore without sensibility, and exhibit only in a low degree the general powers of vitality. They possess a considerable share of elasticity and expansibility, but are not properly contractile, nor do they manifest any properties except those which are common to every part of the body.

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[1] Alison's Physiol. p. 101 et seq., contains a good account of the different varieties of membranes. See also Craigie's Elem. of Anat. Ch. 16, 7; and Quain's Anat. p. 46 et seq.

The third class of membranes, the fibrous, are named from their obvious texture, as consisting of a visible assemblage of fibres, united into a continuous extended surface. They differ from both the former kinds in not being moistened by any fluid, but in their general aspect they are more similar to the serous, being dense, thin, and smooth, although, according to their situation, and the uses which they serve, they are more varied in their form and consistence. Among the most important of the fibrous membranes are the periosteum, which surrounds the bones; the dura mater, which lines the skull; the aponeuroses, those membranous expansions which surround certain muscles and separate them from each other; the capsules of the joints, and the sheaths of the tendons. The structure of these bodies is distinctly and obviously fibrous, and they seem to possess a texture which is more dissimilar to the cellular than any which we have yet examined. The fibres, as far as we are able to separate them by maceration or by dissection, appear to be strong dense cords, without blood-vessels, nerves, glands, or specific apparatus of any kind, and of course they are possessed of no properties but those which belong to every part of the body. Their use in the animal œconomy is principally mechanical; to enclose soft substances and preserve them in their proper form, to separate them from each other, and to keep them in their relative position.

The chemical composition of the proper membranes is similar to that of the membranous matter generally; they consist of a basis of albumen, united to different portions of jelly and mucus. It is stated, that in proportion to the density of their texture, is the quantity of albuminous basis compared to that of the other ingredients. The fibrous membranes consequently contain little else but albumen, while the mucous, at least some of the more delicate of them, are nearly soluble in water. The membranes contain no earth, and only a very small quantity of any saline matter; in an experiment of Mr. Hatchett's, 250 grs. of bladder left a residuum of no more than ·02 gr.[1]

The chemical composition of these bodies, as is the case with membranous substances generally, differs according to the age of the subject from which they are procured; in young animals they contain more jelly, while, as age advances, the proportion of albumen is increased.

Tendons and ligaments are bodies that nearly resemble the fibrous membranes in their minute texture and their chemical composition. It was upon tendons that Fontana's observations were made; and even with the naked eye we can easily observe their structure, as consisting of longitudinal fibres, lying parallel to each other, and closely united together. It is generally admitted that no nerves are sent to them, that they possess very few if any blood-vessels, and no organs have been detected in them for the purpose either of secretion or absorption. Their principal use is to connect the muscles with the bones, and to serve as cords or ropes to transmit the action of the muscles to a distant point, and in doing this, their operation appears to be entirely mechanical. The ligaments, in their texture, nearly resemble tendons; they are like them, compact, strong, and flexible bodies, but they are generally more dense in their consistence, and their fibrous texture is, in most cases, less distinctly marked. They have no nerves, but they have a few blood-vessels distributed to them, and they appear to possess somewhat more connexion with the vital powers of the system. Their use is sufficiently expressed by their name; they are principally employed in connecting the bones with each other, particularly about the articulations. In their chemical composition, tendons and ligaments nearly resemble the more compact membranes; their basis appears to be coagulated albumen, united to different proportions of jelly and mucus; they contain no earth, and only a minute quantity of saline matter.

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[1] Phil. Trans. for 1799, p. 333.

Cartilages are bodies which, in many respects, nearly resemble ligaments, although they differ from them in some important particulars. It is not easy to perceive any fibrous texture in them; on the contrary, their obvious appearance is that of an uniformly dense, membranous matter, not extensible, but highly elastic. Their use is to cover the ends of the bones, especially about the joints, where, for the purpose of motion, a smooth and firm surface is required; and in many parts they supply the place of bone, where strength is necessary, together with a degree of flexibility, as about the thorax, the trachea, and œsophagus. They are described by the most correct anatomists as being without visible vessels or nerves[1]. They appear to consist principally of albumen, with little, if any jelly and mucus; it is said that a portion of earthy matter is always found in them, which Dr. Davy estimates at $\frac{1}{200}$ of their weight[2], but Mr. Hatchett does not consider it as essential to their constitution[3]. Cartilages appear to hold a kind of intermediate place between membrane and bone, a circumstance to which I shall have occasion to recur when I come to treat upon this latter body.

I have stated that the fibrous membranes, the tendons, and the cartilages, possess neither blood-vessels nor nerves; that they are not furnished with any organs that we can detect, for the purpose either of secretion or absorption; and that they do not exhibit any of the appropriate powers of vitality, being neither contractile nor sensitive; it may then be asked, how are they connected with the vital system, or in what sense are we to regard them as possessing life? The question, I acknowledge, is one that cannot be easily answered, and which may perhaps be thought to turn rather upon the definition which we give of certain words and expressions, than upon any absolute facts that we can adduce. I have already mentioned, that some eminent physiologists conceive life to be always necessarily connected with vascularity and with sensibility; and these writers do not hesitate to call the dense membranes and the tendons dead or inanimate, seeming to regard them as only mechanically attached to the more vital parts[1]. But to this opinion it may be objected, that there is no portion of the body, which, as far as we can judge, has not a regularly organized structure, and that this can only be produced by the operation of some vascular action, probably analogous to secretion, by which the matter that composes them may be deposited, particle by particle, in its proper situation. We shall also learn, as we advance in the subject, that every part of the body is liable to decomposition, and is probably removed in process of time, particle by particle, in a manner precisely the reverse of that in which it was formed, and that this process can only be effected by the absorbent system. Although therefore we are not able to detect either a secretory or an absorbent apparatus in these parts, yet we have sufficient proof of their existence, and they must be considered as constituting the medium through which their vitality is preserved, and which connects them with the other parts of the living system.

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[1] Haller, El. Phys. xxix. 4. 27. Soemmering de Corp. Hum. Fab. t. i. § 27.

[2] Monro's Outlines, v. i. *p. 68.* [3] *Phil.* Trans. for 1799, p. 331.

Some writers, especially of the French and German schools, have indeed attempted to explain how this addition and subtraction of matter may be produced without the intervention of vascular action, and have not hesitated to compare it to the effect of mere attraction, similar to what occurs in the formation of crystals. Linnæus, who was too fond of fanciful analogies between things that had little real resemblance, has, in some degree, contributed to this opinion by his well-known aphorism, which he employed to distinguish between the three kingdoms of nature. He says that minerals grow, vegetables grow and live, animals grow, live, and move[2]. But between the growth of minerals and that of animals there is this essential difference, that crystallization consists merely in the apposition of particle to particle, by which the substance has its bulk increased, but without those parts that are already formed having their arrangement, in any way, altered or changed. The consequence of which is, that if a crystal be broken into a number of parts, each part forms as perfect a crystal as the larger one; and, on the contrary, if a small crystal be placed in a saline solution, where it may receive additional matter, the small crystal will differ from the larger one simply in its bulk. But this is not the case with a membrane or a tendon. If we compare the corresponding parts in a young animal and in one that is fully grown, we shall find that the latter is not merely increased in size, or has received an addition of new matter at its external surface, but that every individual part of it is of a different size, and is differently connected with the adjoining parts from what it was originally; so that it is demonstrable to the eye, that the small tendon could never have acquired the size of the large one, until the former had had all its particles gradually removed, and new particles deposited in their place. This subject will be considered more fully when I come to treat upon absorption; it is noticed at present for the purpose of explaining the difference between animal growth and the mere increase of bulk which constitutes crystallization[1]. The proper answer to the question proposed seems therefore to be, that the life of these parts consists in their being under the influence of those actions by which the growth of the body is effected and its organization preserved.

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[1] Carlisle, in Phil. Trans. for 1805, p. 12 et seq.

[2] Philos. Botan. Introd. p. i.

All animals, except those of the simplest structure, possess an outward covering, which connects their parts together, protects them from injury, and prevents the too powerful impression of the external agents to which they are exposed. In the human species, and in those classes of animals that are the most nearly allied to it in their structure and organization, this part is called the skin; and I have classed it among the membranous bodies, because, although it possesses some characters peculiar to itself, it agrees with the membranes in many of its properties, both anatomical, physiological, and chemical[2]. The skin has been divided by anatomists into distinct layers, or rather into distinct organs, which possess peculiar structures and functions; the principal of which are the epidermis or cuticle; the rete mucosum; and the cutis, or true skin[3].

Of these the epidermis is the external covering. It is a thin semi-transparent body, adhering uniformly to the parts on which it is laid, and closely applied to all their inequalities. It does not possess any blood-vessels or nerves that can be detected, it exhibits no marks of sensibility, and seems to have but little connexion with the vital powers of the system. It is frequently destroyed from various accidents, and is quickly reproduced, without causing any material derangement, or any sensible change in the functions of the subjacent parts. With respect to its minute structure, we are informed, that it consists of a thin expansion, in which no specific texture of any kind can be perceived; for the laminated or scaly appearance, which was thought by Leeuwenhoek[1], and some of the older writers, to be natural to it, appears to be the effect either of disease, or of mechanical violence. In some parts, indeed, where it is thicker than ordinary, it is capable of an imperfect division into layers; but these do not seem to possess any very distinct line of separation, and are irregular and not well defined[2]. Some physiologists have considered it as a substance merely spread over the surface, like a crust or film, and supposed it to be formed by exudation from the cutaneous vessels[3], while both Bichat and Cuvier seem inclined to regard it as without any regular arrangement of its parts, and possessed of no visible organization[4].

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[1] This point is very clearly stated by Adelon, Physiol. t. i. p. 20 et seq.

[2] See Bonn de Cont. Mem., who perhaps carries the analogy between the skin and the other membranes too far.

[3] On this subject I may refer to Gualtier's Recherches on the organization of the human skin, in Journ. Physique, t. lxx. p. 214 et seq.; also Cloquet, Anat. t. iii. p. 329 et seq., pl. 117..8, and Manuel, pl. 130..3, for the integuments and parts connected with them. Breschet has lately investigated this subject with much apparent minuteness; Recherches sur le Peau, and Ann. Sc. Nat. t. ii. (2^e. ser.) p. 167 et seq. The treatise of Rayer, as translated by Dr. Willis, contains a most comprehensive account of the physiology *and pathology of the skin.*

As the cutaneous perspiration issues from the greatest part of the surface of the body, it follows that the epidermis must be furnished with pores or passages of some kind for its transmission; yet, with the exception of Bichat[5], anatomists have confessed themselves unable to detect these passages. Indeed, one of the most remarkable properties of this part is its power of retaining fluids of all kinds, and preventing their escape from the surface. It is well known that it retains, for some time, the matter that is discharged from the cutis by a blister; and those who are conversant with dissections must have observed how much less rapidly the surface dries up when it is not deprived of its cuticle[6]. Various explanations of this fact have been proposed; Winslow and Bonn suppose that when the epidermis is detached from the cutis, a portion of the latter adheres to the former, which mechanically closes up the pores; Albinus and Meckel, that it transudes through the substance of the cuticle; Bichat conceives that the pores pass through in an oblique direction, and, consequently, that their sides are pressed together when the body is distended; while the observations of

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[1] Arcana Naturæ, p. 205.

[2] Bichat, Anat. Gén. t. ii. p. 749 et seq.; Gordon's Anat. p. 327.

[3] Winslow, Anat. sect. 7. art. 2. § 140; Haller, El. Phys. xii. 1. 12; Meckel in Cruikshank on Ins. Persp. p. 10, 27.

[4] Anat. Gén. t. ii. p. 752, 757; Tab. Elém. p. 55; see also Lawrence's Lect. p. 274; Gordon more decidedly states it to be "truly inorganized or non-vascular;" Anat. p. 239. Dr. Alison expressly states, that both the cuticle and the rete mucosum, are "extra-vascular and inorganic," Physiol. p. 120.

[5] Ubi supra, p. 746; see also Winslow ubi supra, 147..9; and Bonn. § 6.

[6] *Chevalier's Lectures*, p. 116.

Cruikshank[1] would lead us to conclude that the epidermis is possessed of a kind of elasticity which tends to close the pores, unless they are forcibly kept open by the passage of some fluid through them. Perhaps none of these suggestions entirely removes the difficulty; we may, however, go so far as to remark, that the matter of perspiration, being discharged in the form of vapour, is enabled to pass through very minute pores; and that the epidermis, when removed from the part to which it is attached, will shrink, and thus close up any openings which it possessed while in its natural situation[2].

There has been as much difficulty respecting the vessels that secrete or convey the matter of perspiration as the openings by which it is discharged. Wm. Hunter conceived that he was able to detect them merely by separating the epidermis from the cutis, when they might be seen passing from one to the other like the fine threads of a spider's web[3], but this idea is not countenanced by the observations of subsequent anatomists[4]. Bichat, indeed, speaks of the exhalents that pass from the cutis to the epidermis as being sufficiently visible[5]; on this point, however, as well as respecting the pores of the epidermis, it is difficult to reconcile his descriptions with those of other observers of acknowledged accuracy. We are, therefore, still left in doubt, both respecting the organization of the epidermis, and its connexion with the other parts of the system: yet there are many facts which show that this connexion exists. The facility with which the epidermis is reproduced, when it has been destroyed, is alone a sufficient proof of this point, for the reproduction can only take place in consequence of a regular deposition of particles from vessels appropriated to the purpose. And its own structure, when considered as a whole, exhibits evident marks of what may be termed organization; for although it is difficult to see any thing of this kind, when we examine only small portions of it, yet we observe that there are particular parts of the body where the epidermis is always thick, and other parts where it is always thin, and this obviously connected with the uses of these parts[6]. It is also liable to a visible change in its structure from various morbid causes, so as necessarily to imply a connexion with the vascular system; from which we may infer that this is the case at other times, although the minuteness of the parts prevents us from discovering it. The analogy of the inferior animals leads us to the same view of the subject, for the scales of fish, the thick folds with which the elephant is covered, and other similar substances, are properly productions of the epidermis, or are analogous to what we consider as the epidermis in the human subject[1]. As far as its chemical composition has been examined, it seems to consist almost entirely of albumen.

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[1] On Insens. Perspir. p. 13 et seq.

[2] Mr. Chevalier's recent publication contains an account of some observations which he has made upon this part, which lead us to a new, and, as it appears, a more probable account of its functions than any which had been previously proposed. He conceived it to be composed of "an infinite number of small velamina regularly arranged," so as to form "a bibulous and exquisitely hygrometrical covering;" Lectures on the general Structure of the Human Body, p. 133. pl. 2. fig. 1..6.

[3] Med. Obs. and Inq. v. ii. p. 52. pl. 1. fig. 1, 2.

[4] Gordon, p. 239; See Ruysch. Adv. Anat. Dec. 3. § 8; and the Strictures of Albinus, Acad. Annot. lib. vii. cap. 3.

[5] Anat. Gén. t. iv. p. 746.

[6] Albinus, Acad. *Annot. lib. i. p. 27; Winslow,* ubi supra, 143.

There is a circumstance with respect to the epidermis which deserves to be noticed; its property of being thickened by pressure. I have already remarked, that it is naturally thicker in certain parts of the body, as in the soles of the feet, and the palms of the hands; and we find that, in these situations, the natural thickness is increased by exercise, especially if long continued, and not too considerable. The final cause of this change is very obvious; and it affords one instance, among many others, of that admirable adaptation of the organs to their appropriate uses, by which they are not only fitted for performing certain actions, but are endued with the power of accommodating themselves to incidental circumstances. But the physical cause of this change of structure is not so easy to comprehend, and we are scarcely in possession of any facts which can enable us to explain satisfactorily the mode in which it is produced. It may, perhaps, be referred to an increased action that is excited in the secretory vessels of the cutis, analogous to some other operations of the body, where an increase of vascular action in a part promotes its natural functions; but this explanation is not without its difficulties, and is, at least, entirely conjectural[2].

The nature of the next layer of the integuments, which lies under the epidermis, has been much controverted. Its existence was first announced by Malpighi, who described it as a stratum of soft matter, disposed in the form of fibres, crossing each other in various directions, which was situated between the epidermis and the cutis[3]. Some of the modern anatomists have conceived it to be merely a thin layer of pulpy matter, without any distinct reticulated structure[4]; while Bichat, whose acuteness always entitles his opinions to great attention, altogether doubts its existence as a proper membrane, and supposes that what Malpighi saw and described is nothing more than a net-work of extremely delicate vessels, which, after having passed through the cutis, ramify on the surface in all directions[1]. There are, however, high authorities in favour of the original opinion of Malpighi, some, at least, of which appear to be derived from original observation[2]. Cruikshank, who examined the skin with great accuracy, speaks of the rete mucosum as a substance, of the existence of which he entertained no doubt, and which might be easily detected in all individuals, and even in some of the internal parts of the body[3]. We have a still more recent account of it by Dr. Gordon, who, after controverting the opinion of Bichat, informs us that it was easy to demonstrate the existence of a distinct membrane, between the cutis and epidermis, in the negro, but that it was not to be found in the European[4]. It is difficult to decide between such high authorities; the evidence in favour of the existence of this body seems, however, so strong as scarcely to allow us to doubt upon the point; but we may, at the same time, coincide so far with Bichat as to suppose that the reticulated texture, which Malpighi described, consists rather of a network of vessels ramifying on the surface of the cutis, than forming a part of the corpus mucosum itself[5].

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[1] Blumenbach's Phys. § 177, note.

[2] This supposition would accord with Bonn's idea of the formation of the epidermis, as composed by the continuation of the cutaneous vessels, united together by the fluid which exudes from them, § 7.

[3] Opera Post. p. 28 et alibi.

[4] *Blumenbach*, de Gen. Hum. Var. § 42; et Physiol. § 180.

Malpighi announced this body as being the part from which the colour of the skin proceeds; and whatever opinion we may entertain respecting its structure or its nature, it seems to be generally admitted, that neither the epidermis nor the cutis are the proper seat of colour, but that this depends upon something which is situated between them. In the Negro it is black, in the Chinese it is yellow, in the aboriginal American of a copper colour, while in the European it possesses different shades of red and olive, more or less approaching to whiteness[6]. These different shades of the skin afford a presumption in favour of the existence of the corpus mucosum, or of something corresponding to it: neither the epidermis nor the cutis of the Negro, when separately examined, are black; nor does it appear that there is any difference in the colour of the blood, so that their complexion would seem necessarily to depend upon something not contained in the vessels, and distinct from the other integuments. Besides the general question respecting the existence of the corpus mucosum, it has been asked, what is the exact nature of the colouring matter? Is it inherent in the substance, or is it something superadded to it? It has been asserted that the colouring matter may be dissolved or suspended in water, and it has been compared to the pigmentum nigrum of the eye, or by others, to the oily matter which gives the peculiar colour to the hair; but it is premature to form conjectures about the nature of a substance the existence of which is still doubtful[1].

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[1] Anat. Gén. t. ii. p. 665.

[2] Albinus, Acad. Annot. lib. 1. c. 1, passim; Ruysch, Advers. Anat. Dec. 3. § 8.

[3] On Insens. Pers. p. 3 et seq., p. 22, 36, et alibi.

[4] Anat. p. 242; see also Lawrence's Lect. p. 276; and Béclard, add. à Bichat, p. 272; see also his Elém. d'Anat. l. iii. sect. 3. p. 255..7. I may remark, that a body analogous to the corpus mucosum forms one of the six parts into which Breschet divides the skin; see also Quain's Anat. p. 55, 6, who admits its existence, but supposes it not to be organized.

[5] For a perspicuous account of the opinions of the modern anatomists respecting this organ, I shall refer to a valuable paper in the Ed. Med. Journ. v. xviii. p. 247.

[6] Blumenbach, *de Gen. Hum. Var.* *Physiol.* § 181.

The dark colour of the skin in the inhabitants of the torrid zone has been popularly ascribed to the influence of the sun upon the surface of the body[2], but the tinge produced on the skin, by exposure to a bright light appears to have no connexion with the permanent colour of the negro. The blackest complexions are not found in the hottest regions, and there are some considerable tribes, nearly under the equator, whose skin is whiter than that of many Europeans[3]. Besides, the brownness produced by the sun is not transmitted from parents to their offspring; whereas the children of negroes are equally black in whatever climate they are born, and their complexion is not altered by any number of generations; while we find, on the contrary, that after three or four successive stages, the original colour, whether white or black, is almost entirely obliterated by the union of parents from different varieties. It has not been ascertained upon what part of the integuments the sun acts, whether upon the epidermis, the corpus mucosum, or the cutis; but it is probably upon the epidermis, because we are informed that the tan of the skin may be removed by blisters[4].

As connected with the account of the corpus mucosum, it will be proper to notice a singular variety, which occasionally occurs, where the skin is entirely without colour. In the complexion of the fairest European female, there is always a mixture of red or brown, but in these individuals, who from their appearance have obtained the name of Albinos, the skin is of a dead pearly whiteness. In almost all persons there is a correspondence between the shade of the skin and that of the hair and eyes, and this is found to be the case in the Albino, for the hair is perfectly white, and the eye is without that substance which gives the various colours to the iris. From the relations of travellers, it may be supposed that Albinos are more frequent in some parts of the world than in others, and especially among the Africans and Indians, but they are not very uncommon in all the temperate countries of Europe. This peculiarity appears in both the sexes, and has a tendency to become hereditary, but its origin is entirely unknown[1].

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[1] It would appear to be this part that is the immediate seat of the peculiar colour which is produced by the internal use of nitrate of silver, as I have observed that where, in consequence of scars, the epidermis and cutis adhere together, the surface has not acquired the tinge. In a patient in Guy's Hospital, who had been repeatedly scarified, and had the skin blackened by the nitrate, the marks of the scarificator exhibited the appearance of white lines.

[2] Buffon's Nat. Hist. by Wood, v. iii. p. 405.

[3] Haller, El. Phys. xii. 1, 14. Blumenbach's Phys. Elliotson's note, p. 412. Lawrence's Lect. p. 291. Art. "Complexion," in Brewster's Encyc. by Stevenson.

[4] This point has been lately made the subject of experiment by Dr. Davy; his results lead to the conclusion, that the action is not upon the epidermis; and as he doubts the existence of the corpus mucosum, he refers it to the cutis; in some cases a species of inflammation is excited, but not in all instances; *Ed. Med.* Chir. Tr. v. iii. p. 256 et seq.

The term Albino is derived from the Portuguese, and was applied by them to individuals whom they found on the coast of Africa, that in every respect resemble the Negroes, except in their colour. The same description of persons has been also found in the isthmus of Darien, and in some of the oriental isles, and so numerously as to have induced some writers to conceive that they formed distinct tribes[2]; but for this opinion there appears to have been no foundation. In this part of the world we have sufficient proof that the Albino is an accidental variety, although, as was remarked above, with some tendency to propagate itself when it has been once produced. Besides the whiteness of the skin and the peculiar appearance of the iris, which is of a bright rose colour, the eye is so sensible to light, that the individual is scarcely able to keep it open in the sunshine, although in the shade or the dusk of the evening, the vision seems to be perfect.

Buffon, according to his usual speculative manner, attributes this peculiarity to an effort of the constitution to assume, what he calls, the primitive colour of nature, which he supposes was white, and which has been changed by various circumstances into the shades which it now exhibits[3]. Saussure has given us an accurate and interesting account of two Albinos that were born at Chamouni[4], but it is to a conjecture of Blumenbach's that we are indebted for our knowledge of the cause. He conceived that the pink colour of the eye and its delicate sensibility depend upon the absence of the pigmentum nigrum, the black mucous substance which is spread over the posterior part of the organ. The conjecture of Blumenbach was completely verified by Buzzi of Milan, who had an opportunity of dissecting the eye of an Albino, and found it to be entirely without the pigmentum nigrum. He afterwards examined the skin, and he found that the corpus mucosum was either entirely wanting, or at least that it was perfectly white, so as to escape his observation, and he naturally attributes the absence of colour in the surface to the state of this part. We may therefore conclude that the same cause operates upon the eye and the skin, that the redness of the one and the whiteness of the other depend upon the same physical defect in their organization, and that it is derived from the parent, although we are entirely ignorant of what it is in their constitution or habits which can give rise to this peculiar condition in their offspring. Albinos have been born in different climates and countries, and under circumstances that have no point of resemblance, to which we can, with any probability, refer the phenomena[1].

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[1] It would appear from the following passage in Pliny that this variety was observed among the ancients. "In Albania gigni quosdam glauca oculorum acie, a pueritie statim canos, qui noctu plusquam interdiu cernant." Nat. Hist. lib. vii. cap. 2.

[2] See Buffon, v. iii. p. 328, 344, and 419.

[3] Buffon, v. iii. p. 422.

[4] Voyages *dans les Alpes, § 1037..1043.*

Under the corpus mucosum lies the cutis, or true skin, a body of considerable thickness, tough, flexible, extensible, and elastic, of a dense texture, composed of a number of small fibres or plates, closely interwoven and firmly united together. Its external surface is compact and smooth, while the internal is more loose and irregular; it is connected to the parts below it by the cellular texture, and it passes into this substance by almost insensible degrees. Besides this, which constitutes the proper basis of the cutis, or as it has been termed, the corium, there is attached to it a very extensive system of nerves, blood-vessels, and absorbents, which are dispersed over every part of it with the greatest minuteness. The sensibility of the skin differs very much in its different parts, but in its general extent it may be considered as possessing the most acute degree of feeling of any of the structures of which the body is composed; and it is accordingly observed in surgical operations, that the most severe pain is experienced during the division of the skin. Its external surface, when examined by a microscope, is found to be rendered unequal by little eminences or projections. These, which have obtained the name of papillæ, are supposed to contain each of them the small branch of a nerve, of which they constitute the ultimate ramifications, and seem to be the immediate seat of the organ of touch, as well as of all the other sensations which reside in the surface of the body. They are the most easily detected, and are supposed to be the most numerous in those organs which have the most exquisite sensibility, whether it be that of touch generally, as in the points of the fingers, or in other organs, where there exists some sensation of a more specific kind. The blood-vessels, with which the skin is so plentifully furnished, ramify in all directions over its surface, forming innumerable plexuses and probably producing that appearance which Malpighi mistook for a reticulated membrane. These vessels render the skin one of the most vital parts of the body, subject to a variety of diseases, and intimately connected with various functions, especially with those of animal temperature, secretion, and absorption.

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[1] Blumenbach, de oculis Leucæth. Lawrence's Lect. p. 281 et seq. For some interesting observations on white varieties of animals, see Hunter on the Anim. Œcon. p. 243 et seq. For a more minute account of the Albino, I must beg to refer to an article in the Cyclopædia of Physiology, where the subject is treated much more in detail, than would have been consistent with *the elementary* nature of this work.

With respect to the minute texture of the skin, Haller describes it as being the same with that of membranes generally; he says it is composed of threads and plates, which are short, interwoven, and closely adhering together, the external part being more dense, and the interior gradually passing into the cellular texture[1]. The account which Bichat gives of it is essentially the same, except that he conceives it to be composed entirely of threads or fibres, which are interwoven together in all directions, leaving spaces between them of various forms and sizes[2]. This structure may be easily detected by maceration in water, when the tissue of fibres may be seen, with the interstices or areolæ, through which the hairs, vessels, and nerves probably pass, and by which they are supported. They do not seem, however, to pierce through the skin in a straight direction, but to pursue a winding course, so that it is very difficult to perceive the actual pores through which they have proceeded[3]. The small cells or cavities under the skin, formed by the membranous plates or bands which connect it with the parts below, are generally filled with fat, and there are also connected with certain parts of the skin, a number of sebaceous glands, which secrete an oily fluid, that is probably of a specific nature different from the fat.

The properties of the cutis must be considered under two points of view; those which are attached to it as composed of a membranous basis, which gives the skin its general form and consistence; and those which belong to the system of nerves and vessels that are connected with this basis. The properties of this basis are probably the same with those of the other kinds of membranous matter, and are altogether of a mechanical nature; it possesses cohesion, flexibility, extensibility, and elasticity in an eminent degree. In addition to these, some writers have ascribed to it contractility, and what they have called tonicity, but I conceive that this has been done without sufficient foundation, and that the facts which have been adduced, are either of a very dubious nature, or are more properly referred to the effects of elasticity.

We conclude, partly from analogy and partly from observation, that the papillæ contain the ultimate terminations of the nerves, and are the immediate seat of the sensation which resides in the skin. In all parts of the body it is found that the sensibility of the nerves resides principally, if not entirely, in their extremities, where they are either divided into extremely minute filaments, or spread out into a thin expansion. In what degree the microscopical observations that have been made upon the cutaneous nerves enable us to trace them into the papillæ, is perhaps a little doubtful; but as far as they can be depended upon, they lead to the opinion, that this is their ultimate destination. Besides the nervous filaments, the papillæ are supposed to contain, each of them, a minute branch of an artery and a corresponding vein, together with an exhalent and an absorbent; but the existence of these latter vessels appears to be derived rather from conjecture than from actual observation. It is however certain that the skin is the seat of an extensive system of exhalation and absorption, although it may be very difficult to determine the actual termination of the vessels, or the exact apparatus by which these functions are performed. With respect to the properties of the cutis, considered in its most extensive relations, we may therefore conclude that, in addition to the mechanical qualities mentioned above, it possesses those immediately dependent upon nerves and blood-vessels, but that it is without contractility, which is exclusively attached to the muscular fibre.

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[1] El. Phys. xii. 1. 2. [2] Anat. Gén. t. ii. p. 658 et seq.

[3] *Gordon's Anat. p. 232.*

Although the chemical composition of the cutis has been much attended to by the modern experimentalists, our knowledge concerning it is still imperfect. The best English systematic writers, as Aikin, Brande, Henry, Murray, Thomson, Ure, and Turner, describe it as consisting chiefly of jelly, and the same opinion appears to be generally adopted by the French chemists. Mr. Hatchett, whose researches on these subjects are peculiarly valuable, also regards the skin as being principally composed of a kind of jelly, although of a more dense consistence and less soluble nature than ordinary. Seguin, who paid particular attention to the chemical composition of the skin, in connexion with the process of tanning, enters more minutely into the subject, and supposes that it consists of two parts, which differ in their chemical, as well as in their physical properties; a texture of interlacing fibres, which form its basis; and a semifluid matter mechanically interposed between them. The fibrous part he considers to be nearly similar to the muscular fibre, and to be formed of an oxidated jelly, and the semifluid matter to be of a mucous or gelatinous nature[1]. The idea of the fibre consisting of oxidated jelly appears to be quite hypothetical, and as far as we have any light thrown upon the subject by experiment, I should be led to the opposite conclusion, that the jelly is more oxidated than the fibrous part of the skin. Upon the whole, it is probable that the fibrous part of the skin, which constitutes its proper substance or basis, is composed of albumen, like the other membranous bodies, and that it has intermixed with it a quantity of matter, of a different chemical nature, which we may suppose to be a compound of jelly and mucus.

There is a class of bodies connected with the external surface of almost all animals, which, although very various in their shape and appearance, are analogous to each other in their origin and their chemical composition. They may be divided into two varieties, the first consisting of nails, claws, hoofs, scales, &c.; the second of hairs, bristles, wool, quills, and feathers. The first may be considered as weapons of defence or protection; they are either productions of the skin, or at least they are so intimately connected with it, that it is often difficult to detect the exact line of demarcation between them. They are generally considered as more immediately attached to the epidermis; and it is observed, that, in some instances, they occupy the place of this body, lying directly upon the cutis, and not having any thing exterior to them [1]. We may frequently perceive in this class of bodies a kind of fibrous or laminated texture, although this entirely disappears in those that are the most dense, when they become nearly homogeneous. They are chiefly composed of albumen, with different proportions of jelly and mucus.

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[1] Fourcroy's System, by Nicholson, v. ix. p. 353.

Hair and feathers differ materially from the bodies just described, both in their origin and their structure; they proceed from a kind of bulb or root, which is situated below the cutis, through which they pass, and project beyond its external surface [2]. They consist essentially of an external tube and an internal pulp. In hair the tube is very delicate, and is entirely filled with the pulp; in the quill the tube is firmer, and the internal part is proportionably much smaller in quantity. Although hair seems so smooth to the touch, we are informed by Bichat [3], that it actually possesses an imbricated or bristled texture, the processes all pointing in one direction, from the root to the tip, analogous to the feather part of the quill, and that it is upon this structure that the operation of felting depends, in which the hairs are mechanically entangled together, and retained in this state by the inequalities on their surface [4].

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[1] The nails in particular are described by anatomists as being actually a production of the epidermis of the finger; and in proof of this it is stated, that, by maceration, the nail may be removed along with the epidermis; Haller, El. Phys. xii. 1. 15; Winslow, sect. vii. art. 2. par. 192; Albinus, Acad. An. lib. ii. c. 15. But these two bodies differ so much, both in their structure and in the manner in which they are connected with the contiguous parts, that I conceive it would be more proper to say, that the nail occupies the place of the epidermis, and that they adhere firmly together at their junction, than that they constitute the same organ.

[2] For an account of the mode of the growth of hair, and the connexion which it has with the neighbouring parts, see Roget's Bridgewater Treat. p. 117..9; also the art. "Poil," by Villarmé, in Dict. Sc. Méd. t. xliii, and by Ollivier, in Dict. Méd. t. xvii.

[3] Anat. Gén. t. iv. p. 787.

[4] This peculiar structure has been lately confirmed by the observations of Dr. Goring, Quart. Journ. v. i. (new ser.) p. 433, 4. I may remark, however, that they were not noticed by Leeuwenhoek, see his figures in Phil. Trans. No. 140; nor by Fontana, tab. i. fig. 1; nor do they appear in Hooke's Microg. Rest. pl. iii. fig. 2; nor are they admitted by Young, Nat.

Next to the bones, hair is said to be the most indestructible of the constituents of the body; and there are accounts of its having been found in old tombs, after all the soft parts had entirely disappeared. The hair of different individuals differs considerably in its thickness, being, as it is said, from $\frac{1}{300}$ to $\frac{1}{700}$ of an inch in diameter; and it is no less variable in its other physical properties, some kinds being much more dense and elastic than others, a circumstance which, according to Mr. Hatchett, depends upon the proportion of jelly which it contains.

We are indebted to Vauquelin for an elaborate analysis of hair, from which we learn, that it consists principally of an animal matter, united to a portion of oil, which seems to contribute to its flexibility and cohesion. Besides this, there is another substance of an oily nature, from which the specific colour of the hair is derived, and there are also small portions of iron, manganese, sulphur, and the phosphate and carbonate of lime[1]. The animal matter, which constitutes nearly the whole bulk of the hair, is conceived by Vauquelin to be a species of mucus; but Mr. Hatchett has more correctly designated it as being chiefly albumen, united to a small quantity of jelly. Vauquelin found that the colouring matter of hair is destroyed by acids; and suggests that when it has suddenly changed its colour and become white, in consequence of any great mental agitation, it is owing to the production of an acid in the system; but this idea seems very hypothetical, and I conceive it more probable that the effect depends upon the sudden stagnation of the vessels which secrete the colouring matter, while the absorbents continue to act and remove that which already exists[2]. As the colour of the hair seems to depend upon a peculiar kind of oil,

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Phil. v. ii. p. 190. I may farther remark, that I had an opportunity of viewing the hair of various kinds of animals, in the microscope of Mr. Bauer, but was unable to detect these appendages. Dr. Fleming, Phil. of Zool. v. i. p. 88, and the author of the art. "Anatomy," in Dr. Brewster's Enc. v. i. p. 842, describe the hairs as being conical, tapering from the root to the tip. Dr. Gordon, Anatomy, p. 444, conceives them to be solid, and the same idea was maintained by Hooke. See also Béclard, Anat. p. 281 et seq.; Cloquet, Man. pl. 133, fig. 6 et seq. The different opinions which microscopical observers have held on this point, which it might be supposed could have been so easily decided, afford a useful illustration of the degree of confidence which we ought to place in such observations.

[1] Ann. Chim. t. lviii. p. 41; Henry's Chem. v. ii. p. 461, 2; Turner's Chem. p. 1012.

[2] I have suggested an explanation of the fact upon the supposition that it is an actual occurrence; but although every one must have heard of numerous instances of the hair becoming suddenly grey, I do not find any cases related where it happened under the immediate observation of the narrator; and when we reflect upon the manner in which the hair grows, by protrusion from the bulb, it is certainly difficult to conceive how, when once grown, its physical properties can be changed. The existence of the disease called Plica Polonica, which might seem an analogous circumstance, is now generally disbelieved. The fact appears, however, to be admitted by Dr. Alison, and is alleged by him as proving, that the vital processes of nutrition and absorption are carried on in the substance of the hair itself; Physiol. p. 122.

and as there is often a correspondence between the colour of the hair and the skin, it has been supposed that the colouring matter of the corpus mucosum must, in like manner, be of an oily nature; the conjecture is not without plausibility, but it has not been confirmed by any direct facts or experiments.

In their natural state all these bodies are without sensation, and they possess no visible blood-vessels; but under certain circumstances, they are subject to a species of inflammation, when vessels may be detected, at least in some of them, and they become acutely sensitive. The painful sensations in this case appear to proceed, not from any nerves that are distributed to the organs themselves, but from the increased bulk of the part, as produced by the state of inflammation pressing upon and irritating some contiguous nerves, in the same manner as in the inflammation of the ligaments and tendons. An obvious use of hair, in the inferior animals, is to protect the body from external cold, but except on the head, this cannot be considered as applying to the human species, nor can we easily conceive what is its object in our œconomy; yet it is contrary to our ideas of the nature of things to suppose that what is so constantly found to exist, should not be formed for some useful purpose [1].

Among the older writers we meet with narratives, apparently well authenticated, where the hair is said to have continued to grow after death, and even to attain an extraordinary length, but, upon whatever evidence they may appear to rest, we may safely conclude that there is some fallacy or inaccuracy in the statement.

[1] The existence of hair on the surface of the human body must probably be referred to one of those general facts or laws, as they have been termed, according to which a uniformity of structure is, to a certain extent, maintained in a long series of animals. We may aid our conception of the subject by saying that an original type was formed, and that deviations from it were introduced according to the necessity of each individual case. See Dr. Roget's Bridgewater Treatise, v. i. p. 48.

CHAPTER II.

OF BONE.

In treating of bones I shall first give an account of their external form, their internal structure, and their physical properties; afterwards we shall examine their chemical composition; in the third place I shall inquire into the mode of their formation; and shall conclude with some remarks upon the nature of their connexion with the other parts of the living system.

Sect. 1. *Form and Structure of Bone.*

With the general form and appearance of bones every one is sufficiently familiar; they are hard bodies, without contractility or sensibility, very little subject to decay, and are perhaps the only substances to which the term solids strictly applies. They serve as a defence and support to the soft parts, either affording them a case, in which they are lodged and protected from injury, as in the instance of the brain and lungs; or as pillars to which the more flexible and delicate organs may be attached and kept in their relative position, as particularly takes place with respect to the muscles. The bones are also fixed points against which the muscles re-act, when they commence their contractions; they form a system of cylindrical levers, by which all the movements of the body are effected; and they likewise very essentially contribute to these movements, by the share which they have in the formation of the joints; so that, in conjunction with the muscles, they constitute the principal organs of the important function of locomotion. In man and the higher orders of animals the bones are generally speaking in the interior of the body; and even when they approach towards the surface, are always covered by muscles or membranes; but in the crustacea, the testaceous mollusca, and in certain insects, the bones compose an external case within which all the soft parts are contained.

The larger bones are nearly uniform in different individuals, but there is some irregularity among the smaller ones, so that the total number of bones in the skeleton is not always the same; in general, however, they amount to about 260[1], exhibiting every variety of figure and size, according to the structure and uses of the particular parts in which they are found.

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[1] Sœmmering, Corp. Hum. Fab. § 12. Boyer, Anatomie, t. i. p. 12. *Monro's Outlines*, v. i. p. 12.

They may be arranged into three classes, the long round bones, the broad flat ones, and the short bones approaching more or less to the square form; to the first class belong the bones of the upper and lower extremities, to the second class those of the skull, and to the third the vertebræ. These three kinds of bones, as we shall afterwards find, differ not merely in their external shape, which may be conceived to be an incidental circumstance and one of little importance, but likewise in the more essential points of the mode of their growth and their mechanical structure. They are also distinguished by the uses which they serve in the animal œconomy. The long bones are more immediately adapted for the purposes of motion, either enabling us to shift our position from place to place, constituting what is termed locomotion, or to act upon other bodies that are contiguous to us, as is especially the case with the hands and arms; the flat bones obviously serve for the protection of the soft parts; while the third class of bones are usually found in those organs where it was necessary to unite in the same part a considerable degree of strength with the capacity for free motion[1].

It would be foreign to the purpose of this work to enter upon a description of the forms or uses of the individual bones, but it may be proper to make a few observations upon the beautiful mechanism of this part of the animal fabric, and to shew how admirably each of its individual organs is adapted to its particular use. For this purpose we may take the example of the upper and lower extremities. In the human subject the arms are obviously intended, not for support, but for acting upon contiguous bodies. They are therefore so attached to the trunk as to be easily applied to them in all directions, the upper part admitting of free motion, and at the same time possessing considerable strength, while the extremity of the limb is composed of a great number of smaller bones, that have less motion upon those immediately connected with them, yet the whole assemblage constituting an apparatus which is capable of executing all the various movements that are necessary for the purposes of life, with a degree of precision and velocity that would be almost inconceivable, were we not so familiar with its operations. The arm is so placed as to be applied the most easily to the objects that are before us and nearly on the same level; the joints of the elbow and wrist are obviously fitted for the same purpose; while the structure of the hand and fingers points them out as the organs of what has been called prehension, and as being singularly adapted for examining the texture and figure of the bodies that are within our reach.

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[1] For figures and descriptions of the bones, their connexion with each other, and their relation to the soft parts, the following works may be consulted; Cheselden's Osteographia; Albinus, Tabulæ Oss. Hum.; Winslow's Anatomy, by Douglas, Sect. 1; Monro on the Bones; Bichat, Anat. Des. t. i. p. 284 et seq. and Anat. Gén., with Blandin's notes, t. iii. p. 5..144; Cuvier, Leçons d'Anat. Comp. t. i. p. 473 et seq.; Blumenbach, de Gen. Hum. Var. § 5, 10; Béclard, Élém. d'Anat. Ch. 8, contains a valuable list of references; Abernethy's Physiol. Lect. No. 3; Wilson's Lectures on the Bones; Cloquet (Jules) Anatomie de l'Homme, t. i.; Do. Manuel, pl. 1..59; Cloquet (H.) Anatomie, § 1; the same trans. by Knox, Ch. 1; Mayo's Physiol. p. 324..334; Craigie's Anat. Ch. 18. Sect. i. p. 526 et seq.; Quain's Anat. Ch. 2. p. 98 et seq.; Cumming's Ossa Humana; the art. "Os," by Monfalcon, in Dict. Sc. Méd., and "Squelette," by Cloquet, in Dict. Sc. *Nat. in loco.*

The lower extremities are equally fitted for their specific object, the support of the body and its various progressive motions. They are strong pillars so placed as to bear its weight in the most advantageous manner; the foot is so adjusted to the leg as to form a firm basis, while its smaller parts possess that degree of motion upon each other, which assists in the changes of position, without admitting of that variety of complicated actions that are observed in the hand, which, in the foot, would have been not only useless, but even injurious, as necessarily diminishing the stability of the body [1]. Without going into a more minute detail, it may be asserted, that if a skeleton was to be found of an unknown animal with extremities formed like those of man, we should be at no loss to decide concerning its general habits; that it was essentially a biped, that its body was intended to be kept in the erect position, that it was neither a flying nor an aquatic animal, but that its natural abode was the surface of the earth [2]. Nothing therefore can be more unfounded than the speculations of those metaphysical physiologists, who, on the one hand, for the purpose of assimilating the human form and functions to those of the monkey, have conceived that man was naturally a quadruped, nor of those, on the contrary, who consider the latter animals as bipeds. Technically speaking, they are quadrumanous [3], their extremities all possessing the characters which point them out as instruments of prehension.

The form and structure of the articulations are among the most interesting parts of the animal œconomy. According to the language of anatomists, every part where two bones are connected together is denominated an articulation, whether they admit of any degree of motion upon each other, or are firmly fixed together[1]; but I shall only notice in this place those articulations which are moveable, where the bones are united by ligaments, or other membranous bodies of a flexible nature, so as to be capable of changing their direction or relative position. The moveable articulations present a great variety of forms, which have received appropriate technical names, but they may generally be referred to two principal classes, the ball and socket, and the hinge. In the ball and socket joint the moveable body is furnished with a round end, which plays in a corresponding hollow in the fixed bone; while in the hinge both are furnished with processes and depressions, which are mutually adapted to each other. The hip-joint is an example of the first, and the elbow of the second species of articulation; it is obvious that the first admits of a rotatory motion in all directions, while the second is capable of being moved in two directions only.

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[1] Cuvier, Leçons, t. i. p. 473 et seq. Bichat, Anat. Descrip. t. i. p. 284 et seq. Blumenbach, de Gen. Hum. Var. § 5, 10. Mr. Abernethy's third Physiological Lecture contains many interesting observations on the bones and joints.

[2] I may here refer to the use that has been made of this mode of reasoning by Cuvier, in determining the habits and functions of extinct animals, by the inspection of a certain portion of their skeleton. See his Ossemens Fossiles, passim. Also to a detailed account of the comparative osteology of the Orang and the Chimpanzee, read by Mr. Owen to the Zoological Society, in which the author takes occasion to point out the circumstances in the mechanism of these animals which essentially differ from that of man, especially as respects the posterior extremities; Phil. Mag. v. 6. p. 457 et seq. The same thing is done by Dr. Grant in his Comp. Anat. v. i. p. 118..0.

[3] Buffon, v. x. p. 15. Cuvier, Tabl. El. p. 94. Adelon, Physiol. t. i. p. 141.

Although the general form of the articulation may be observed in the solid body of the bone itself, yet, in most cases, cartilage materially contributes to the accurate completion of these parts, and the whole extent of the articulating surface is always covered with this body. Many obvious advantages arise from this construction. The smoothness of the cartilage, as well as its elastic nature, admits of a more easy motion than could have existed if the two hard substances had been in immediate contact, while, at the same time, the parts are less liable to injury from violent concussion, than if they had possessed a more rigid texture. In order to facilitate motion, by diminishing friction, the joints are enclosed in a membranous bag, filled with a dense lubricating fluid, called synovia, which is always interposed between the moveable extremities. To complete the mechanism of these parts, they are provided with a suitable apparatus of ligaments, which serve to keep the bones in their relative situations, and to regulate the motions of the joints, so as to prevent their displacement, except under circumstances of extraordinary violence. To enter into a description of the ligaments, or indeed of any of the individual joints, would be to encroach upon the province of the anatomist; I shall only further observe on this subject, that in no part of the body is the adaptation of means to ends more apparent than in the construction of the joints and the apparatus connected with them[2].

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[1] Sabatier, Anat. t. i. p. 20. Boyer, Anat. t. i. p. 55. Bichat, Anat. Gén. t. ii. p. 174 et seq.; the subject of the articulations is treated by this anatomist in considerable detail; see also Blandin's notes to the Anat. Gén. t. iii. p. 59..81; Cloquet, Anat. Descrip. § 2. p. 210 et seq.; the same trans. by Knox, Ch. 2. p. 172 et seq.; Quain's Anat. Ch. 3. p. 215 et seq.; also the articles "Articulation," by Jourdan, in Dict. Sc. Méd. t. ii.; by Béclard, in Dict. de Méd. t. iii.; and by Dr. Todd, *in* the Cyc. Anat. v. i.

[2] An *interesting example of this kind* has been lately pointed out by Mr.

The mechanical structure of bône formed a part of the investigations of Malpighi, and he is considered as having been the first who announced that its basis consists of an animal matter, the texture of which resembles that of the cellular substance [1]. The experiments of Duhamel proved, that the animal matter, under certain circumstances, assumed a laminated appearance [2]; but we are indebted to Herissant for the important fact, that bone contains an earthy matter, and that many of its specific properties depend upon this ingredient. He distinctly states that bone is essentially composed of two substances, the one a cartilaginous basis or parenchyma, which gives the general form to the part; the other a peculiar earthy matter, which is deposited in the cartilaginous basis, and is the cause of its hardness [3]. This may be demonstrated by digesting bone in diluted muriatic acid, so as to dissolve the earthy matter without acting upon the membrane, when we procure a substance retaining its former bulk and shape, but converted into a soft, flexible, and elastic body. In this process we have removed the earth, and left the membrane; by burning the bone we may reverse the operation, for we may suffer the animal matter to be consumed, while the earth is left untouched, preserving, in a great measure, its former texture.

The general opinion among modern anatomists respecting the structure of bone, and the manner in which its membranous part is arranged, is, that like the other soft solids, it is essentially composed of fibrous laminæ or plates, which are so connected together, as to form, by their intersection, a series of cells, analogous to those of the cellular texture, in which the earth is deposited. Gagliardi conceived that the plates were held together by small processes, like nails, the form of which he minutely describes [4]; but this has not been confirmed by subsequent observations, and seems to have been a mere fanciful conjecture. Bichat has even denied the existence of the laminated structure of bone, and has endeavoured to show that all the facts and experiments, which seem to demonstrate its presence, are fallacious, and depend, either upon the peculiar

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Earle, in the structure of the spine in certain birds; Phil. Trans. for 1822, p. 276.

[1] Anat. Plant. p. 19.

[2] Mém. Acad. pour 1739, 1741, 1742, 1743, passim.

[3] Mém. Acad. pour 1758, p. 322. Nesbitt had indeed previously shown, that what he styles cretaceous matter, was an ingredient in the composition of bone; see his Human Osteology, p. 31, 2 et alibi; but his ideas on the subject were somewhat vague and indeterminate.

[4] Anat. Ossium, passim, and fig. 2. This treatise would appear to exhibit one of those remarkable cases of self-deception, which are occasionally met with, even in the palpable science of anatomy. Probably in the same light we must regard the account given by Havers of the longitudinal and transverse pores; see his Osteologia, § 35..37. Havers's description is, however, partly sanctioned by the authority of the elder Monro; Anatomy of *the Bones and Nerves*, p. 13.

mode in which the bone has been treated by the operator, or upon some other cause, which induces the laminated appearance, although the laminæ did not previously exist[1]. To a certain extent the opinion of Bichat may be correct. By a kind of loose analogy, which is so often introduced into all departments of science, the substance of bone has been described as consisting of regular concentric rings, like those that compose the trunks of trees; an analogy that was probably derived from the hypothesis of Duhamel respecting the formation of bone, which will be presently noticed. These concentric layers certainly do not exist[2], but I think it equally certain that the membrane of bone is composed of plates, very similar in their general form and disposition to those of the cellular texture, and it is probable that the earthy matter is inserted between these plates, and thus is likewise disposed to assume the laminated structure. The proof of this structure will appear when we come to consider the internal conformation of bone, and the appearances which it exhibits when partially decomposed[3].

When a bone is divided longitudinally, so as to disclose its internal structure, we observe its different parts to exhibit a variety of appearances, especially with respect to the greater or less compactness of its composition. These varieties have been reduced to two, the hard or compact, and the cancellated, reticular, or spongy. Generally speaking, there is no bone which does not exhibit both of these textures, the compact forming its external, and the spongy its internal part. The long bones consist of a hollow cylinder of compact matter, including a quantity of the spongy substance; but the proportion of the two varies much in the different parts of the same bone. The shank or body of the bone consists principally of the compact with but little of the cellular matter, while the extremities or heads of these bones are principally composed of the cellular matter, with only a thin crust of the compact substance. It has been asserted, although I do not find that the experiment has been accurately made, that equal cylinders of the same bone, taken from different parts of their length, contain the same absolute quantity of solid fibres, but differently disposed. The flat bones generally consist of an external covering of the hard substance on each of their surfaces, with a layer of the spongy matter interposed between them ; while, in the short bones, the disposition and proportion of the two kinds of texture is more irregular. In the large bones of the extremities, where the structure is seen to the most advantage, the compact substance is found to be a completely solid body scarcely exhibiting any visible arrangement, either fibrous or laminated; but as we proceed towards the inner part, we find the substance to be less and less dense, until, at length, it becomes completely cellular, forming what have been termed the cancelli. In the centre of the bone there is scarcely any of the spongy matter, and a considerable hollow space is left, which is filled up with a series of membranous cells, in which the marrow is lodged: some writers have called this the reticulated part of the bone.

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[1] Anat. Gén. t. ii. p. 155 et seq. Cheselden says, "Nor are the parts of bones disposed into visible lamellæ, stratum super stratum, as many have painted." Osteographia, Introd.

[2] The mechanical structure of the membranous part of bone has been elaborately developed by Scarpa, who has detailed a series of accurate observations on it in its various states of growth and disease, as well as by subjecting it to the action of chemical re-agents. He very satisfactorily refutes the idea of the membranous part of bone being composed of a series of regular concentric laminæ; see his essay De Penit. Struct. Oss. p. 16 et alibi. Raspail applies to the bones his hypothesis of vesicular arrangement; but I am disposed to think with less success than in most of the other textures; § 543 et seq.

[3] As we may presume that the earthy part of the bone is moulded into its appropriate form by the membrane into which it is deposited, we may judge of the structure of the latter by that of the former, which from its firmer consistence it is more easy to ascertain. Now whether we examine the bone during its formation in the fœtal state, or after it has had its membrane destroyed by the action of fire, we find the earth to assume the appearance of fibres, which, when the bone is perfected, have a tendency to a laminated arrangement. In the first number of the British and Foreign Medical Review, p. 231..3, we have some valuable observations on the structure of cartilage and bone, by Prof. Arnold, taken from the Journal of Tiedemann and Treviranus. See also the remarks of Dr. Benson, in the Cyc. of Anat. v. i. p. 432, 3.

I have been thus particular in describing the structure of the long bones in order to shew how admirably the arrangement of their parts is adapted to the purposes for which they are destined. Their extremities are the fixed points from which the muscles re-act, and where greater space was required for the insertion of the tendons; their diameter is, on this account, considerably increased, and their osseous matter is disposed in nearly an equal degree through their whole substance; while, in the middle of the bone, which is more exposed to external violence, and where nothing was wanting but mere strength, the bony plates are all consolidated together into a compact dense ring, leaving the centre nearly hollow. This form of the part, as consisting of a quantity of compact matter disposed round a central cavity, has the important effect of increasing the strength of the bone without adding to its weight. The resistance of a cylindrical body to a force applied transversely may be mathematically demonstrated to be increased in proportion to its diameter, so that the same number of fibres, placed as it were round the circumference of a circle, produce a stronger bone than if they had been all united in the centre, and the diameter of it had been proportionably diminished[1]. We accordingly find that the hollow cylindrical bones are always placed in those parts of the body where the power of resisting external force was an important object; but where, at the same time, it was very desirable not to add unnecessarily to their weight.

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[1] Monro, Anatomy of the Bones and Nerves, p. 21; Porterfield, in Ed. *Med. Essays, v. i. p.* 112.

Although the hard external part of the bone is a perfectly compact body, in which we can scarcely perceive any trace of a specific organization, yet there is reason to conclude that it is made up of fibres and plates similar to those of the spongy or cancellated part, and differing from it principally in its greater degree of condensation. When we examine a bone during the process of ossification[1], we find that those parts which afterwards become the most completely solidified are of an evident fibrous texture; and we observe the fibres to become more and more numerous as the process advances, and to adhere more and more closely together, until, at length, the substance becomes perfectly compact. And when a bone is subjected to any operation, by which its substance is decomposed, and its texture destroyed, as by calcination, by maceration in diluted acids, or by long exposure to the atmosphere, it always exhibits a laminated or fibrous appearance, and shews a tendency to separate into longitudinal portions. Besides, the transition from the compact to the spongy part of the bone is not marked by any decided limits, but they pass into each other by insensible degrees, so as to shew that there is no essential difference between them.

The direction of the fibres is found to vary, in the three kinds of bones, according to their respective forms; in the round cylindrical bones they are long, and lie parallel to each other,—in the flat bones they generally exhibit a radiated structure,—while in the short bones their direction is more irregular, depending, in each particular case, upon the figure of the bone to which they belong.

In its physical properties, bone is the most simple of any of the components of the body. Membrane, as I remarked on a former occasion, is not possessed of any properties that are peculiar to the living system, and which do not belong to many other substances; but bone, when in its most perfect state, is neither flexible, extensible, nor elastic, and, in short, has no mechanical properties but those which necessarily belong to every kind of solid matter[2].

SECT. 2. *Chemical Composition of Bone.*

The chemical nature of bone was very imperfectly understood until about fifty years ago. It had, indeed, been discovered by

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[1] Albinus, Icon. Oss. Fœtus, tab. i. fig. 1, 2.

[2] The various genera of the Cetacea, although they have so many physiological relations with the other orders of the Mammalia, differ considerably from them in the composition and structure of their bones. On this subject I may refer to the article "Cetology," by Dr. Kirby, in Brewster's Encyc., where we have many interesting observations on the comparative anatomy and physiology of these animals. The article contains a valuable list of references.

Herissant to be a compound of an animal and an earthy substance, but nothing was known respecting the exact nature of either of its ingredients. Gahn seems to have been the first who discovered that the earth was the phosphate of lime[1]; an earthy salt, which is insoluble in water, and bears a high temperature without being decomposed, so as to give to bone the property of resisting, in a remarkable degree, most of the external agents to which it is exposed, and to render it the most durable of any organized body with which we are acquainted. Accordingly the bones of animals are found in a tolerably perfect state after a lapse even of many centuries, and after having been exposed to all the revolutions to which the surface of the earth is incident[2]. Indeed, from the discoveries which have been lately made by the modern geologists, we are induced to believe that bones still remain, which have existed long before any traditionary or historical records of which we are in possession, and when the earth was peopled by animals of a different kind from any of its present inhabitants.

The nature of the animal matter of bones was still longer in being understood, and we are indebted to Mr. Hatchett for our knowledge on this subject. He found it to possess all the characters of condensed albumen, the substance which I have already mentioned as the basis of membranous matter of all descriptions. Of the different species of these bodies, it appears the most nearly to resemble cartilage; and, from the observations that have been made on the original formation of bone, it is reasonable to conclude that it is identical with this substance. Besides the solid animal matter, bones contain a quantity of jelly, which may be extracted from them by boiling[3], and we find that this jelly is much more abundant in the bones of young than of old animals. Before the experiments of Mr. Hatchett, the same erroneous opinion was entertained respecting the animal matter of bones as of membrane, that it consists entirely of jelly, an opinion which is maintained even by Bichat[1] and Cuvier[2], as well as by other eminent physiologists, whose works are of recent date. Perhaps this may be the case with some of the bones of very young animals; but, with respect to the perfect bones of the adult, it is certain that, unless the water be applied under such a degree of compression as to raise its temperature much above the ordinary boiling point, as takes place in Papin's digester, a small portion only of the bone will be dissolved.

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[1] The claim of Gahn to this discovery, which was long doubtful, is, at length, fully established by Berzelius; see Progress of Animal Chemistry, p. 76. The more accurate researches of contemporary chemists, and especially Mr. Hatchett, MM. Fourcroy and Vauquelin, and Prof. Berzelius, have discovered that the earth of bone is less simple than was previously supposed to be the case. Besides the phosphate of lime, which forms nearly 82 per cent. of the weight of the earth, it contains, according to Berzelius, the fluate and the carbonate of lime with the phosphates of magnesia and soda; Chimie, par Esslinger, t. vii. p. 469 et seq. The analysis indicates a considerable excess of lime above that necessary to saturate the acids; and the same excess, although in a little different proportion, is indicated by the experiments of Dr. Dalton; Manchester. Mem. v. iii. ser. 2d, p. 5; this, however, as he observes, may, perhaps, be owing to a quantity of carbonic acid being driven off by the calcination. For an account of the chemical composition of bone, I may farther refer to Aikins' and Ure's Dictionaries, and to the systems of Thenard, Thomson, Henry, and Turner, in loco. It is worthy of remark, that in some of the lowest classes of animals, the part which may be considered as analogous to the skeleton, consists principally of a siliceous basis; Grant's Comp. Anat. v. i. ch. 1. § 2.

[2] Clift in Phil. Trans. for 1823, p. 84.

[3] Proust in Journ. Phys. t. liii. p. 227; Berzelius on Animal Chem. p. 78; see also Ann. Phil. v. xii. p. 106, for the process employed at Geneva for procuring jelly from bone; but, as is remarked by the editor, it is the sub-

Besides the marrow which occupies the central cavities of some of the larger bones, the pores and cancelli of the bone itself contain a kind of oily matter, which has been thought to differ from marrow merely in possessing a greater degree of fluidity[3]. The marrow is said to be lodged in a series of membranous cells which, like those in which the fat is deposited, do not communicate with each other; while, from the observations that have been lately made by Mr. Howship, it seems probable that what has been called the oil of bones is deposited in longitudinal canals that pass through the solid substance of the bone through which its vessels are transmitted[4].

Many conjectures have been formed concerning the use of the marrow and the oil of bones, but they all seem to be unsatisfactory. The general opinion of physiologists about the time of Boerhaave and Haller was, that the oil served to render the bones less brittle, and that the marrow was deposited in the centre to be carried into the body of the bone, and diffused through its substance, as it was required for this purpose. Even the most approved of the moderns, as Sabatier and Boyer, seem still to attach some importance to this hypothesis, for it is stated by them, although, perhaps, with less confidence than by their predecessors[5]. As to the oil, it does not appear that it could have the effect which has been assigned to it under any circumstances, and it is still less probable when considered in its actual relation to the bones, because it appears that the oil which is found in them is rather lodged in separate cavities than mixed up with the earthy matter, or diffused through the substance of the bone generally. But after discarding the old hypothesis, we have little that is more satisfactory to offer in its room; the only plausible conjecture that I can form is, that the

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stance alone to which the term "gelée" is applied, that we are to consider as jelly.

[1] Anat. Gén. t. ii. p. 160 et seq.

[2] Tab. Elém. p. 32; Leçons, t. i. p. 103.

[3] Boyer, Anat. t. i. p. 38.

[4] Med. Chir. Trans. v. vii. p. 393 et seq.

[5] Sabatier, *Anat. t. i. p. 16; Boyer, Anat. t.* i. p. 40.

marrow and the oil of bones serve the same purposes in the animal œconomy with the other oily secretions; and, as it was desirable for the bones to be either hollow, or filled with a substance which should not add much to their weight, advantage was taken of this circumstance to employ them as deposits or reservoirs for adipose matter[1]. With respect to the use of the fat generally, this will be treated of hereafter.

SECT. 3. *Formation of Bone.*

There are few subjects in physiology that have afforded more scope for speculation and hypothesis than the origin of bone, or the manner in which the process of ossification is accomplished. The ancients, who were ignorant of the nature of bone, could not be expected to form any accurate notions on this subject, and they accordingly satisfied themselves with saying, that there was present in the fluids an ossific matter which became condensed, as some thought, by the operation of animal heat, some by the evaporation of its watery parts, or, according to others, by mere pressure. But these opinions, and many others equally vague and gratuitous, were refuted by the modern physiologists, and particularly by Haller and Albinus[2]. Haller made a number of minute observations upon this point, which, although they did not lead him to a perfect knowledge of the subject, at least enabled him to avoid the gross errors of his predecessors. His opinion was that as the growth of the body generally depends upon the arterial blood, so that of each of its individual organs is immediately effected by an impulse given to the vessels of the part, by which an additional quantity of fluid is carried to it; and that, in consequence, either of some provision of the system, or of some occasional exciting cause of a more mechanical nature, the action of particular arteries is augmented at certain periods of life, so as to cause their successive development[3]. With respect to the bones, his idea was that the osseous particles being, as he styles them, of a gross nature, the small arteries of the fœtal bones are not capable of receiving them. At a certain period, however, as the heart acquires more force, it propels its contents more powerfully, and thus distends the vessels, and enables them to receive the earthy particles. But, after a certain quantity has been deposited, and the bone has acquired a certain degree of firmness, its rigidity resists further distention; and, at length, by the continued addition of the osseous matter, the whole becomes solidified, and concretes into a perfect bone[1].

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[1] Mr. Wilson, in his Lectures on the Skeleton, entertains the same idea respecting the use of the marrow, p. 48 et seq. See also the remarks of Dr. Benson, Cyc. of Anat. v. i. p. 435.

[2] See Acad. Annot. lib. vii. c. 6, for a sketch of the opinions of the earlier anatomists upon the nature and formation of bone, as well as for the author's own views upon the subject.

[3] This hypothesis of the successive development of the different parts of the body, in consequence of local arterial action, constituted one of the favourite speculations of Cullen; it does not clearly appear whether he or Haller has the merit of priority.

Many objections present themselves against this hypothesis, both when it is considered in its general outline and in its detail. It is altogether of too mechanical a nature, and attributes all those changes to a mere alteration in the diameter of the vessels, which probably depend upon some action immediately connected with the functions of life. If we descend to particulars, we may ask what became of the osseous matter before the arteries of the future bone were sufficiently capacious to receive it? The arteries of the bone begin to convey the earthy particles before they are large enough to admit the red particles of the blood, so that, from the very earliest period of fœtal existence, we have arteries which are obviously larger than those that are sent to the bones when they begin to acquire their earthy matter; why, then, we may ask, was not this matter deposited in these larger arteries? and, in short, according to this mechanical view of the subject, what prevents the whole body from becoming ossified, as each separate artery, or system of arteries, acquires sufficient magnitude to admit the passage of these gross particles? On this, however, as on many other topics in physiology, it is extremely easy to overthrow the hypotheses of others, but very difficult to substitute more correct or consistent ones in their place; and, on the subject now under consideration, I confess that I am not in possession of any adequate means of explaining the difficulty. Under these circumstances, I shall proceed to give a brief description of the phenomena that attend the process of ossification; and, without attempting to reduce them to a regular theory, I shall offer some remarks upon them, and shall endeavour to show how far they can be reconciled with the other operations of the animal œconomy, and how far they must be admitted to be inexplicable. And I must here remark, that although I have thought it necessary to be explicit in my objections to Haller's hypothesis of ossification, yet I am fully disposed to allow him every degree of merit for the accuracy of his statements; for it is to his treatise on the formation of bone[2] that we are indebted for the first, as well as some of the best observations that we possess upon the subject.

When we examine the fœtus, in the earliest stages of its existence, as soon as we are able to observe the rudiments of its future limbs, and the different parts which are destined to compose the skeleton, we are able to trace the figures of some of the larger bones, but they appear to be composed of a matter which is perfectly soft or semi-fluid, contained in a delicate membrane. By degrees the parts acquire more consistence, and the membrane becomes more dense, until they gradually assume the appearance and exhibit the properties of cartilage. This cartilage which is at first transparent and colourless, after some time exhibits opake, whitish spots on different parts of its surface, which, when examined by the microscope, are found to consist of a number of delicate lines; these increase in size and in density, and at length red points are seen to be dispersed through them, indicating that the blood-vessels of the part are sufficiently capacious to admit the passage of the red globules through them. From this period, which, according to Blumenbach, is, in the human subject, about the seventh or eighth week after conception[1], the earthy matter is copiously deposited in its appropriate cells; the parts, which were at first soft and afterwards elastic, now become hard and rigid, so that the blood seems to be scarcely capable of forcing a passage through its vessels, compressed as they are by the dense matter which accumulates round them in all directions, and either entirely obliterates them, or at least greatly diminishes their number and capacity[2].

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[1] El. Phys. xxix. 4. 23 et seq.; Op. Min. t. ii. p. 595 et seq. See also Winslow's Anat. by Douglas, sect. 2.

[2] Exper. de Ossium *Form. in Oper. Min. t. ii.* p. 460 et seq. et 536 et seq.

From Mr. Howship's elaborate observations on the process of ossification, which seem to have been conducted with much accuracy, it might appear doubtful, whether the first deposition of phosphate of lime is not anterior to the formation of the cartilage, for he informs us that, in the long bones, the first appearance of osseous matter is a short hollow cylinder, which is said to exist before any cartilage can be distinguished, and which is conjectured to be secreted by the vessels of the periosteum[3]. Before, however, we can admit this inference, we must decide in what sense the term cartilage is to be employed; and it must be proved that the soft matter, in which this osseous cylinder is formed, is not itself the future cartilage, merely in a soft state, united to a large proportion of water. It would seem, that at this early period, it is difficult to recognize either the periosteum or the cartilage, and that it is rather from theoretical deduction, than from actual observation, that we assume the presence of either of them.

During this deposition of bony matter another very important operation is going forwards. The cartilage, which is destined to become the basis of the future bone, is homogeneous in its texture, and contains no cavities of any kind, but while the different parts of it are changed in their chemical composition, its mechanical structure undergoes an equal alteration. In proportion as the secretory arteries deposit the proper bone, the absorbents carry off the cartilage; but although their action corresponds in point of time, they differ as to the seat of their operations, the greater quantity of osseous matter being deposited on the external part of the bone, while the absorption is carried on at the centre, so that when the former acquires its proper degree of hardness, the interior is either reduced to a complete cavity or into the loose cellular substance that has been described. In contemplating this very curious metamorphosis, many important subjects of inquiry present themselves to us; and among others we may ask, in what way is the earthy matter, which we assume to be conveyed by the arteries, deposited by them in its appropriate situation; whether it is forced out by their extremities, or discharged from their sides by a kind of infiltration, or whether it remains lodged in them, so as in fact to convert the capillary arteries themselves into osseous fibres. To this question we are unable, I conceive, to give a decisive answer; but, upon the whole, it appears to be the most agreeable to the general actions of the animal œconomy to adopt the idea, that the phosphate of lime is poured out from the extremities of the vessels.

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[1] Inst. Phys. § 642.

[2] See appendix at the end of the chapter.

[3] *Med. Chir.* Tr. v. vi. p. 263 et seq.; v. vii. p. 387 et seq.

And here again a new difficulty occurs respecting the mode in which the deposition takes place. The cartilage previously appears to be an homogeneous body, yet the earth is deposited according to a specific mode of arrangement, which must depend either upon some mechanical change in the texture of the cartilage, or upon a tendency in the particles of the phosphate of lime to assume this peculiar arrangement. I apprehend that we have no facts, and only a very imperfect analogy, which can enable us to form any opinion on this point, and until this difficulty be solved, it is impossible to form an adequate theory of the process of ossification[1].

In order to investigate the subject, and to throw any real light upon the nature of the effect that is produced, we must first of all inquire, what is the exact nature of the animal matter that occupies the place of the future bone or composes its basis. Some physiologists, as Haller, Sabatier, and Boyer[2], have stated that it is gelatinous, Bichat[3] calls it mucilaginous, while Broussais styles it albumino-gelatinous[4]; but all these terms, we may presume, were employed in a vague sense, referring more to the physical properties and consistence of the substance, than to its chemical nature, which it is probable was never accurately examined, nor indeed was the knowledge of animal chemistry sufficiently advanced to enable the earlier writers to obtain any correct knowledge on this point. Even Bichat, when he styles it mucilaginous, does not appear to have affixed any other meaning to the word, than that of a semi-fluid substance, possessed of a certain degree of tenacity; for the proofs which he brings in support of his position are altogether inadequate. As far as we are able to form an opinion on a point in which we are entirely guided by conjecture, it is more probable that the first rudiment of the bone is gelatinous than mucilaginous. We find that all the membranous parts of young animals contain a considerable quantity of jelly, and that, as they advance in life, the proportion of jelly gradually diminishes, while that of the albumen, which constitutes the proper membrane, is increased. Besides, mucilage appears, in all cases, to be the product of glandular secretion, and we have no proof of the existence of any organs of this description connected with the fœtal bones.

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[1] Dr. Roget's section on the "Formation and Development of Bone," may be here referred to for many valuable remarks on these topics; Bridgewater Treatise, v. i. p. 375 et seq.

[2] Haller, El. Phys. xxix. 4. 23; Sabatier, Anat. t. i. p. 13; Boyer, Anat. t. i. p. 40; see also Gibson in Manch. Mem. v. *i.* new series, p. 151.

[3] Anat. Gén. *t. ii. p. 189.*

[4] Physiol. t. i. p. 10.

In its second, or what may be called its cartilaginous state, I am not aware that any direct experiments have been performed upon its chemical nature; but as it is then in a condition which admits of more minute examination, and exists in much larger quantity, we are better acquainted with its physical properties, and there is reason to suppose that the substance which occupies the situation of the future bone, is nearly of the same chemical nature with the membranous matter that afterwards enters into its composition. Still, however, the mechanical disposition of its parts differs so much in the two states, that it seems most probable, and is most analogous to the usual operations of the system, that the first cartilage should be entirely removed, and that a new deposition of animal matter should take place. We are not able to determine precisely what is the nature of the change which induces the partial opacity of the cartilage in those places which afterwards become the centres of ossification, whether it be merely a greater condensation of the part, or the abstraction of a portion of the water contained in it, or whether it be the commencement of the actual deposition of the osseous matter[1].

The next thing that we observe is the presence of the vessels carrying red blood, a circumstance which must no doubt depend upon an increased local action; but what is the immediate cause of this, or what connexion it has with the previous condition of the cartilage, is altogether unknown. I have already pointed out the difficulty of explaining the manner in which the deposition of the earthy matter is brought about, and indeed enough has been said to prove that although we are acquainted with the different steps of the operation, and with the order in which they succeed each other, we are scarcely able, in a single case, to decide upon their efficient cause, or to point out any connexion between them.

In considering the formation of bone, and especially the immediate source whence its component parts are derived, it may be proper to notice an hypothesis, which for some time enjoyed a considerable share of celebrity, and which, although at present discarded, deserves to be mentioned as having led to some important facts on the subject. Duhamel, an ingenious French naturalist, had formed an opinion, that the successive layers or annual rings of wood, which are formed in the trunks of trees, are deposited from the inner bark, or rather that the inner bark of each year is, during the following season, converted into the alburnum, or the external layer of the proper wood. This hypothesis he endeavoured to extend to the bones, and for this purpose devised a set of experiments, which were prosecuted with much diligence. It had been accidentally discovered[1], that when an animal has had madder mixed with its food, the bones become tinged with a reddish colour. He accordingly gave madder to an animal for a certain period, then omitted it for some time, and afterwards again resumed its use, when upon examining the bones after this plan had been pursued, he informs us, that they exhibited alternate rings of a red and white colour, corresponding to the times when the animal had used the madder or omitted it. His conclusion was that the bones are formed of concentric laminæ or rings, which are deposited from the periosteum or investing membrane; and the results of his experiments, as he reported them, were generally conceived to afford decisive evidence of the truth of his hypothesis. Mr. John Bell shrewdly remarks, that when speculators perform experiments, they generally find exactly what they desired to find, and so it appears to have been with Duhamel. We are now assured that the succession of differently coloured rings which Duhamel described, could have no existence, or that if any thing resembling them took place, it could have no connexion with the periods during which the madder had been given or withheld. The hypothesis was indeed very satisfactorily controverted by Haller[2], who at the same time gave the proper explanation of the phenomenon, supposing that it depended upon the affinity which exists between the colouring matter, and the phosphate of lime[3]; this opinion Rutherford has since confirmed by direct experiment[4], and has correctly referred it to the general principle by which colouring matters are peculiarly disposed to unite to earthy salts, a principle upon which the operation of mordants in the art of dyeing depends.

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[1] See the remarks of Dr. Milligan, in his notes to the Translation of *Magendie's Physiol. p. 703..5.*

On the subject of ossification, I shall only further remark, that its immediate cause appears to be unknown, but that, in its general nature, it may be considered as analogous to those operations which we ascribe to the function of secretion, where the arteries possess the power of either separating particles already existing in the blood, and appropriating them to some specific purpose, or of forming new combinations, which may be afterwards separated and employed in different ways. The only circumstance that is peculiar to this case is, that the secreting process is confined to a limited period of our existence; that it commences without any assignable cause; and when it has proceeded for a certain length of time, and supplied the wants of the system, it ceases in a way which is equally inexplicable. When we come hereafter to treat more particularly upon the growth of the body, and the gradual development of its different organs, we shall observe this adjustment of its physical condition to the circumstances in which it is placed, not only as respects the whole system, but in each of its individual parts; and we shall find, with regard to the bones in particular, that they receive their perfect form, and complete constitution, in the order which is the best adapted to the situation of the animal. As to the cause which determines these effects to be produced at certain periods of our existence, we can say little more than that we find it to be a matter of fact. It is a part of the general constitution of the animal system, that, at regular times, certain changes should take place, without our being able to assign any physical cause for them. In the present case, the final cause is sufficiently obvious; at the commencement of our existence, softness and flexibility, are absolutely requisite, and hardness would be injurious, while, as the necessity for resisting external violence gradually arises, the capacity for resistance is proportionably produced.

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[1] Haller, El. Phys. xxix. 4. 26; see also Duhamel in Mém. Acad. pour 1739; and Gibson, in Manchester Mem. vol. i. new series, p. 146.

[2] El. Phys. xxix. 4. 33..36.

[3] § 26.

[4] Blake on the Teeth, p. 138 et seq. It is not a little remarkable that Hunter fell into the error of supposing that the madder attaches itself to the animal matter of the bone, and not to its earthy part; Home's Lect. on Comp. Anat. p. 64.

The power which the constitution possesses of repairing bones when accidentally injured is, perhaps, more wonderful in its operation than that which originally produced them, as it exhibits, in a more remarkable manner, that mutual adjustment of the different corporeal actions, and the adaptation of it to fortuitous circumstances, which distinguishes the animal machine from all mechanical contrivances. Not only do we find that if a bone be completely divided the fractured ends are quickly cemented together, and rendered as firm as before the injury; but that even, after a considerable portion of the bone has been removed, a new piece is generated to supply the deficiency[1].

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[1] The publications of Mr. Park and M. Moreau, on the excision of diseased joints, exhibit, in a remarkable degree, the powers of the constitution in repairing injuries of the bones, or rather replacing considerable portions of bone that had been removed. The fourth volume of the Dublin Hospital Reports contains an interesting paper by Mr. Crampton, on the same subject; a paper which indicates the talent of a skilful operator, combined with a correct knowledge of the animal œconomy. We have some cases of the same kind in Ed. Med. Jour. v. xl. p. 338, 9; and a series of experiments by Flourens on the regeneration of bone, in Ann. Sc. Nat. t. xx. p. 169. For the most complete account of the operation and its effects,

A similar kind of controversy subsisted, for a long time, respecting the reparation of bone as concerning its original formation. The older writers supposed that the soft mucus or jelly, which is effused in the first instance, was condensed by heat or pressure into a hard gluten, which formed the uniting substance[1]. This they called callus, and conceived that it always retained its membranous state, and was never converted into proper bone. Some physiologists supposed that this callus was immediately produced from effused and coagulated blood, and others that it was derived from the periosteum of the old bone. It is now, however, generally understood that the process by which bone is repaired is very similar to that by which it is originally produced; the arteries of the periosteum and the neighbouring parts[2] throw out a soft matter called lymph, the nature of which has not been exactly ascertained[3]; this be-

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I may refer to Mr. Syme's Treatise on the Excision of Diseased Joints; see also his Principles of Surgery, p. 323..9.

[1] Boerhaave, Aphor. 343 et seq. cum comment. Sweiten.

[2] From the experiments of Mr. Wood, it would appear, that the vessels which are principally concerned in this process are, in the first instance, those which belong to the internal membrane of the bone; Manch. Mem. v. iii. new series, p. 275 et seq. This opinion, as well as the other modern doctrines respecting the reparation of bone, allowing for some inaccuracy in the terms employed, necessarily depending upon the imperfect state of chemical science, may be found in the writings of Haller; see Op. Min. t. ii. p. 477 et alibi; also Scarpa de Struct. Oss. p. 31. Sir B. Brodie, in giving an account of the mode in which fractured bones are united, states the process to consist of the following steps:—There is, in the first place, a thickening of the neighbouring parts, by which a quantity of a gelatinous matter is effused, constituting the basis of the callus; this callus then becomes ossified; and finally, after the union of the extremities of the bone, the callus is itself absorbed. It would appear, that it is not the vessels immediately belonging to the bone, but those connected with the adjoining parts, both muscular and cellular, which are the agents in this operation. We have an account of some experiments by Mr. Murray, of Aberdeen, which lead to the same general conclusion, with respect to the vessels concerned; Ed. Med. Jour. v. xxxvi. p. 377. The formation of the callus, its subsequent ossification and gradual moulding into its appropriate figure, have been minutely described by Breschet and his colleagues; their opinion may be considered as generally similar to that of Sir B. Brodie. See also the remarks of Mr. Quain, Anat. p. 46..9. In the Anat. Pathol. of Cruveilhier, t. ii. p. 25 et seq., we have an account of the experiments and the opinions of some of the Continental writers in the beginning of the century. The elaborate article on the "Pathological Conditions of Bone," by Mr. Porter, in the Cyc. of Anat. may be advantageously consulted; also the art. "Ossification du Cal," by Villermé, in Dict. Sc. Méd. t. xxxviii, and "Os," by Marjolin, in Dict. de Méd. t. xl.

[3] We should be induced by analogy to conclude that the matter effused, in this case, is principally composed of coagulated albumen; but I believe it has not been made the subject of distinct experiment. Dr. Dowler's experiments prove that fibrine enters into the composition of the buffy coat of the blood, and probably of the fluids which are poured out in the adhesive inflammation of the soft parts; Med. Chir. Trans. v. xii. p. 86 et seq. Some of the late French physiologists speak of the lymph which is effused in inflammation as an albumino-fibrous substance, but this opinion appears to be founded merely on analogy.

comes gradually converted into cartilage, or rather perhaps, is replaced by it, after being itself previously absorbed; the earth of bone is then deposited in this cartilage, and the cartilage either removed, or new moulded, in the manner which was described above. But what is the immediate cause by which this change is effected, why the arteries throw out this substance, how it is moulded into its proper form, whence the supply of earth is derived just at the exact period when it is required for the wants of the system, are questions that have not yet been satisfactorily answered. The hypotheses that have been formed upon the subject have been, in some cases, the mere expression of the fact in different words; in others, the substitution of the final for the efficient cause; or they have proceeded upon the assumption of some imaginary agent created by the fancy of the writer to meet the present emergency. We cannot doubt that there is a proper efficient cause for this, as well as for every other change which occurs in the system; and that, were our knowledge of the animal œconomy complete, we should be able to refer it to the general laws by which the body is directed. At present, however, our acquaintance with the minute operations of nature is extremely limited, and we are only retarding the advancement of science by premature attempts at explaining them.

SECT. 4. *Connexion of Bone with the living System.*

Having now taken a view of the structure of bones, of their physical properties and chemical composition, and made some remarks upon the mode of their growth and formation, it remains to consider the nature of their connexion with the system at large, and the properties which they possess, as forming a part of a living organized body. I have already remarked, that bone, in its most perfect state, possesses few blood-vessels, compared with many other structures; it does not seem that any nerves are sent to it, and we judge of the presence of the absorbents, rather from observing effects which can be ascribed to no other cause, than from being able actually to demonstrate their existence. Bone is, consequently, devoid of sensibility, and is also equally without contractility; it partakes only in a small degree of the general action of the system, and its changes of all kinds are effected slowly, and often in an almost imperceptible manner. Yet, like all other organized parts, we have reason to suppose that every portion of it is connected with both the arterial and the absorbent systems, and that, in process of time, each particle is removed, and fresh ones deposited in their place. This gradual exchange of old for new matter is proved by the phenomena which attend the growth of bone[1]. A solid organized body cannot grow by the distention of its parts, or by the accretion of new matter to its external surface, but by the gradual re-modelling of the whole. If the secreting vessels be supposed to act more powerfully than the absorbents, the new matter is either conveyed more rapidly, or in greater quantity than the old matter is removed, so that the bulk of the whole is ultimately increased, and yet the operation is effected so gradually, that the general form of the bone and the relation of its different parts to each other are not materially altered.

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[1] The experiments of Duhamel, on the effect of madder upon the bones, were generally supposed to afford the most direct proof of this interchange

These observations refer to the bones in their healthy state. When labouring under disease, they exhibit very unequivocal marks of vitality, being subject to affections which are precisely similar to the inflammation, swelling, and suppuration of the soft parts, making allowance for the difference of their mechanical structure. And although healthy bone is insensible, yet, in some of its diseased states, it becomes exquisitely painful; and, in this case, it may be presumed that the sensation arises, not from any nerves actually sent to the bone itself, but from its increased bulk and firm texture pressing upon or irritating the nerves that are distributed upon the contiguous parts, as takes place with respect to dense membranes of all descriptions.

The same general observations, with respect to the nature of their vitality, will apply to the bones as to the cartilages and the tendons; but there is one point respecting it, which appears to present an additional source of difficulty; are we to consider the earthy matter as organized and possessed of life? Perhaps, at the first statement of this question, every one will be disposed to deny the possibility of life being attached to an earthy salt, and, in a general sense, the objection is valid. But when we come to consider the subject in its most minute relations, it will not be easy to point out any essential difference between the earthy and the animal matter which enters into the constitution of bone. They are both derived from the blood, and deposited by vessels connected with the arterial system; they both possess a specific determinate arrangement; and they are both, after a certain period, taken up by the absorbents, and again carried into the mass of circulating fluids[1]. It is not improbable that, before they are either of them expelled from the system, or are again applied to any other use in it, they undergo decomposition, and that part of their elements may be employed in forming new compounds, while the remainder may be rejected by some of the excretory passages.

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of particles, even by those who admitted the hypothesis of the concentric layers to be imaginary. But the experiments and reasoning of Mr. Gibson have shown, that the removal of the red matter depends upon the serum, which circulates through the vessels of the bones, abstracting the colour from the phosphate by its superior attraction for it; Manchester Mem. v. i. new series, p. 160.

[1] See the remarks of *Dr. Roget; Bridgewater* Treat. v. i. p. 382, 3.

I should be inclined, therefore, to say, that the phosphate of lime, while forming a part of an organized body, is alive, because the bone is so generally; but the phosphate of lime, or its elements, while they are circulating in the blood, or passing off by the kidney, or alimentary canal, cease to be so, in the same manner as the carbon which is expired from the lungs, or the mucus which is expelled from the mouth, are not considered as being alive, although they may, perhaps, a short time before, have been employed in the composition of a muscle or a nerve. This view of the subject will lead us to reject the mechanical idea which has been entertained by some physiologists, that the earthy matter of the bones is simply deposited in the interstices of the membrane, and has its particles kept together merely by the cells in which they are lodged. I conceive that the earthy particles have an affinity for each other, and perhaps for the membrane, by which they are combined in a form that belongs to them, as necessarily as to any of the soft parts, although it produces in them a peculiar arrangement, which may not be found in any other substance.

APPENDIX TO CHAPTER II. FROM PAGE 66.

I SHALL take this opportunity of noticing the speculations of Serres, respecting what he terms the laws of "Zoognie," which he conceives regulate the formation of all the organs of which the body is composed, and the bones among the rest. These laws are two in number, and are denominated the law of symmetry and that of "conjugaison;" the first of these is designated as "le principe du double développement des organes," the second as "le principe de leur ré-union." He adds, "De ces deux lois dérive toute la morphologie des organs."[1] In considering the progress of ossification, Serres dwells much upon a circumstance, which he supposes has considerable influence in the development of the parts, "la marche excentrique de l'ossification de toutes ses pièces." It is stated as a matter of fact, that if we watch the gradual formation of the bones, we shall perceive that the external parts are first visible, and that the interior and central parts are composed of productions from these. It is in consequence of this eccentric progress of ossification, that the double development of the single parts, which compose the centre of the skeleton, is effected; and hence arises the law of symmetry, by which, with a few exceptions, the two sides of the skeleton correspond to each other.

The effect of the law of "conjugaison," is next examined, and its operation is pointed out in the formation of the various cavities, holes, and canals, which are found in the bones, and which are supposed to be produced by a union of what were originally separate parts, or, as the author expresses it, "de l'ingrenure des pièces primitives dont les os sont composés." By the application of these principles, it is supposed, that what we may consider as the mechanical process by which the solid framework of the body is progressively developed, may be explained, and the relation detected which its component parts bear to each other. The same principle is applied to every part of the body. Their growth is supposed always to proceed from the exterior to the interior parts, where the union takes place, and thus forms the central or single organs, which are found in so many situations. It is by this operation that the apertures, canals and tubes of all descriptions are formed, as the intestines, the œsophagus, the trachæa, and even the aorta. The same principle is applied to the great cavities of the body, the thorax and the abdomen, and even the nervous system is said to exhibit the same laws in its formation. The truth of these laws obviously depends upon the degree in which they accord with the observations made on the progressive development of the organs, and of the analogies which may be traced between the higher orders of animals and those of a more simple structure. I may add, that the details into which the author enters in support of these and his other positions are very numerous, and bear every mark of having been prosecuted with great industry and accuracy.—Anat. Comp. du Cerveau, Prel. Dis. In connexion with this theory of Serres, I may refer to the work of Is. St. Hilaire, entitled "Histoire des Anomalies de l'Organization," in which the author employs it to explain the production of irregular or monstrous formations. There are few modern works that display a more philosophical spirit; it embraces a wide range of subjects, while its materials are judiciously selected and well arranged. According to the former theory, which was that embraced by Haller and Cullen, and generally adopted by their contemporaries and immediate successors, the heart and brain are supposed to be the centres, from which the sanguiferous and the nervous systems are respectively formed; Is. St. Hilaire styles it "Theorie du développement centrifuge." In the "Theorie du développement excentrique ou centripète," the branches are supposed to produce the trunks, the progressive formation of the parts following the course of the venous blood, while in the former theory it was in the reverse direction, according to the course of the arterial blood; p. 440, 1 et alibi. See also the remarks of Flourens, on the symmetry of the vital organs, in Ann. Sc. Nat. t. iii. (new ser.) p. 40 et seq.

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[1] *Anatomie comparée du Cerveau*, p. 25.

CHAPTER III.

OF MUSCLE.

THE next subject which we are to consider is the muscles, and I shall arrange what I have to say respecting them under six heads. I shall first describe the form and structure of muscles; second, their chemical composition; in the third place, their properties; fourth, their uses; fifth, their mechanism; and lastly, I shall offer some remarks upon the hypotheses that have been formed to explain their action.

SECT. 1. *Form and Structure of Muscles.*

Muscles constitute what we call the flesh of animals, but although these terms are now by every one regarded as synonymous, the older authors made a distinction between what they styled the flesh, and the fibrous part, regarding this latter only as the proper organ of motion; and it was not until the middle of the seventeenth century that this error was rectified by Steno[1]. In their usual form, muscles are composed of masses of fibres[2], lying parallel to each other, intermixed with a quantity of membranous matter, a structure which is visible to the naked eye, and may be rendered more apparent by cutting the muscle transversely, and macerating it, for some time in hot water, or in alcohol[3]. The whole muscle is enclosed in a membranous sheath, which covers it in every part, except where its ends are attached to the bones. We observe that the fibres are disposed into small bundles, called lacerti, each of which is also inclosed in a sheath of membrane, and that these bundles are divisible into still smaller bundles, apparently without any limit, except what arises from the imperfection of our instruments.

Although the fibres of many of the muscles appear to be of considerable length, yet it has been doubted whether this be actually the case, or whether what appears to be one continuous fibre may not, in reality, be made up of a number of smaller ones that are connected at their extremities; the authorities for each of these opinions are nearly balanced, but, perhaps, those for the continuity of the fibre may, upon the whole, preponderate. The fibre is represented by many writers as exhibiting a wrinkled or waved appearance; but there is reason to doubt whether this be its natural state, and whether it may not depend upon the condition in which it is found, when it is examined after death, and detached from the neighbouring parts. In most muscles, the centre is thicker than the rest, and appears to contain more fibres; this is called the belly; hence it gradually diminishes in size to the extremities, one or both of which terminate in a membranous body, which is either a tendon, or an expanded membrane, called an aponeurosis, according to the situation of the muscle, and its connexion with the neighbouring organs. There are considerable interstices between the muscles, which are occupied by fat and cellular texture, and in these intervals a safe lodgment is afforded for the trunks of the blood-vessels and nerves. Most of the large muscles are situated near the surface, covering the bones, and filling up the spaces between them so as to produce the general form and outline of the body. Besides the aponeuroses, which are attached to the muscles, and the membranous sheaths which cover them externally, and inclose their lacerti, expanded membranes are often found entering into the body of the muscle, and dividing them into separate portions. All these varieties of mechanical structure are obviously adapted to the uses of the individual muscles in which they are found, and there is no part of the animal œconomy which exhibits more of this kind of adaptation than the muscular system.

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[1] De Musc. Obs. Specimen, in Manget, Bib. An. t. ii. p. 518 et seq.

[2] Croone appears to have been the first physiologist who had a distinct idea of the fibrous structure of muscles, and that muscular motion depends upon the contraction of the fibres. We learn from Eloy, Dict. Hist. "Croone," that he published a treatise, "De Ratione Motus Muscul." in 1664; see also Acta Erud. for 1682; Phil. Trans. 1681, Phil. Col. No. II. p. 22.

[3] For a most correct delineation of the disposition and direction of the fibres of the different muscles connected with the trunk of the body, I may refer to Prof. Tiedemann's beautiful lithographic plates of the arteries; a work which is no less admirable as a specimen of art than of anatomical *accuracy.*

With the exception of some of the viscera, muscles are more plentifully supplied with arteries than any other parts of the body; they are distributed among the fibres in numerous branches, which continue to subdivide with so much minuteness, as at length to become no longer visible. The capillary veins are equally, or even more numerous than the arteries, and form a complete vascular net-work; the contents of which are gradually discharged into larger and larger vessels, until the blood at length arrives at the main trunks. The veins that belong to the muscles are remarkable for the number of valves which they contain. The ultimate termination of the blood-vessels, or the manner in which the arteries are connected with the veins, is not very accurately ascertained; but this is a point which will be considered with more propriety hereafter.

The apparatus of nerves, which is sent to the muscles, is very considerable; and especially to those which are under the control of the will, being greater in proportion to their size than to any other part of the body, except the organs of the senses.

The nerves that belong to the voluntary muscles proceed almost exclusively from the brain itself, or from the spinal cord, whereas the muscular coats of the viscera are, for the most part, supplied immediately from the ganglia. The former are so much more numerous than the latter, that, according to the remark of Haller, the nerves that go to the thumb are more in quantity than those that supply the whole substance of the liver. There are many curious circumstances connected with the distribution of the nerves, and the course which they take, as, for example, where a nerve runs for a considerable distance, as if for the express purpose of supplying a particular muscle, which might have received its nerves from a nearer source; and where two or more nerves come to the same muscle when there is no apparent reason, from the structure of the part, why any one of them alone might not have been sufficient[1]. It has been thought that each separate fibre, or, at least, each of the smallest bundles into which the fibres are arranged, contains one of the ultimate branches of an artery and a nerve; our actual observations scarcely enable us to decide upon this point, but there is some reason to suppose that it may be the case[2].

I have now been describing the structure of muscles as it appears to the naked eye, but many anatomists have attempted, by the aid of the microscope, to ascertain the nature of the ultimate fibre, as it has been called, or that which is no longer capable of further subdivision without a breach of its substance. As is generally the case in microscopical observations, the descriptions that have been given by these writers are very various, both as to the size and the form of the ultimate fibre; and there is also a want of uniformity in the terms which they have employed to express the gradations of the component parts of the muscle, which apparently increases the discordance of their statements.

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[1] These and other apparent anomalies of a similar kind are explained by the ingenious hypothesis of Sir C. Bell; see Phil. Trans. for 1821, p. 398 et seq.; this subject will be considered more particularly in a subsequent chapter.

[2] For plates and descriptions of the muscles, the following works may be referred to; Winslow's Anat., by Douglas, sect. 3; Cowper, Myotomia Reformata; Albinus, Tabulæ Musculorum; Douglas, Descrip. Muscul.; Innes on the Muscles; Cloquet (Jules) Anat. de l'Homme, t. ii. and Manuel, pl. 61..128; Cloquet (H.) Anat. Descrip. p. 299..512; the same by Knox, p. 230 et seq.; Dr. Quain's Anatomical plates, which may be characterized as excellent specimens both of the lithographic art and of anatomical accuracy, while the 4th chapter of his anatomy I am disposed to regard as perhaps the most valuable portion of the work; it concludes by a useful "Table of muscles in the order of dissection," p. 412..7. For an account of the properties and actions of the muscles, I may refer more particularly to Bichat, Anat. Gén., "Système musculaire," with the notes of Béclard and Blandin, t. iii. p. 301 et seq.; to Barclay on Muscular Motion; to the 8th chapter of Richerand's Physiol., with Dr. Copland's notes; to the 3d chapter of Mr. Mayo's Physiology; and to Dr. Craigie's Elements, ch. 14. sect. 1. p. 486 et seq.

Leeuwenhoek, who is celebrated for the early use which he made of the microscope in anatomical researches, describes the ultimate filament as being almost inconceivably minute, some thousands of them uniting to form one visible fibre. We learn from him that the ultimate fibres are serpentine and cylindrical bodies, lying parallel to each other; that they are of the same figure in all animals, but differ considerably in their size. He states that their size bears no proportion to that of the animal to which they belong; and that even, in some instances, the smallest animals have the largest fibres; as, for example, the fibres of the frog are said to be larger than those of the ox[1].

Muys, an industrious Dutch anatomist, was engaged, for several years, in investigating the minute structure of muscles, and his description, in many respects, agrees with Leeuwenhoek's, except that he supposes the ultimate filament to be always of the same size. He imagines that the fibres are distributed into regular gradations or series, and that the smallest fibrils of which the last series is composed, are some hundred times less than the finest hair, a proportion larger indeed than that assigned by Leeuwenhoek, yet still too minute to permit us to form any conception of it[2]. Many other accounts of the structure of muscles have been published from time to time; some anatomists described them as being straight, others zigzag or waved, and others wrinkled or knotted: some as being solid and others hollow, while many eminent physiologists have conceived that they are jointed, and consist of a number of parts, connected together like a row of beads[3]. Borelli, a learned and ingenious Italian, well known for his elaborate work on muscular motion, announced that the fibre consists of a series of hollow rhomboidal vesicles, and deduced from this structure a theory of muscular contraction, which he supported by a long train of mathematical problems, and while mathematical reasoning was fashionable in physiology, his demonstrations were conceived to be incontrovertible. A peculiar modification of Borelli's opinion was proposed by Stuart, who thought that the muscular fibre was composed of a string of vesicles, immediately formed from the substance of the nerves, which he conceived was similar to that of the tendons, and that these vesicles were covered by a net-work of blood-vessels[4].

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[1] Arcana Naturæ, p. 43 et seq.

[2] De Fabrica Fibræ Mus., as referred to by Haller, El. Phys. xi. 1. 3.

[3] Haller, El. Phys. xi. 1. 3..6; Sœmmering, Corp. Hum. fab. t. iii. § 14; Prochaska, de Carne Mus. p. 19 et seq.

[4] Dis. de Mot. et Struct. Mus. c. 8. The idea of the vesicular structure of the muscular fibre was embraced by Hooke, in part at least from his own observations, and was at first admitted by Leeuwenhoek, although he afterwards, upon further examination, retracted it. It appears to have been previously employed by Croone as the basis of his hypothesis of muscular contraction, and was adopted for the same purpose by Keill and *Stuart; yet notwithstanding* the sanction of so many learn-

Another opinion entertained respecting the nature of the muscular fibre was, that it is entirely composed of vessels, either possessing some peculiar structure, or consisting of the small branches of arteries. This hypothesis, which appears to have been first broached by Hooke, was adopted by many learned physiologists, especially those of the mechanical sect, and was made the basis of some of their speculations concerning muscular contraction. A number of facts were adduced in its support, but they may all be explained by the numerous vessels which are dispersed through the muscles, without having recourse to the supposition that the fibre itself has a vascular structure[1]. Many celebrated names, and among others those of Willis[2] and Baglivi[3], are attached to an erroneous opinion, that besides the longitudinal fibres, muscles possess transverse fibres, crossing the others at right angles, and that these are important agents in muscular action. This diversity of opinion has in part arisen from the uncertainty which attends all microscopical observations, and in part, no doubt, from the state of mind with which the observers made their inquiries, biassed by a favourite notion, and anxious to discover some appearances which might support their hypothesis. The sagacity of Haller perceived the futility of these fanciful opinions, and his authority greatly contributed to effect their downfall. Since his time the subject has been examined by Prochaska, Fontana, Sir A. Carlisle, Mr. Bauer, Dr. M. Edwards, and Dr. Hodgkin. We meet likewise with a great number of valuable remarks on the muscles, and on the mode of their actions in the writings of Bichat, who, although he has not added any absolutely new facts or observations, has arranged the knowledge which we possess on the subject with much ingenuity, so as to present many parts of it under a novel and interesting aspect. If his classification should appear too minute and intricate, and some of his opinions rather subtile than well founded, still there is in them much that is extremely important both to the anatomist and the physiologist.

Prochaska, in entering upon his work, proposes a nomenclature of the component parts of the muscle, which professes to be derived from the actual structure of the parts. To the larger divisions of the muscles he applies the old term of lacerti, using it in the same sense with Haller and other preceding anatomists; the term fibre he restricts to the smallest divisions of the lacertus, which can be easily separated by mechanical means, while

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ed names, it seems to be totally void of foundation. See Leeuwenhoek, Arcan. p. 43, 54, and 58; Phil. Trans. Phil. Col. No. v. p. 152, and No. vii. p. 188, April 18, 1682. I may observe that this is altogether different from the globular structure which has been announced by some late observers.

[1] See Carlisle, Phil. Trans. for 1805, p. 7.

[2] De Motu Muscul. in Pathol. Spas. p. 46.

[3] Opera, p. 399.

the still more minute parts, which are only to be detected by the use of glasses, he calls the threads or filaments. He informs us that each of the fibres, as well as the lacertus, is inclosed in a proper membranous sheath, but it does not appear that this is the case with the filaments, a number of which are invested in one common sheath, and are connected together by a fine web of cellular texture. The fibre, when properly prepared, and separated from all extraneous matter, he conceives to be of the same thickness through the whole of its extent, and continuous from one end of the muscle to the other, not as Haller and many other anatomists have supposed, consisting of a number of smaller fibres connected together by their extremities. Leeuwenhoek, Muys, and most other preceding writers, had described the fibres as being cylindrical, but Prochaska says that they are obviously of an irregular polyhedral form, and that they are generally flattened, being thicker in one direction than the other. The fibres are not always of the same diameter, they differ in different animals, and likewise in different parts of the same animal, and he also observes that they are smaller in young subjects, and increase in size as the body increases in bulk generally. These circumstances, as he remarks, make it very difficult to institute any very accurate comparison between the size of the fibre in different animals, and render Leeuwenhoek's observations on this point very doubtful.

With respect to the ultimate fibres, or, as he styles them, the filaments, their shape and extent is said to be similar to that of the larger fibres, being flattened polyhedrons, reaching the whole length of the muscle. They differ, however, from the proper fibre in being always of the same magnitude, and this he estimates, nearly as Muys had done, at about one-fiftieth part the size of the red globules of the blood. As the fibres are of different diameters, the number of filaments contained in each fibre must be necessarily different, varying from 100 to 400 or 500. The filaments are solid and homogeneous; when prepared for examination they have a number of depressions or wrinkles on their surface, which gives them a waved appearance, and, when viewed in a certain direction, makes them appear somewhat serpentine or zig-zag, but these depressions he conceives are produced by the blood-vessels, nerves, and membranous bands which crossed them[1].

The account which Fontana gives us of his microscopical observations on the ultimate muscular fibre, is, on the whole, not very different from that of Prochaska. By the use of a fine needle he divided the muscular fibre into small filaments, which seemed to be incapable of further subdivision; these he calls the primitive fleshy filaments, and some hundreds of them unite to compose what he denominates a primitive fleshy fasciculus or bundle, by which he probably means the same division that Prochaska simply calls a fibre. The primitive filaments are described as solid cylinders, marked externally with transverse lines or bands at equal distances; the filaments lie parallel to each other, and are not twisted together, as is the case with the primitive filaments of membrane; and from this circumstance he says that the two parts may, at all times, be distinguished from each other. The extreme branches of the blood-vessels and nerves, although so plentifully distributed through the muscles, do not seem to enter into the substance of the filaments, nor even of the primitive fasciculi. The smallest vessel capable of containing red blood, is about three times larger than the muscular filament, and the smallest nerve about four times larger than the smallest blood-vessel, so that there is no difficulty in detecting them when they are mixed with the filaments[1].

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[1] De Carne Musculari, p. 25 et seq.

The observations of Sir A. Carlisle differ, in many respects, from those of preceding writers, especially of Prochaska and Fontana. He describes the ultimate fibre, by which he appears to mean the filament of the above authors, as "a solid cylinder, the covering of which is a reticular membrane, and the contained part a pulpy substance regularly granulated, and of very little cohesive power when dead." He speaks of it as what may be very easily detected by a microscope, and as not being so extremely minute as had been previously conceived, but he prudently declines stating its actual size. The extreme branches of the blood-vessels and nerves are seen ramifying on the surface of the membrane inclosing the pulp, but we are not able to trace them into the body of the fibre. There is, upon the whole, a simplicity and clearness in this description which inclines me to place confidence in it, but, at the same time, it would be desirable that the observations should be repeated and confirmed, as the authority of some of the anatomists who differ from him is too respectable to be hastily abandoned[2].

The account which Mr. Bauer gives us of the muscular fibre differs considerably from that of either Prochaska, Fontana, or Carlisle. In examining the globules of the blood with his high magnifiers, he found that these bodies, when deprived of their colouring matter, were of the same diameter with the ultimate muscular fibre, and that the fibre was in fact composed of a series of the globules arranged in straight lines. He confirmed his observations by a subsequent experiment, in which, by a certain degree of maceration, he succeeded in reducing a muscle, first, into a number of fibres of the same diameter with the globule, and, by continuing the operation, into the globules themselves; the size of the globule, when deprived of its colouring matter, and consequently that of the muscular fibre, he estimates at $\frac{1}{2000}$ of an inch in diameter[3].

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[1] Sur les Poisons, t. ii. p. 228; pl. 6, fig. 6, 7, 9.

[2] Phil. Trans. for 1805, p. 6.

[3] *Phil. Trans. for* 1818, p. 174, 175; pl. 8. fig 4, 5, 6.

Dr. M. Edwards, in the course of his miscellaneous researches into the different textures of the body, examined the structure of the muscular fibre. He informs us that, like all the other parts, it is resolvable into a series of globules, so far coinciding with the opinion of Mr. Bauer; but he differs from him very much with respect to their size; for while this latter physiologist states their diameter to be $\frac{1}{2000}$ of an inch, Dr. Edwards conceives that it is no more than $\frac{1}{7500}$, a difference so great, as to lead to the suspicion, that some optical deception must have interfered with the observations[1].

I have already referred to Dr. Hodgkin's examination of the intimate structure of the various components of the body. With respect to the muscular fibre, he informs us, that although he employed the most powerful microscopes, he could not detect the globular appearance, and that the muscle was ultimately resolvable into minute fibrils, which were crossed, nearly at right angles, by transverse striæ[2]; these striæ appearing to be an invariable character of the muscular structure. We are informed, however, that this peculiar structure is confined, or very nearly so, to the voluntary muscles, and could not be detected in what have been termed the muscular coats, as in those of the bladder, the intestines, the uterus, or the arteries.

Among the more noted hypotheses that have been formed respecting the nature of muscles, independent of their visible appearance, I must not omit to mention one which prevailed very generally about 50 years ago, and was zealously defended by Cullen, that muscles are, to use his own expression, the moving extremities of nerves[3]. The fibres of the muscle are supposed to be continuous with those of the nerve, and to be absolutely the same substance, but that they experience a change in their structure, so that when the nerve is converted into muscle it loses the power of communicating feeling, and acquires that of producing motion. This doctrine of Cullen's seems to have been the result of the physiological speculations that he had formed respecting the nature of life. Following up the idea of Hoffmann, that the animal functions exhibit phenomena of a specific kind, which cannot be referred to any other powers in nature, he classed them together under the denomination of vital; and as both sensation and spontaneous motion were obviously to be placed among the vital operations, he hastily concluded that they must proceed from the action of the same organs. In answer to this hypothesis I think it sufficient to observe, that substances which differ in their appearance and structure, as well as in their physical and chemical properties, can have no claim to be regarded as identical. And with the same remark I may dismiss an analogous speculation, that muscle and tendon are the same substance, differing only in the more condensed state of the latter; an opinion which was transmitted from the ancients, embraced by Boerhaave and his disciples, was adopted by Albinus, who studied the muscles with such minute attention, and was in short so generally admitted, even in the middle of the last century, that Haller[1] and Sabatier[2] scarcely ventured to give a decided opposition to it.

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[1] Mém. sur la Struct. Elém. p. 14. Dutrochet agrees with Dr. Edwards, in conceiving that the muscular fibre is composed of straight rows of globules; Recher. Anat. et Physiol. sur la Struct. &c. Prevost and Dumas' account of the structure of the muscular fibre, being intimately connected with their hypothesis of muscular contraction, will be detailed in the subsequent part of this chapter. Raspail supposes the muscular fibre to be a cylindrical tube, filled with a certain substance partially miscible with water, and containing globules attached to its inner surface; § 491.

[2] Phil. Mag. and Ann. Phil. v. ii. p. 136; also appendix to the Trans. of Edwards, p. 446, 7.

[3] Instit. § 29, 94.

Besides the bodies which I have described above, to which the name of muscles has been generally applied, muscular fibres appear under a different form, and one which is less obvious to the eye, but which is no less necessary to the existence of the animal. I refer to those structures, where fibres, which appear essentially to resemble those of the proper muscles, are attached to membranous expansions, composing what have been called muscular coats. These muscular coats are connected with the hollow cavities that exist in different parts of the body, in the form either of pouches or cylinders, and are destined for the transmission or lodgment of various bodies of a soft or fluid consistence, and which propel their contents by means of these fibres. The mechanical structure of the muscular coats is considerably different from that of the proper muscles; the fibres are much shorter, and instead of lying parallel, as is always the case with the muscles, they seem to be interlaced or twisted together, and, according to Prochaska, sometimes even to anastomose or bifurcate[3]. The fibres of the muscular coats do not exhibit that division into lacerti or bundles, nor have they the regular belly or tapering extremities of the others. Their immediate attachments are also different; the proper muscles have one of their ends at least terminating in a tendon of fibrous membrane, while the muscular coats are attached to membranes that exhibit less of the fibrous and more of the cellular texture.

The uses of these two classes of bodies are likewise very different. The proper muscles are always designed to produce the motion of some part of the body, by altering its relative position with respect to the other parts, while the motions that are caused by the fibres of the muscular coats are designed to operate solely upon the contents of the organ to which they belong, and consist in a number of small contractions, in each of which a few fibres only act at the same time. And these two kinds of organs differ moreover in the connexion which they have to the other parts of the living system, and particularly to the nerves; for while most of the proper muscles are supplied with nerves, either from the brain itself or from the spine, which may be regarded as an immediate appendage to the brain, or are, many of them, more or less dependent upon the will, the muscular coats are, in most cases, supplied from the ganglia, and their action is entirely involuntary.

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[1] El. Phys. ii. 1. 18.

[2] Anatomie, t. i. p. 242.

[3] De Carne Muscul. Tab. 6. fig. 2 and 3.

From this difference in their structure and properties most anatomists have restricted the term muscle to the regular masses of parallel fibres; but Bichat applies it generally to both of them. The proper muscles, as being the media through which we observe the operation of both sensation and motion, those qualities which are essential and appropriate to animal existence, he styles muscles of animal life, while the other class he calls the muscles of organic life, in consequence of their being destined principally for those organs which serve for the support of the individual, but which do not present so obviously the phenomena of sensation and motion[1]. The correctness of Bichat's names depends upon that of his peculiar theory of vitality, which will be examined hereafter: I shall therefore adhere to the former nomenclature, and when I speak of muscle in the abstract I must be understood to refer solely to the larger masses of parallel fibres, while to the others I shall apply the usual term of muscular coats.

In man and the more perfect animals muscles generally possess a reddish brown colour, but this seems not to be essential to them, as by sufficient ablution in water, or by maceration in alcohol, they may be deprived of it, and be rendered nearly white, without having their texture apparently altered. As their colour is most considerable in animals with red blood, it has been usually attributed to a quantity of red blood remaining attached to the fibres, either extravasated through them, or simply contained in the vessels; but Bichat endeavours to show that this is not the case, and that the colour depends upon some foreign substance that is combined with the fibre. He founds his opinion upon the circumstance, that in the same animal some of the muscles are always much redder than others, and yet that they do not appear to have a greater quantity of blood sent to them, and also that in different classes of animals the colour of the muscles does not appear to correspond with the quantity of red blood circulating through their vessels[2]. But whatever be the nature of the colouring matter, we may conclude that it is not necessary to the constitution of the fibre or to its specific properties; for some of the muscular parts that are the most contractile are of the lightest colour. Generally, however, the red colour prevails in the voluntary muscles, and it has also been observed that in those which are naturally coloured, the shade becomes deeper in proportion to the degree in which the muscle is exercised[1].

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[1] Anat. Gén. t. ii. p. 400.

[2] Anat. Gén. t. ii. p. 327.

Besides the membranous substance, which seems to enter into the necessary structure of a muscle, which envelopes its fibres and lacerti, and forms its sheath, determining its figure and preserving it in its proper position, there is likewise a quantity of cellular substance of a more loose texture interspersed through the body of the muscle, and filling up the cavities between its separate parts. This is similar to the cellular substance which enters so largely into the composition of the body generally, and like it appears to be intended to contain both fat and the peculiar albuminous fluid. It has been conceived that, besides the proper fat which is lodged in its appropriate cells, muscles contain a quantity of oil of a more fluid consistence, which is intimately united with them, and serves to lubricate them, to assist their movements, and to prevent their adhesion; but this opinion is rather founded upon conjecture and the supposed utility of the substance, than upon any experiments which have directly proved its existence.

A quantity of albumen, of jelly, and of the peculiar substance called osmazome, may be procured from the muscles by boiling, but it does not appear that these form any essential part of their substance, and it is doubtful whether the jelly be always present in the muscles of the adult. In young animals it appears that the muscles, as well as the membranes and bones, contain a considerable quantity of jelly, but as they advance in age this jelly disappears, and is replaced by albumen. When muscles are digested in warm water, a quantity of saline matter is separated from them, but it is doubtful whether this be attached to the muscular fibres themselves, or be merely lodged in the different vessels that pass through them; the former, however, appears the more probable supposition, because, as far as it has been examined, it would seem not to be the same combination of salts which exists in the blood[2].

When the muscular fibre has been macerated for a sufficient length of time, and is cleared as much as possible from all extraneous matter, we obtain it in a pure state. It is nearly white, without much taste or smell, and if it be kept free from moisture, it will remain a long time without undergoing decomposition or experiencing any change[3]. If the water which has been employed in the maceration, and which contains albumen, jelly, extract, and various salts, be evaporated to dryness, and then treated with alcohol, the extract alone is dissolved, and, by the evaporation of the alcohol, may be obtained in a pure state. This substance was discovered by

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[1] Home's Lect. on Comp. Anat. p. 31 et seq.

[2] Henry's Chem. v. ii. p. 459.

[3] Fourcroy's System, by Nicholson, v. ix. p. 335.

Thouvenel; it has a brown colour, an acrid taste, and an aromatic odour, is soluble both in water and in alcohol, and would seem to be the ingredient which gives the specific flavour to the flesh of different animals, and especially to be the part which forms the brown crust on roast meat. It is to this substance that Thenard has given the name of Osmazome[1].

The salts that are contained in muscular flesh, or connected with it, are principally the phosphates of soda, ammonia, and lime, and the carbonate of lime. For the discovery of the phosphate and carbonate of lime we are indebted to Mr. Hatchett[2]. Fourcroy and Vauquelin inform us that they have detected sulphur and potash in muscles[3], and Prof. Berzelius the muriate, phosphate, and lactate of soda[4]; but perhaps there is still some uncertainty respecting the nature of the salts, and the mode in which they exist when entering into the constitution of the muscle.

SECT. 2. *The Chemical Composition of Muscle.*

The muscular fibre, in its pure state, is readily acted upon by various chemical re-agents. Most of the stronger acids and the caustic alkalies dissolve it; but it will not be necessary to enter into a detail of the phenomena that occur with any of these, except with the nitric acid. By this re-agent fibrin is partly decomposed and partly dissolved, while a quantity of gas is disengaged, consisting principally of azote, united to about one-tenth of its bulk of carbonic acid. The same kind of gases are produced by other animal substances, when they are treated with nitric acid, but the muscular fibre differs from most of them in extricating a larger proportion of azote, indicating that a greater quantity of this substance enters into its composition. As this is the element which prevails in animal bodies, and particularly distinguishes their chemical composition from that of vegetables, muscles are said to be the most completely animalized part of the body; and it is worthy of observation, that in the same degree as the animal frame differs from the vegetable in its chemical constitution, it acquires, at the same time, its most characteristic physiological properties. It is generally understood that the muscles of the animals with red blood, which possess the greatest variety of functions, and enjoy them in the most perfect state, contain more azote than those of fish or reptiles; and that in animals of the same species, those of adult age contain more azote than the same animal soon after birth. This is one among several instances where it would appear that the full development of the animal functions is necessarily connected with a certain chemical constitution;

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[1] Chimie, t. iii. p. 759. Berzelius, however, does not think this substance to be a distinct, proximate principle; see his View of Animal Chem. p. 82.

[2] Phil. Trans. for 1800, p. 395.

[3] Ann. Chim. t. lvi. p. 43.

[4] Thomson's. Chem. v. iv. p. 474.

but although this appears to be a matter of fact, we are not authorized to conclude, with some modern physiologists, that the vital powers are the necessary result of a peculiar chemical mixture.

When the action of nitric acid upon the muscular fibre is promoted by heat, the muscle is quickly dissolved in large quantity, the fluid assumes a deep yellow colour, and acquires a degree of unctuosity, while, at the same time, there is a rapid escape of gas, consisting principally of a mixture of azote and carbonic acid, together with a quantity of nitrous gas. After the acid has dissolved a considerable portion of the fibre, globules of oil appear on the surface; these, when the fluid cools, assume a concrete form, and are found to be a substance of a peculiar nature, which, from possessing properties intermediate between those of fat and wax, has obtained the name of adipocire. Portions of the oxalic and malic acids are also formed, and besides these several other substances are contained in the fluid, and some, as it appears, of a peculiar nature, generated by the action of the nitric acid upon the muscular fibre[1].

The muscular matter experiences a peculiar change in its chemical composition, under certain circumstances, which was, in the first instance, effected spontaneously, but which has been since imitated by various artificial processes. There was an immense burial-ground in Paris, called La Cimetière des Innocens, which, in consequence of some improvements that were going forwards, it was determined to remove. This place had been the receptacle for a considerable part of the population of Paris for several centuries. The number of burials was supposed to be some thousands annually; the bodies were deposited in pits or trenches about 30 feet deep, each capable of holding from 1,200 to 1,500 bodies, which were then covered with a few feet of earth, so that the whole area, occupying about 7,000 square yards, was converted into a mass, consisting principally of animal matter, rising several feet above the natural level of the soil. Upon opening the ground for the purpose of removing this prodigious collection of dead bodies, they were found to be entirely altered in their nature and appearance. What had formerly composed the soft parts of the body was converted into an unctuous substance, of a grey colour, and of a peculiar, but not very offensive odour. According to their position in the pits, and the length of time they had been deposited, the bodies had undergone this transformation in a more or less perfect manner. The transformation was found to be most complete in those bodies that were nearest the centre of the pits, and when they had been buried about three years; and in these cases, every part, except the bones, the hair, and the nails, seemed to have lost all their specific properties, and to have acquired those of this peculiar substance.

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[1] For an account of the experiments of Fourcroy and Vauquelin on the action of nitric acid on the muscular fibre, see Ann. Chim. v. lvi. p. 37 et seq.; also the remarks of Berzelius, Med. Chir. Tr. v. iii. p. 205.

We are indebted to Thouret[1] for an interesting detail of the circumstances that attended this opening of the burial ground, and to Fourcroy for a chemical analysis of the peculiar substance into which the bodies were converted. By subjecting it to the action of the appropriate chemical re-agents, he found it to be a saponaceous compound, consisting of ammonia united to a substance termed adipocire, similar to what is produced by the action of nitric acid upon muscle. When the adipocire was freed from the ammonia, and obtained in a state of purity, it was found nearly to resemble spermaceti, both in its physical and chemical characters[2].

It was afterwards found that this conversion of muscular flesh into adipocire might be produced by other means besides that of inhumation: mere immersion in cold water, especially in a slow running stream, completes the operation much more speedily than the spontaneous decomposition which was carried on in the burial-ground at Paris; and the action of diluted nitric acid is still more rapid; but in this case a portion of the acid adheres to the adipocire, of which it is very difficult entirely to deprive it[3]. On account of its resemblance to spermaceti it was proposed to establish a manufacture of adipocire, from the carcases of such animals as were not proper for food; and attempts of this kind have been actually made, both in this country and in France, but in both cases without success, in consequence, as it appears, of the difficulty of entirely removing from the adipocire its unpleasant smell and dingy colour. From observing what takes place, when the conversion of muscle into adipocire is effected by means of diluted nitric acid, we may conclude that this substance differs from the muscular fibre, in containing less carbon and azote and more hydrogen and oxygen.

SECT. 3. *Properties of Muscle.*

I now come, in the third place, to give an account of the properties of the muscular fibre, and these I shall arrange under the heads of physical and vital; the first comprehending those which are connected with its mechanical form, its structure, and its obvious external characters; the second comprising its powers as forming part of a living organized body. With respect to the first class of properties, those which I have denominated physical, there is some difficulty in precisely ascertaining their nature, in consequence of the close attachment which there is between the muscular fibre and the membranous matter, so as to render it impossible to separate one from the other without altering, or rather entirely destroying, its texture. It seems, however, reasonable to conclude that the muscular fibre possesses all the properties that belong to membrane, although not in the same degree. The muscular fibre is cohesive, but much less so than membrane, while its flexibility is perhaps greater; it is highly extensible, and perhaps elastic. Its extensibility is probably equal to that of membrane; indeed, in most instances, they are brought into action at the same time, and must necessarily exist in the same degree, as the two substances are so intimately connected together; but I am not acquainted with any experiments that have been expressly made upon this point. There is a considerable difference between the extensibility of the proper muscles and that of the muscular coats; the former are much limited in this respect by their local position, and their connexion with the neighbouring parts, while the muscular coats, from their structure, possess almost an indefinite power in this respect, and frequently exhibit very remarkable instances of it. The change in the size of the uterus, the stomach, and the bladder, from their most contracted to their most extended states, is well known to every one; and although, in these cases, it is not certain in what degree the membrane and the muscular fibre individually partake in these changes, yet it is reasonable to conclude that it belongs to them both nearly in an equal degree.

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[1] Journ. de Phys. t. xxxviii. p. 249; see also Home in Phil. Trans. for 1813, p. 149.

[2] Ann. Chim. t. iii. p. 120; t. v. p. 154; t. viii. p. 17.

[3] See Gibbes, in *Phil. Trans. for 1794, p. 169*; and for 1795, p. 239.

There is considerably more doubt respecting the elasticity of the muscular fibre; for although the proper muscles, and still more the muscular coats, exhibit decisive marks of elasticity, yet here it is impossible to say how much of this belongs exclusively to the fibre, and how much it has in common with the membrane. The soft and yielding nature of the fibre, when detached from the membrane, would not indicate any great degree of elasticity[1]; and as Sœmmering remarks[2], the almost total absence of this property in the fibre after death would lead us to the same conclusion, yet there are some phenomena which render it probable that the fibre is possessed of this quality in a certain degree[3]. To the elastic nature of the muscles generally, as consisting of a compound of fibre and membrane, I should refer the *natural* contraction of Whytt[4]; those actions which Cullen, and many physiologists since his time, have called tone, or tonicity, and which Bichat has classed under the head of contractility from texture, in which, after a part has been distended by any cause, when the distending force is withdrawn, it gradually recovers its natural form and dimensions;

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[1] See Carlisle, in Phil. Trans. for 1805, p. 3.

[2] Hum. Corp. Fabrica, t. iii. § 5.

[3] The crassamentum of the blood, when obtained in a fibrous state, is in some degree elastic; but although it agrees with the muscular fibre in its chemical properties, it probably differs in its mechanical organization

[4] Essay on Vital and Invol. Motions, sect. 1. § 3.

and I conceive also, that some of the effects which Dr. M. Hall has ascribed to nervous action, may be more easily explained upon this principle: this point will, however, be considered more fully hereafter.

But all these physical properties of muscle are little worthy of our attention, compared to its vital properties, or those which it possesses as forming a part of a living organized body, giving rise to a class of phenomena, which we consider as essentially connected with life, and the cessation of which indicates that its complete extinction has taken place. Of these properties, by far the most important and interesting, if not the only one, is that which has been styled by many modern physiologists irritability, but to which I have preferred giving the name of contractility. It may be defined, that power which the muscular fibre possesses of diminishing its length, or of contracting and shortening itself. From a very extensive range of facts and analogies we conclude that this power is never exercised without the agency of some direct independent cause, to which the name of stimulant has been applied; and we arrive at this conclusion, not merely from the general principle, that every effect in nature must have its appropriate cause, or that every event is preceded by some other event, which stands to it in the relation of a cause, but from actually observing, that when we see muscular contraction take place, and have an opportunity of examining all the previous circumstances, we can assign the exact event which has produced the contraction. It must, however, be admitted, on the other hand, that the stimulants, or the causes of contraction, are very different from each other in their nature; and in fact, as far as we can judge, have not any single property in common, except that now under consideration, of producing the contraction of the muscular fibre.

Since the time of Haller, who had the merit of first clearly comprehending the nature and extent of this property, and strictly pointing out its effects, the term irritability has been generally applied to it [1]. This word was, I believe, first used in physiology by Glisson, who entertained some opinions on the subject of muscular action, that were, to a certain extent, correct, and were a considerable advance upon the knowledge of his predecessors [2]. Although I feel a great objection to alter the language of science unnecessarily, yet, in the present instance, a necessity for the proposed change exists. In the first place, irritability is a term employed in physiology, in pathology, and in ethics, and in each of these sciences in a different sense. In physiology it simply designates a certain faculty in the muscular fibre; in pathology it signifies a peculiar condition of the vital powers generally, and of their relation to external agents; and in ethics it expresses a certain state of the temper and feelings. Then with respect to physiology alone, irritability is objectionable, because it was employed by Haller and his disciples to express a peculiar property, which is necessarily connected with an hypothesis, not the mere contractility of the muscular fibre, but the way in which the contraction is produced, or rather implying the nature of the cause as well as the effect; and although I am disposed to think that the hypothesis of Haller is correct, yet it is still only an hypothesis; and the influence of language over opinion is so great, that it is always desirable to avoid those phrases which may, even unconsciously, affect our judgment on such topics. The term contractility has the advantage of simply expressing the fact, its use has been sanctioned by several eminent physiologists, and although it may be perverted or improperly applied, it seems in itself to be unobjectionable[1].

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[1] Mém. sur la Nature Sens. et Irrit. des Parties du Corps. Animal, mém. 1. sect. 2.

[2] See particularly his treatise, "De Ventriculo," c. v. in Manget, Bibl. Anat. t. i. p. 80 et seq.

In considering the subject of contractility, it will be proper to begin by giving an account of muscular contraction, describing its phenomena and direct effects, then those of the relaxation of the muscles, and I shall afterwards make some remarks upon stimulants, or the agents which act upon the fibre. Relaxation is the natural state of the muscle, or that condition which it affects, when not acted upon by any external cause. Upon the application of a stimulant its contraction commences; its surface, which was before smooth, now becomes furrowed and wrinkled, its belly swells out and grows hard and firm to the touch, while the ends approximate, and the whole muscle is rendered thicker and shorter[2]. This is an account of the general effect of contraction, but many questions present themselves when we minutely consider the operation. It has been a subject both of theory and experiment, whether the specific gravity of a muscle be increased during its contraction, or whether the fibres gain in thickness precisely what they lose in length [1]; an inquiry which, as will be seen hereafter, is not a subject of mere curiosity, but is connected with the theory of contraction, or the intimate nature of the operation by which it is produced.

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[1] Blumenbach uses it in a different sense, to express the re-action of the cellular texture, making it synonymous with his *vis cellulosa*. Some of the French physiologists employ it in a more general way to express every motion of any part of the body. Fournier, Art. "Contractilité," Dict. des Scien. Méd. t. vi. p. 137 et seq., defines it to be the motion of which all parts of the body are capable except the nerves; and it appears to be employed in the same way by Broussais; at least he does not confine it to the muscular fibre, t. i. p. 14. See the remarks of Rullier, Art. "Contractilité," Dict. de Méd. t. v. p. 567 et seq., and Art. "Force," t. ix. p. 318 et seq. I may notice in this place the proposal, which has been lately made by Dr. M. Hall, to employ the term irritability in a different mode from that in which it is commonly used. Such changes, unless absolutely necessary, I consider to be very undesirable, and I cannot but regret that the attempt should have been made by so active a cultivator of science, and one to whom physiology in particular is so much indebted; see Phil. Trans. for 1832, p. 321 et seq. We have some judicious remarks by Blandin on the mode in which the term has been employed by Bichat, p. xiv et alibi; see also the remarks of Dr. Roget, Bridgewater Treatise, v. i. p. 124 et seq., and of Prof. Tiedemann, Comp. Physiol., by Gully and Lane, § 572 . . 4. p. 401.

[2] Haller, El. Phys. xi. 2. 17 . . 20. The different steps of the process are *well described by Gerdy*, Class. nat. des Phén, de la Vie, p. 4 et seq.

Some experiments were performed by the older anatomists, and especially by Glisson, which seemed to prove that the muscle was altogether diminished in bulk during contraction; and experiments of a similar kind have been more lately made by Sir G. Blane [2] and Sir A. Carlisle [3]. They were, however, induced to conclude that the absolute bulk of a muscle, and of course its specific gravity, is not changed during contraction. Probably, however, experiments of this kind, which consisted in examining whether the bulk of a muscle be affected by its contraction, do not admit of a very decisive result, because we may suppose that while one set of muscles is contracted, another set is relaxed, and that besides the simple effect of contraction, there may be a displacement of parts, or some alteration in their arrangement, which may affect the bulk of the part, without there being any absolute change in the density of the muscular fibre itself [4].

It has likewise been a subject of controversy, whether the quantity of blood in muscles be diminished during their contraction. Most of the earlier writers supposed it to be the case, and this opinion was adopted by the Boerhaavians [5], but it may be inferred that they were influenced rather by hypothesis than by actual observation; for as it was the general opinion that the muscles were rendered smaller by contraction, it was concluded that a portion of their blood must be squeezed out. We are indeed, as a proof of this, told by Winslow [6], and some of the most respectable anatomists of his age, that the muscles become paler during contraction, and again resume their colour when relaxed; and this was particularly stated to be the case with the heart of the frog, and that of the chick during incubation; but, as Haller remarks, this effect does not depend upon the blood being expelled from the substance of the muscles of the heart, but from the cavity of the ventricles, the heart in these animals being so transparent as to permit the colour of its contents to be perceived externally [7]. The opinion that the blood is expelled has been more lately adopted by Bichat, but his reasons for it do not appear of much weight. He principally insists upon the well-known fact, that during the operation of drawing blood from the arm, the flow is increased by contracting the muscles [1]; but the additional quantity of blood that is expelled in this case is not derived from the capillary vessels dispersed through the fibres, but from the larger venous trunks, which are pressed upon by the swelling out of the bellies of the muscles. Sir A. Carlisle also adopts the same opinion, but rather upon general grounds, than as derived from any decisive experiments; he only states, in a loose way, that the muscles become pale during contraction[2], without alleging any proof of the fact; and it may be remarked, that if, as he supposes, the absolute size of the muscle be not affected by contraction, it does not seem likely that the quantity of blood in it will be diminished.

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[1] Haller, El. Phys. xi. 2 . . 22.

[2] Select Dissert. p. 239..241.

[3] Phil. Trans. for 1805, p. 22, 23.

[4] The experiments of Sir Everard Home, on the increased size of muscles during contraction, only prove that their bulk is increased in one direction; Lect. on Comparative Anatomy, p. 33. Mr. Mayo, by employing the heart of a dog, has obviated the objections which exist against the experiments of Sir G. Blane and Sir A. Carlisle; he finds the bulk to remain precisely the same during the states of contraction and relaxation; Anat. Comment. p. 12.

[5] Boerhaave, Instit. § 401.

[6] Anat. sect. 3. art. 1, § 48.

[7] El. Phys. xi. 2. 21.

The nature of muscular contractility, or the relation which it bears to the other powers of matter, has been a subject of long and learned discussion, yet until very lately it was entirely misunderstood. Now, indeed, that we are become familiar with the conception of it, as a property of a specific nature, inherent in the muscular fibre and peculiar to it, it seems scarcely possible to conceive of any quality that possesses more distinct characters. Yet, although Glisson, Baglivi, and others, made some approaches to a correct view of the subject, it remained involved in much obscurity until the time of Haller. This great physiologist, however, clearly pointed out its nature, marked its specific differences, and announced it as being exclusively attached to the muscular fibre; and what, perhaps, should be regarded as the most important of all his numerous discoveries, and the one which had the greatest effect in promoting our knowledge of the animal œconomy, he showed in what respects the phenomena of muscular contractility differ from those of nervous sensibility, and referred them respectively to their appropriate causes[3]. Even the most learned and judicious of his immediate predecessors or contemporaries, as Hoffmann, Boerhaave, and Cullen, were not sufficiently aware of this distinction, and perpetually confounded their effects[4]. But what is a still greater, and, as it now appears, a less pardonable error, they had not learned to distinguish between contractility and elasticity: they referred many of the effects of the former to the operations of the latter; and also endeavoured to account for them entirely upon mechanical principles, such as are alone applicable to elastic bodies. Yet the difference between these two powers is most obvious and essential. Elasticity always depends upon simple re-action, and is never the source of actual power; it merely restores, in a contrary direction, the force which had been impressed; and even, when acting to the greatest advantage, the effect which it produces can never be greater than the amount of the cause, and the re-action can never take place as long as the cause continues to be applied. Thus the force with which a steel spring recoils, even supposing it to be a perfect elastic, is only equal to that which is required to bend it, and as long as the force remains applied, it is impossible for the recoil to take place[1].

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[1] Anat. Gén. t. ii. p. 378. [2] Phil. Trans. for 1805, p. 27.

[3] El. Phys. xi. 2. 4 et seq.; Mém. sur les Part. Sens. et Irrit. Op. Min. t. i. p. 329.

[4] Even the acute and accurate Bichat has not clearly perceived the distinction, and to this cause may be ascribed much of the obscurity and intricacy which we occasionally find in his works. For a clear and perspicuous account of the doctrine of Haller on this subject, I may refer to Dr. Thomson's Life of Cullen, v. i. p. 235..0.

But in muscular contraction we observe a very different train of events. The mechanical effect is infinitely greater than the mechanical cause producing it, and indeed bears no physical proportion to it, while at the very time that the cause is applied, and is acting with all its force, the re-action commences and far surpasses the force of the agent[2]. But what is still more decisive against the doctrine, that contractility is only a modification of elasticity, is, that the most considerable effects of muscular action are frequently produced without any mechanical cause at all, where the agent is of a kind which has no relation to any of the mere physical properties of matter. No fact in the whole range of natural phenomena occurs more frequently to our observation, yet such is the devoted attachment to theory which takes possession of the mind, and so difficult is it to shake off established errors, that all the mathematical physiologists attempted to explain muscular contraction by the laws of mechanical impulse. We can scarcely review, without a feeling of humiliation, the various hypotheses which were invented by the most learned men of the age, and were brought forwards, with all the aid of geometrical demonstration, and enforced by a string of problems, theorems, corollaries, and lemmas. To all this learned trifling it is sufficient to reply, that a mechanical force, of an indefinite extent, is frequently produced without the intervention of any mechanical cause whatever, and must therefore be referred to a principle of a totally different nature.

One of the most remarkable circumstances respecting contractility is, that in all muscular action, however powerful be the stimulant, still after some time the effect ceases, and the muscle becomes relaxed[3]. And this succession of alternation after contraction occurs, even although the stimulus continues to be applied. This we perpetually observe in all our experiments upon muscles, with either mechanical or chemical substances; it likewise takes place in all the natural operations of the system, and is to be observed, in a very remarkable degree, in the muscles that are under the control of the will. In performing any voluntary action, where the mental energy continues to be exercised with equal, or even with greater power, although our very existence immediately depended upon it, we find ourselves unable to persevere in the action beyond a certain length of time. The muscles that have been contracted become, what is termed, exhausted[1], and a certain period is necessary to elapse before they are again capable of being stimulated or excited into action. In a majority of instances we may observe a degree of correspondence between the subsequent exhaustion and the previous stimulation, but many causes interfere to prevent these two circumstances from bearing an exact ratio to each other[2].

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[1] J. Hunter appears to have been one of the first British physiologists who very clearly distinguished elasticity from contractility; Treatise on the Blood, p. 105, 106.

[2] Fordyce, in Phil. Trans. for 1788, p. 35, observes, that if the inside of the heart be slightly scratched by a needle, it will contract so strongly as to force the point of the needle into its substance.

[3] Cullen's Instit. § 108.

The phenomena which attend upon the relaxation of a muscle are precisely the reverse of those of its contraction: the belly becomes soft, its swelling subsides, and the wrinkles disappear from its surface; the force of contraction no longer existing, the ends not being drawn together, recede, and the whole resumes its natural state. Relaxation is generally conceived to be merely a passive effect, and to consist simply in the absence of contraction; but when parts have been displaced by contraction there is a necessity for some absolute power to bring them back to their former situation. This power is in most cases, that of the antagonist muscles. The muscular system is so arranged that, in most parts of the body, one muscle or set of muscles has another muscle or set of muscles which act in precisely a contrary direction, and is intended to produce precisely the opposite effect; one muscle draws a part to the right hand, another to the left; one muscle raises it, another depresses it; and when either muscle has been in action, it generally happens that the opposing muscle then acts and produces the contrary effect[1]. Besides the antagonist muscles, another counteracting force, which is often useful in replacing parts, is elasticity. Muscles are frequently so situated that when they contract, they move some elastic membranous body, or not unfrequently a quantity of elastic membranous matter enters into their own composition, which, when the fibres relax, re-acts and restores the part to its natural position. Examples of this kind occur in the muscles about the thorax and the larynx, where the muscles are connected with cartilages, which are compressed or distended according to circumstances, and which immediately re-act when the contraction ceases. A third means by which muscles are replaced after contraction is the force of gravity: it not unfrequently happens that the action of a muscle has the effect of raising up some part and sustaining it without support, and of course, when the muscular contraction ceases, the part falls down by its own weight. This frequently occurs in the motions of the extremities. The hollow muscles and the muscular coats are excited to contract by some substance which distends their cavities; the act of contraction, by discharging the distending substance, removes the exciting cause, and relaxation naturally ensues.

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[1] The term *exhausted* is here employed as the one in common use, without any reference to the hypothesis from which it originated, because a word did not occur which might convey a more simple expression of the fact. On this subject the reader may peruse with advantage Dr. Park's remarks in the Quart. Journ. v. ii. p. 228.

[2] It may, I think, be questioned, whether exhaustion ever takes place in cases of simple contractility, or whether it be not confined to those in which the sensibility of the nerves has been called into action. We are indebted to Dr. Wollaston for an interesting observation, which renders it probable that the state of exhaustion, or rather the alternation of contraction and relaxation, is much more rapidly produced than is commonly supposed. He founds his opinion upon a peculiar vibratory sound which is perceived when the finger is inserted into the ear with a moderate degree of force. This acute philosopher conceives that, in this case, the voluntary effort, although apparently continuous, "consists in reality of a great number of contractions *repeated at extremely* short intervals." Phil. Trans. for 1810, p. 2.

But although relaxation has generally been considered as simply a passive state, in which the muscle merely ceases to act, and where it is brought into its ordinary condition by other agents, the contrary doctrine has been occasionally maintained; and as it has found a supporter in Bichat, it may be necessary to notice it. He conceives that relaxation is in part at least an active effect, and that it consists in something more than the mere cessation of contraction. He founds his opinion, as it appears to me, upon very insufficient grounds of reasoning; the only fact which he adduces is, that if the heart be grasped during its diastole, it may be felt to press with considerable force upon the hand[2]. But this is too vague an experiment on which to build an opinion of so much consequence. The heart is a remarkably complicated organ with regard to its mechanism, and as we shall afterwards find, when we come to treat upon its motions, there are many circumstances respecting it which require to be taken into consideration.

I have already remarked upon the multifarious nature of stimulants, as they have been called, those agents which possess the specific power of exciting the muscular fibre to contraction. It has been asserted, and is indeed literally true, that every body in existence is a stimulant to the muscular fibre, because, independently of any other quality, the mere contact of a material substance produces this effect. Stimulants have been arranged in various ways[1], but perhaps the most convenient and comprehensive is into the three heads of mechanical, chemical, and what we may term vital. Mechanical impulse of all kinds, beginning with the slightest touch that is capable of being perceived, and proceeding to a degree of violence short of that which absolutely destroys the texture of the part, are of the first class; a great variety of chemical substances that have few properties in common, as alcohol, acids, alkalies, metallic salts, and many vegetable acrids, are of the second class; while in the third we may place those agents, that seem to operate immediately upon the vital powers, without producing any apparent physical change in the part, as the electric fluid, and particularly that modification of it which constitutes galvanism. Independent of any external agents, the muscles are thrown into the strongest contractions by a variety of nervous affections, which arise from internal causes, and above all by the act of volition. By a process which will probably always remain inexplicable, we no sooner will the motion of any muscle, than it obeys the summons with promptness and accuracy.

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[1] The doctrine of antagonism has been made the subject of particular consideration by Prof. Bellingeri, in his treatise on the Spinal column, and by Dr. M. Hall, in a paper in the Phil. Trans. for 1833, "On the reflex function of the Medulla oblongata and Medulla Spinalis." I shall have occasion to enter more fully into the consideration of this doctrine when I come to treat on the nervous system.

[2] Anat. Gén. t. ii. p. 468.

It is a remarkable circumstance connected with the effect of stimulants upon the muscular fibre, that particular sets of fibres are specifically acted upon by particular stimulants, and this without any difference that we can discover in the fibre itself, or any thing in the nature of the stimulant which could enable us to predict the result. Thus certain substances taken into the stomach produce the healthy action of this organ, and cause its fibres to exercise their vermicular motion, so as in due time to propel its contents into the intestines, while others instantly excite the violent action of vomiting. Substances which have passed through the stomach, without producing any particular effect upon its fibres, when they arrive at the intestines, throw these organs into strong contractions. In the same way the urine acts specifically upon the bladder, certain sapid substances upon the salivary glands, and in short there is scarcely one among the muscular coats, or among the system of muscular fibres, that contribute to the production of the organic functions, which have not some specific or characteristic property of this kind. In the proper muscles, those which serve for motion, the effect of stimulants is more uniform, and may, for the most part, be referred to the ratio of quantity.

Concerning the specific nature of contractility, it is most remarkable that it should be called into action by such a variety of agents, and I know of no method of explaining this singularity. The effect of all the substances is, however, the same in kind; or if there should be found any difference in this respect, it is in the proportionate degree of their intensity and their duration, or their subsequent and secondary operation on the system. This great variety in the nature of the stimulating agents affords an additional proof of the absolute impossibility of accounting for muscular action upon any mechanical principles; for it not only seems to be equally affected by the two great powers of mechanic impulse and chemical attraction, but by mere mental impressions, which, as far as we can judge, have no resemblance to any of the properties of matter.

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[1] See Smith, de Act. Mus. p. 32. Blumenbach, § 52, divides them into chemical, mechanical, and mental; Sir G. Blane into internal and external, arranging them according to the situation where they are applied rather than *the mode* of their action; Select Diss. p. 245.

Besides contractility, or the proper Hallerian irritability, the muscular fibre has been supposed to possess another specific or peculiar quality, which has been called tone or tonicity. I have already remarked that Cullen, as well as many of the modern physiologists, have insisted upon this property of muscle; and it is a term which is very extensively employed in pathology, but in this case apparently with little precision, and probably without any decided meaning being attached to the use of it. Physiologists have attempted to describe it more accurately, and it has been illustrated by the retraction which a muscle exhibits when its fibres are divided transversely, or by the drawing up of one side of the face when the muscles of the other side have become paralyzed, and by other similar occurrences. It seems, therefore, to be a contraction which the muscular fibre exhibits when not under the influence of any distending force, which takes place without the intervention of any external stimulant, is slow in its operation and limited in its extent, and is not subject to the alternations of relaxation[1]. Although, therefore, both its direct and its ultimate effect be contraction, yet, in every respect, it differs from the proper contractility which has been described above, and I conceive has no connexion with it. Such a power undoubtedly exists in the muscles, but it is by no means certain whether it ought to be referred to the proper muscular fibres or to the membranous matter to which they are attached. There are many circumstances which seem to render it probable, that all the soft solids of the body are kept in a state of moderate distention, and that when this distention is removed, the parts slowly contract, but in a manner which more resembles the re-action of an elastic body than the contraction of the muscular fibre, and I should therefore refer it either to the elasticity of the membranous matter, or to that of the muscular fibre itself, and not in any degree to its proper contractility, to which it seems to have no analogy. Haller accurately discriminates the power of contraction, which the muscular fibres possess in common with all other matter, and which he calls the general contractile power or the dead force, from its proper irritability, as differing both in its seat, its mode of action, and its effects[1]. This general contractile power or dead force seems evidently to be the operation of elasticity, and I conceive that there is nothing in the phenomena that have been ascribed to tonicity, which may not be referred to the same power.

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[1] Fordyce, in *Phil. Trans. for 1788*, p. 30 et seq.

Among the different modifications of contractility which are pointed out by Bichat, I regard that which he styles contractility from structure in the same point of view, as an effect of elasticity, whether residing in the membrane or in the proper fibre, but as having no relation to the Hallerian irritability, of which, indeed, he appears to be himself well aware. I shall not at present follow this author through all his complicated arrangement of the different species of contractility, as the propriety of his divisions depends very much upon his general views concerning the nature of life, which will be better understood when we are further advanced in our subject.

Besides the specific property of irritability, many physiologists ascribe to the muscular fibre a degree of sensation, and even Haller himself, to whom so much merit is due for the sagacity with which he has discriminated between the powers of the muscles and the nerves, employs expressions from which it might be conceived that he considers the *vis nervea* as an actual property of the muscular fibre itself, as well as its irritability, or *vis incita*, as he styles it[2]. The vis nervea of Haller is that power in the muscular fibre which enables it to receive impressions conveyed to it by the nerves; but this supposed vis nervea ought not to be regarded as a function of its contractility, for in fact it is nothing more than the power which the fibre possesses of receiving the impressions that are made upon it, so as to cause it to contract, the impressions that are conveyed to it by the nerves being only one among the other stimulants which produce its contractions. Bichat supposes that the muscular fibre possesses proper and inherent sensibility, but the muscular sensibility differs from the vis nervea of Haller, as Bichat ascribes to muscles an actual degree of feeling which does not seem to differ essentially from the sensibility residing in the nerves. The doctrine which was so warmly contended for by the antagonists of Haller, and which may, perhaps, be regarded as the most popular at the present time, is in its essence precisely the reverse of this opinion, although they are frequently confounded together. According to the neurologists, as they have been termed, where a stimulant acts upon a muscular part, the immediate action is not upon the fibre itself, but always, in the first instance, upon the nervous filaments connected with it; but the further discussion of this question, as well as the nature of sensibility itself, and the distinction between this power and contractility, must be referred to the chapter on the nervous system.

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[1] El. Phys. xi. 2. 1..3.

[2] El. Phys. xi. 2. 15.

I must remark in this place, although the subject will be more fully considered hereafter, that muscular contractility is much influenced by many of those faculties or functions which seem to be intermediate between the corporeal and mental parts of our frame. Thus the power of volition is exercised most conspicuously in every thing which is connected with muscular contractility, and indeed is the grand theatre on which its effects are manifested. The operations of habit and of sympathy are also sufficiently obvious, but in these cases it is doubtful how far the effects are the result of the immediate action of the muscular fibre itself, independently of the intervention of the nervous influence[1].

SECT. 4. *Use of Muscles.*

Having given an account of the form and structure of muscles, of their chemical composition and their properties, I now proceed to consider their uses. The general use of the muscles is sufficiently obvious; they are the great organs of motion, both of that by which the body is moved from place to place, constituting loco-motion; that by which each of its separate parts is moved, when we act upon the contiguous bodies in our intercourse with the external world; and that by which many of the various minute actions are performed, which are essential to the exercise of the vital functions. In short, muscular motion seems to be concerned in almost every operation that is produced, either by the system at large or by its individual parts[2]. All these effects are brought about by the simple act of contraction, or that by which the fibres shorten themselves, and by approximating the ends of the muscles, draw together the parts to which the ends are attached. Nor is the operation of the muscular coats less important than that of the proper muscles, although it is less obvious to the eye. Here the fibres, not being collected together into large masses, which act simultaneously, the result of their contraction is not the movement of any particular part, but they act, as it were, fibre by fibre, producing in the organ to which they are attached a peculiar kind of undulatory, or, as it has been termed, vermicular motion, from its resemblance to the crawling of the worm, which serves to keep their contents in a state of perpetual agitation, to mix them intimately together, and ultimately to produce their expulsion.

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[1] We have some useful observations on the nature of loco-motion in Gerdy's treatise on the phenomena of life; he considers voluntary motion as consisting of four stages; 1. The transmission of volition from the brain to the muscle by the nerve; 2. The vital contraction of the muscle; 3. The mechanical tension of the fibrous parts which unite the bones; and lastly, The mechanical movements of these and of all the parts connected with them; p. 4.

[2] Adelon arranges all muscular motions into seven classes; those which serve for the preservation of the posture, those for progression, for prehension, those connected with the organs of sense, with expression, with the nutritive functions, *and with generation; Physiol. t.* i. p. 71.

The particular operation of the muscular coats will be more fully explained when we come to that part of our subject which treats of those functions that depend immediately upon their action; but before we quit this subject it will be necessary to inquire into a point, which has often been discussed, whether all the spontaneous motions that are observed in the body are to be referred to contractility, under one or other of its species? We know that both elasticity and gravity are the cause of motion in the body, but they are sufficiently distinct from contractility; the question is, therefore, whether motions which do not appear to depend upon any of the usual physical powers of matter are all to be referred to contractility? The difficulty which exists in this case is, that there are certain very obvious motions of particular parts, where no muscular fibres have been detected, and yet where the quantity of motion and the size of the part is so considerable, that we might have supposed that the fibre would have possessed a sensible magnitude. The iris was long supposed to be an organ of this description, in which, although the motions were rapid and considerably extensive, yet for a long time, the most skilful anatomists were not able satisfactorily to demonstrate the existence of muscular fibres[1]. Blumenbach attempted to solve this difficulty by saying that the iris and some other parts, which are in the same predicament, possess a peculiar power, which he calls their vita propria, and that this is the cause of their contraction[2]. But when we come to consider this hypothesis, it will appear to be merely a form of speech which throws no light upon the phenomena or upon their cause, and does not tend to generalize analogous facts, but which forms a part of that system of obscure causes, which is too often had recourse to by physiologists, when they are at a loss for a rational explanation. Nothing, however, can be more injurious to the progress of science than this method of substituting new words for new ideas, and of advancing hypotheses which, when we come to examine into their real foundation, must be regarded as merely verbal. Upon the whole, we may conclude, that when we perceive any motions which seem, in all respects, to agree with those which are the evident result of muscular contraction, it may not be unreasonable to conjecture that they depend upon the same cause, even although we are not able to detect the apparatus by which they are produced[1].

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[1] The question respecting the muscular fibres of the iris is decided in the affirmative by the microscopical observations of the accurate and indefatigable Mr. Bauer; Phil. Trans. for 1822, p. 78. See also the still later observations of Mr. Jacob, Med. Chir. Trans. v. xii. p. 514. I may remark that his beautiful engravings do not appear on all points quite to correspond with the magnified figures of Mr. Bauer. Berzelius also informs us that the iris has all the chemical characters of muscle; View of Animal Chemistry, p. 86. Another consideration which might be alone sufficient to prove the muscularity of the iris, is deduced from the fact, that in certain individuals the motion of this part is under the control of the will; we are informed by Dr. Roget that this is the case with his eye; Travers's Synopsis of the Diseases of the Eye, p. 72.

[2] *Inst. Phys.* § 42 and 273.

SECT. 5. *Mechanism of Muscles.*

In considering the mechanism of muscles, we must bear in mind that their action consists essentially in the approximation of their extremities, in consequence of the shortening of their fibres, and that the immediate effect of this is to move any body to which the ends are attached. It, however, generally happens that one of the ends is connected with some fixed point, while the other is much more moveable, and of course the bone or other solid body which is attached to it is moved in the same manner. In order to promote the symmetry of form and the facility of motion, we find that, in many cases, the flesh of the muscle itself is not inserted into the body which is to be moved, one or both of the ends terminating in membrane, which, according to the situation or use of the part, is either condensed into a strong cord, constituting a tendon, or spread out into a membranous expansion.

Although a very slight knowledge of the structure and functions of the body would render it obvious that the muscles are the great instruments of its motions, yet no accurate conception of the mode in which they operate seems to have been entertained before the publication of Borelli's celebrated work on the motion of animals[2]. This writer very ingeniously referred the action of the muscles upon the bones and solid parts, to the effect of a mechanical power acting upon a lever. He reduced his doctrine to a mathematical form, pursued his idea through all the organs of the body, and clearly explained the mode in which every individual action is produced, in a most elaborate and minute detail[3]. Winslow may also be mentioned as among the first of those who presented a clear and correct idea of the subject, which, although posterior to that of Borelli, may, in some respects, be considered as deserving of even more commendation, because it has the merit of not being clogged with any fanciful hypothesis, which is unfortunately the case with that of the former writer.

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[1] It may be objected to this conclusion, that many of the zoophytes exhibit very extensive motions, where no proper muscular structure has been detected, even by the most powerful microscopes. But it is difficult to extend the analogy to animals, which differ so much in their form and functions; we are moreover informed, that Ehrenberg has detected muscular fibres in some of the rotiferæ; Roget's Bridgewater Treatise, v. i. p. 189.

[2] De Motu Animalium.

[3] Although Borelli has been generally supposed to have established his theory of the mechanism of muscular motion, it may be proper to remark that it has been called in question by Barthez; see Art. "Barthez" in Suppl. to Encyc. Brit.; also Journ. de Phys. t. xlvii. p. 271.

The fixed points of the body, from which motion commences, or against which the muscles re-act when they begin their contractions, are generally the bones, and the motions are performed by the intervention of joints. Considering the bones, therefore, as being acted upon by the muscles after the manner of levers, the part where the muscle or tendon is inserted into the bone will represent the power, the joint the fulcrum, and the part that is moved constitutes the weight. Writers on mechanics have divided levers into three kinds, according to the relative position of their three essential parts; the weight, the power, and the fulcrum. Those of the first kind have the fulcrum in the centre; in those of the second kind the weight is in the centre; while in the third the power is in the centre; the bones are of this last description, in which the power is placed between the fulcrum and the weight. The motion of the fore-arm may be taken as an example of the effect of muscular contraction and the manner in which it is produced. When we wish to raise a weight by bending the elbow joint, it is effected by muscles situated below the shoulder, which have tendons inserted into the top of the bone of the fore-arm near the elbow[1]. The consideration of the manner in which the muscle acts in this case, proves that the mechanism of the animal body is calculated to produce a great loss of absolute power. It is an established position in mechanics, that in the action of levers, the power is to the weight as the distance between the weight and the fulcrum is to the distance between the power and the fulcrum. In the present case, therefore, a small part only of the power of the muscle is exerted in raising the weight, the rest being expended in acting against the disadvantage of the position. We shall, however, find it to be a general fact, or, as it is termed, a law of the animal œconomy, that muscular power is always sacrificed to convenience. Had the object been to raise the weight with the least possible power, the muscle would have been placed on the fore-arm, and the tendon inserted into the lower part of the shoulder-bone, but in this case the awkwardness of the limb would have much more than counterbalanced the supposed advantage of the saving of muscular power. The remark applies with still greater force to the fingers. At present they are moved by the contraction of muscles placed on the fore-arm, and are connected to them by long delicate tendons which pass over the wrist and hand. But if this order had been reversed, and the flesh of the muscle had been placed on the fingers, the hand would have been almost useless from its clumsy form.

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[1] Winslow, sect. 3. art. 5; and Mém. Acad. pour 1720. See Cloquet's Man. pl. 61, for an account of the different kinds of levers as applicable to the action of the muscles.

Another important advantage which arises from the present construction of the muscles, as consisting of levers, where the power is situated near the fulcrum is, that we acquire a great degree of velocity. This will be sufficiently obvious by reflecting on the mechanical disposition of the parts, and, as Paley judiciously remarks, there are many more cases in which it is useful to raise a small weight rapidly than a large one slowly[1].

Besides the loss of power which is occasioned by the nature of the lever, in consequence of the power being applied nearer the fulcrum than the weight; there are other circumstances in the construction of muscles which produce the same effect, by which there is a very considerable expenditure of absolute power; but in this, as in the former case, this loss of power is always attended with some very obvious advantage. Most of the muscular fibres are so placed as to act obliquely, and it is well known that by this arrangement a quantity of power is lost, in proportion to the degree in which the direction of the fibres differs from that of the moving body. But what we in this case lose in power we gain in the saving of the quantity of contraction. It is obvious that the antagonist muscles will be less stretched, that there will be a less displacement of parts, a less degree of pressure upon the vessels and nerves, and that less distention and straining of the membranous matter will ensue, the smaller is the degree of contraction of the fibres, and the less alteration the muscle consequently experiences in its general form. In pursuance of the same principle we may remark, that the extent of action in a muscle is necessarily in proportion to the length of the fibre, for it is obvious that a long fibre will have to diminish in a less proportion than a shorter one to produce the same degree of absolute contraction[2].

A third source of loss of power depends upon the situation of the muscles with respect to each other, an action being seldom performed without the concurrence of two or more muscles to the same effect. In this case, not only the fibres must act in an oblique direction, but the action of each of the muscles must, in some measure, oppose each other. Here is a loss of power from what is styled by mechanicians the composition of forces, but, as in all the former instances, the present construction is attended with many important advantages. When two or more muscles act upon the same point, the effect will be to draw the body in the diagonal; and we consequently have it in our power to alter the direction of the motion with great ease and accuracy, by throwing, at pleasure, a little more or less energy into one or other of the muscles, and drawing the body into any of the intermediate positions. Thus a great variety of motions may be produced by two muscles only, and the body is less liable to feel fatigued in any one part by the exertion being, as it were, diffused through a larger space, and a less quantity of it being required at each single point.

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[1] Natural Theology, p. 141.

[2] Winslow's *Anat. sect. 3, art. 1, § 54.*

A fourth circumstance connected with the mechanical construction of muscles, which proves a source of the loss of power is, that the tendon is generally inserted into the bone at an acute angle, whereas, in order that the power should have operated to the most advantage, it ought to have acted upon the lever in a perpendicular direction. Upon the same principle, power is also lost by having the muscular fibres inserted obliquely into the tendons; but although power is thus sacrificed, it is obvious that the present arrangement is much more commodious; and indeed, in many cases, it would have been impossible for the muscles to have acted perpendicularly upon the bones, or to have been differently inserted into the tendons without a total change in the form and arrangement of all the body.

A fifth cause by which muscular power is lost arises from the circumstance of the two ends of the muscle pulling against each other. This is obviously the case where both ends are moveable, and where one end is fixed, as much force is expended on this as on the moveable extremity, and before this latter can produce any effect, it must counteract the resistance offered by the former, and in this case exactly half its mechanical power is lost before the motion commences. Besides what have been mentioned, physiologists have pointed out other circumstances which, in like manner, cause a loss of absolute power, but where this loss either necessarily arises from the nature of muscular contraction, is essentially connected with the form of the body, or is compensated by some obvious advantage.

Amidst so many examples, where muscular power is expended for the purpose of producing some important benefit to the system, there are a few instances of a contrary kind, where the parts are evidently formed for the purpose of assisting muscular action. The heads of the bones into which the tendons are inserted, not unfrequently swell out into a rounded projection, by which means the muscles act upon the bone at a less oblique angle, and this we observe to take place more particularly in those cases where the greatest exertion of muscular power is required, as in the muscles of the trunk and the lower extremities. For the same purpose some bones are provided with processes of considerable length, which seem to be solely intended for the insertion of muscles, and the same appears to be the principal object of the small detached bones, which are occasionally found near the joints, as the patella and the sesamoid bones. These, however, can only be regarded as exceptions to the general rule, and in all cases we perceive the operation of the principle which was stated above, that the quantity of power employed appears to have been no object in the construction of *the body,* but that it is always sacrificed, without any reserve, either to general convenience, to the symmetry of the form, to the gaining of velocity, or to the saving of the extent of contraction. The advantages which arise from the velocity of our movements, from the facility with which we can alter their direction, and from the connexion of the muscles to the moving points, by means of tendons, which, like ropes, serve to convey the force from the point where it is actually generated, to the part where it is wanted to be employed, are so obvious as to require no further illustration. It is not, however, so evident what is the advantage to be gained by the saving of contraction; yet so much attention appears to have been bestowed upon this point, that we must suppose there to have been some urgent reason for it. As the intimate nature of muscular contraction is itself unknown, it is scarcely to be expected that we should be able to give a satisfactory solution of this difficulty; for it does not obviously depend, like the former circumstances, upon any general principles of mechanics, but upon something specific in the nature of muscular contraction. I shall, however, hazard a conjecture upon the subject, after premising that it is merely to be regarded as such, and must therefore be maintained no longer than it appears to be countenanced by the phenomena, or serves satisfactorily to explain them, without violating any established principle or well-ascertained facts.

The only conception that we can form of the contraction of the muscular fibre is, that it consists in the approximation of the individual parts of which it consists, whether it be of the whole fibre, or of some of its constituents, which give it its specific properties. This attraction does not seem to bear any resemblance to the attraction of gravity, or to that of chemical affinity, the one operating upon large masses of matter, the other upon the separate particles of which they are composed. The contraction of the fibre appears to differ from them both in its causes and in its phenomena, and we may therefore suppose that it is essentially different in its general laws and modes of action. The force of the attraction of gravitation increases as the distances decrease, and this is probably the case with chemical attraction, but it would appear that the attraction of contractility has not this property, or at least that this property, if it exist, is counteracted by other circumstances. Of these circumstances one is sufficiently obvious, the re-action of the membranous matter attached to the muscular fibre. Whether this be the only cause, or whether there be others that operate, whether the attraction between the particles of the muscular fibre increase with the decrease of distance, whether it be the same at all distances, or whether it may not even decrease in the direct ratio of the distances, are questions that it is entirely beyond our power to answer. But this is certain, that as the particles approximate, the membranous matter will be compressed or bent out of its ordinary situation, and that this compression will *increase as the distances* diminish; that, therefore, as the degree of contraction increases, a greater force will be required to continue it, and still more to go on augmenting it.

This view of the subject would reduce the effects of muscular contractility to an attraction between the individual particles of the fibres, which is counteracted by the elasticity of the membranous matter that is connected with them. Whether this attraction, like that of gravity, increases as the distances decrease, and is counteracted in its operation merely by the elastic nature of the membrane, is a point upon which I do not pretend to offer any opinion. We can only say that the membrane must be compressed, and in proportion to the increase of compression, so will be the necessity for an increase of power to continue the contraction.

In speaking of the animal fabric we are obliged to employ terms derived from the workmanship of other bodies. We therefore speak of the sacrifice of power and of its expenditure, as we should do in a machine of any description, where the object of the engineer is to œconomize labour. But no idea of this kind ought to attach to our conception of the human body, where a certain construction of parts was adopted, as being the most useful for all the purposes of life, and those powers are assigned to it which are necessary for preserving it in its proper condition. Muscular contractility is one of these powers, and of course the proper quantity was given for the due performance of the functions that depend upon it.

Having given an account of the mechanism of muscles and the nature of their operation, it remains for me to make some remarks upon the force, the velocity, and the extent of this power. From the observations that have been made above, upon the quantity of force that is expended in order to produce a certain effect, we may conclude that the absolute force of muscular contraction, the power which the fibres actually exert, is exceedingly great. The mechanical physiologists, and especially Borelli, attempted to estimate the degree of this force, and although we cannot place implicit confidence in their estimates, as they proceed, in some measure, upon hypothetical and erroneous principles, yet we may allow that they are sufficiently correct to assure us that it exceeds very much any idea that we should have previously formed respecting it. According to his estimate the flexor muscles of the thumb possess a power equal to nearly 4,000 lbs., which may be considered as at least 100 times greater than the actual power which they are capable of exercising[1].

There are many cases in which the velocity of muscular contraction is no less remarkable than its force. As an example of this the muscles connected with the organs of speech are often adduced, where, in rapid enunciation, the number of distinct contractions that take place, in order to form certain combinations of vocal sounds, is very great, each word, or rather each syllable, requiring several different contractions, which must succeed each other in rapid succession, with proper intervals between them. The motions of the muscles connected with the fingers, in playing upon musical instruments, is no less remarkable, for here the contractions are generally greater in extent, and therefore must proceed with proportionably more velocity, although they do not succeed each other so rapidly as those of the vocal organs.

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[1] Prop. 126; the estimate of Borelli is, however, conceived by Pemberton to be considerably exaggerated; see Introduction to Cowper's Myotomia *Reformata.*

The extent of muscular contraction, or the degree in which any particular muscle is capable of shortening itself, has not been very accurately ascertained. In the proper muscles it has been thought that the fibres never diminish to more than one-third of their natural length, and even this must be considered as a remarkable case. In the muscular coats indeed the contraction of the whole substance is much greater, but then it is not ascertained what portion of it belongs to the proper fibre and what to the membrane. It has been remarked that some of the lower tribes of animals, as the polypi and the actiniæ, appear to contract their limbs to a much greater degree, and there is every reason from analogy to believe that they are provided with organs for the purpose of motion, which may be considered as muscular; but scarcely enough is known concerning their nature to enable us to employ them as the basis of any calculation that we may form upon the subject.

SECT. 6. *Hypotheses of Muscular Contraction.*

After this account of the structure of the muscular fibre, of its chemical nature, its properties, uses, and mechanism, I come, in the last place, to consider the hypotheses that have been invented to explain its action. Two distinct questions here present themselves; first, What is the efficient cause of the contraction of the fibre, or by what physical cause is it produced? and secondly, What is the cause of contractility, or of that property of the fibre which produces contraction? These questions have generally been confounded together, or have been considered as involving only one subject of inquiry, and yet I apprehend they are clearly distinct, and that we may conceive it possible to afford a satisfactory answer to one of them, without our being able to solve the other. We may perhaps discover something in the mechanical construction of the fibre, in the arrangement of its particles, or in the mode in which its constituents are connected to each other, which may explain to us the reason of their alternate contraction and relaxation, upon the application of the appropriate exciting cause; or, on the contrary, without being able to accomplish this object, we may perceive the correspondence between the operation of some external circumstance and the action of contractility, which will warrant us in regarding this as the cause of the effect. Both the above questions are highly interesting, but they are unfortunately both of them of very difficult solution. With respect to the efficient cause of muscular contraction, it may be remarked generally, that every attempt to account for it on the principles of mere mechanics must be obviously abortive, because in the operation of the muscles we have an actual generation of power. In the best contrived machinery we have only the existing power applied in a new direction, better adapted for some particular object; but power is never actually generated. In those engines which act from the mere force of gravity, what we gain in power we lose in velocity, or the reverse; and when from the re-action of an elastic body, as from the recoiling of a spring, there seems to be a real production of power, the effect thus apparently produced is no greater than was originally employed in compressing it, and its effect is necessarily limited to a short period, for when this power is expended all motion ceases. On this account it will be unnecessary for us to enter into any detail of those hypotheses which attribute muscular contraction to any mechanical construction of the fibre; as that of Borelli, who supposed that it consisted of a series of rhomboidal vesicles, which were in some way made to expand when the muscle contracted, and to collapse when it was relaxed, an hypothesis which he laboured with much care and supported by a long train of mathematical reasoning. Or the hypothesis of Steuart, which is framed with a great appearance of learning and geometrical precision, and which supposes that the muscular fibre is composed of a string of vesicles formed of the substance of the nerves, which, during muscular action, are inflated by the ingress of the nervous fluid. Nor shall we find the still more elaborate hypothesis of Keill, which assumes the vesicular structure of the fibre, and supposes the vesicles to be inflated by some peculiar action of the nervous fluid on the blood[1], to throw any light upon the subject, notwithstanding the learning and ingenuity which he bestowed upon it, and the mathematical precision with which its various parts are adapted to each other. It is sufficient to remark concerning them, that the whole rests upon a supposition which is not countenanced by a single direct fact, and that should we admit the vesicular form of the fibre, we are still as much at a loss as at first to know in what manner it becomes distended, and shall have to call in some new agent to perform this part of the operation. Nor shall we find a better explanation of the efficient cause of muscular contraction by having recourse to any chemical operation, such as the production of a gaseous body, which was a favourite notion with the physiologists of the seventeenth century, who supposed that there was an effervescence excited in the muscle. This effervescence was attributed to various causes; by some to the mixture of an acid and an alkali, which were imagined to be brought together in some mysterious manner, while others, and those among the most learned and ingenious men of the age, such as Willis, Bellini, Mayow, and Keill[1], ascribed it to a fermentation or effervescence, excited by a union of the particles of the muscular fibre with the nervous fluid, or with some etherial spirit contained in the blood. When electrical phenomena began to be attended to, it was supposed that the fibres of the muscle might be disposed in such a manner as to form a kind of battery, which should produce contraction by its explosions; and after the discovery of galvanism, an elaborate attempt was made by Valli of Pisa, to account for muscular action by supposing that the muscles consisted of an arrangement of parts analogous to that of the elements of the galvanic pile[2]. It will be quite unnecessary to enter upon any formal examination of these hypotheses, which are now completely discarded; it is sufficient to observe concerning them that they are not supported by any foundation of facts, that they have scarcely any analogies in their favour, in short, that they were purely gratuitous, and could never have been tolerated, had not the mind been disposed to listen to any thing which promised to throw the smallest ray of light upon a subject that was involved in so much obscurity.

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[1] Tentamina, No. 5.

There is much more simplicity and less violent improbability in the hypothesis of muscular contraction that was advanced by Prochaska, although, I fear, we must admit that it is equally without foundation. From his explanation of the structure of the muscles, he concludes that the minute branches of the arteries are every where connected with the ultimate muscular filaments, that they creep about them, and cross them in all directions. Hence he argues that when these vessels are rendered turgid by an accession of blood, in passing among the filaments they must bend them into a serpentine form, and thus diminish their length, and that of the muscle generally[3].

In connexion with this hypothesis of Prochaska's, I think it not improper to notice a microscopical observation of Hales's, which, although it may have been perverted by the causes which are so apt to affect all observations of this kind, comes from too respectable a quarter to be entirely neglected. He informs us that when he viewed the muscles of a frog with a powerful lens, he observed the fibres lying parallel to each other, with the blood running up and down between each fibre in the small capillary arteries. If the muscle was then made to contract, to use his own expression, "the scene is instantly changed from parallel fibres to serieses of rhomboidal pinnulæ, which immediately disappear as soon as the muscle ceases to act."[1] We may easily conceive that an appearance similar to what Hales describes would follow from the shortening of the fibres and the necessary contraction of all the parts connected with them, but it does not throw any light upon the nature of the operation, nor does it enable us to judge whether the fibres or the vessels were the prime agents in the production of the change.

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[1] See especially his Tentamina, No. 5.

[2] Experiments on Animal Electricity, with their Application to Physiology, by E. Valli; See Brit. Crit. for Mar. 1794. Jour. de Phys. t. xli. contains several letters of Valli on the subject.

[3] De Carn. Mus. § 2. c. i.

The only other opinion which I shall notice on this subject is the one that was brought forward a few years ago by Sir G. Blane, and that indeed more in the form of a conjectural speculation, than of a formal hypothesis. As he was led from his experiments to conclude that the actual bulk of the muscle is not altered during its contraction, but that it gains in thickness exactly what it loses in length, he observes that we may account for this change by supposing that the muscle is made up of particles of an oblong form, and that when the muscle is contracted, the long diameter of the particle is removed from a perpendicular into a transverse direction[2]. This speculation has certainly the advantage of containing a much smaller number of assumptions than most of those alluded to above, but its foundation is equally gratuitous, and, like that of Prochaska, seems totally inadequate to produce the effect in question.

The only remaining hypothesis of muscular contraction, which requires to be noticed in this place, is the one which has been recently brought forwards by Prevost and Dumas. It rests principally upon their alleged discovery of the structure of the muscular fibre, and of the relation which it bears to the nerves connected with it. They inform us, that when the fibres are observed in their quiescent state, they are seen to lie in right lines parallel to each other, but that when they are viewed under the influence of a stimulus, or in the state of contraction, they assume a zig-zag, or waved appearance. The flexures or angles are found always in the same part of the fibre, and it was therefore concluded, that they must depend on some fixed or permanent cause, and the cause assigned is the presence of a series of minute nervous filaments, which are stated to intersect the muscular fibre at right angles, and at short distances from each other.

Upon this foundation, which, so far as the mechanical arrangement of the muscular and nervous fibres is concerned, appears to be partly, if not in a great degree hypothetical, is erected the theory. It is supposed, that in every case of muscular contraction, electricity is set in motion, that it is conducted more readily by the nerves than by the muscles, that it consequently passes through the parallel nervous filaments, which cross the muscular fibre, that it establishes an attraction between these nervous filaments, and thus causes the muscular fibres to assume the angular or zig-zag form, which constitutes its contraction[1]. I conceive that my readers will agree with me in the opinion, that before this hypothesis can be assumed, we have many points to ascertain, both of theory and of fact, and many difficulties to remove, and that perhaps after all, the principle of referring contractility entirely to a mechanical condition of the part, incident to the passage of electricity through it, is scarcely consistent with our conception of this power, as a specific property, exclusively attached to the muscular fibre[2].

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[1] Statical Essays, vol. ii. p. 59.

[2] Select Dissert. p. 243.

After these melancholy examples of failure before our eyes, it will not be expected that I should attempt to unravel a mystery, which has hitherto remained in such impenetrable obscurity. It may, however, be desirable to state in what degree the efficient cause of muscular contraction is a legitimate object of inquiry, and towards what points we ought particularly to direct our attention. In the first place, the simple act of contraction must consist in the approximation of the particles of which the fibre is composed, and this may be brought about in various ways. The fibre itself may be condensed in its whole substance, or it may be bent or folded up into a kind of zig-zag form, or the attraction between some of its parts may cause the whole to be corrugated, thus shortening it in its perpendicular direction without producing any actual condensation. But I think I may venture to assert, that we have no proof, either from the evidence of our senses or from any correct deduction of reasoning, of any specific structure or constitution of the fibre which can, in any degree, explain the manner in which this approximation is effected. It is not likely that any further discovery can be made upon this subject by the aid of microscopes, for it appears that there is a limit to the employment of high magnifiers, beyond which the liability to ocular deception is so great, as to counterbalance any supposed advantage from the increased magnitude of the object. If therefore, any additional information can ever be acquired on this point, it is more likely that it will be done by observing the effects produced on the whole muscle, and by tracing the analogy between these effects and other natural phenomena, than by the mere examination of the separate fibres. And if we are unable to account for the approximation of the particles, still less are we able to explain why the various things which we call stimulants, so extremely heterogeneous in their nature, and which have no other common property, should all coincide in producing the same effect upon the fibre. This is so unlike the operation of any other physical cause with which we are acquainted, that we must for the present consider it as an ultimate fact, one of those mysteries in nature, which daily present themselves to our observation, but which elude all our attempts to refer them to any more general principle.

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[1] Edwards, De l'Influence, &c., Appendix; Cloquet's Man. pl. 60.

[2] See the remarks of Raspail, p. 260..2, on the "Mechanism of muscular contraction," in which he offers some strictures on this hypothesis.

The other inquiry which I proposed, what is the cause of contractility, remains involved in as much obscurity as the one we have been considering. The attempts that have been made to explain it are not less numerous than in the former case, nor can they be considered as more fortunate, although they may probably appear less palpably absurd. Before we enter upon the inquiry it will be proper to have a clear and explicit statement of its object, a circumstance which is always necessary in philosophical investigations, but which seems to be particularly so in this instance, where several hypotheses have been advanced, which in fact appear to be no more than mere verbal explanations, or peculiar expressions which do not convey any distinct idea to the mind. Our object is to inquire whether, when a muscle contracts in consequence of the application of a stimulus, this event is uniformly preceded by any other event, so that the latter may stand to the former in the relation of its cause. This necessary antecedent to contraction may be something of a peculiar and specific kind, or it may be referable to some of the other agents in nature. If it be of the former description, we are to prove that it is governed by appropriate laws, that cannot be referred to any other power, and we are to point out in what this specific difference consists. If it be found to belong to any of the known agents, we are to prove the reality of this connexion, to show that the effect never takes place without the presence of this supposed agent, that when this agent is present the power of contraction continues, and that an increase or diminution in the quantity or force of the agent is always attended by a corresponding increase or diminution of the contractile power.

It will be necessary for us to examine more in detail the hypotheses that have been formed to account for the cause of contractility than those concerning the efficient cause of contraction, because while the latter have been all nearly discarded and are generally neglected, the former are many of them of modern growth, are maintained by many living authors, are daily referred to by physiologists and pathologists, and are made the foundation of many topics both of speculation and of practice. We may arrange these hypotheses under two divisions; first, those which ascribe muscular contractility to the presence of some extraneous agent or power superadded to the animal body; or, secondly, those which ascribe it to some peculiar state or function of the body itself. The idea that contractility depends upon the presence of free caloric may be adduced as an example of the first, and, as an instance of the second, the opinion that contractility necessarily results from a peculiar chemical composition of the muscular fibre. The first class of hypotheses will not detain us long, because they have been brought forwards in a less formal shape, and because, being less clogged with obscure speculations, and being of a more palpable nature, they are more easy to refute. Because it was observed that there is a connexion between the temperature of an animal and the degree of its contractility, some physiologists have conceived that contractility depended immediately upon caloric, or the matter of heat interspersed in an uncombined state between the fibres. Others, perceiving how remarkably the muscles are affected by the electric fluid, supposed that this was the immediate cause of muscular contractility, and set themselves to invent different modes in which what they styled animal electricity might be generated. With respect to both these hypotheses we may remark that caloric and the electric fluid are found to be very powerful stimulants to the fibre, and it would appear, with respect to the first of them, that a certain range of temperature is necessary for the existence of the contractile state. Another opinion concerning the cause of contractility, which must be placed in our first division, was fashionable a few years ago, according to which the immediate cause of this property was ascribed to oxygen. It was conceived that oxygen is absorbed by the lungs during respiration, is carried by the arterial blood to the muscles, and gives them their contractile power. It was imagined that in various states of the system, and from various incidental causes, oxygen was absorbed and carried to the muscles in very different quantities, and that in proportion to the quantity their contractility was increased or diminished. This speculation, which appears to have been first formally brought forwards by Girtanner[1], and was zealously adopted by Beddoes, was applied by him very extensively to pathology, and was made the foundation of some supposed improvements in the practice of medicine. For a short time this doctrine obtained a considerable share of popularity, but when the first impression of novelty had subsided, and its real merits began to be canvassed, it was found to be built upon a set of entirely gratuitous positions, and was almost universally abandoned. It has, however, lately found a supporter in Richerand, a writer more remarkable for a popular air which he gives to his works, and for the liveliness of his imagination, than for the correctness of his judgment. He adds to it the additional speculation, that the union of the oxygen in the arterial blood and the elements of the muscle is brought about by the nervous fluid, which produces an effect something like that of the electric spark[2].

In the second class of hypotheses that have been formed to account for contractility, there is one that has been detailed with a considerable degree of minuteness, and has had a great variety of arguments, and even experiments, adduced in its favour; I refer to that which ascribes contractility to the chemical composition of the fibre. It is found that a certain proportion of chemical elements composes a body endowed with certain properties, and whenever the elements are put together in a proper proportion, these properties are the necessary result. Thus, a certain proportion of sulphur and oxygen forms sulphuric acid, a substance which possesses a set of qualities necessarily belonging to it: it is a heavy, unctuous, acid fluid, and we may correctly say, that these properties are necessarily attached to its chemical composition. The same reasoning is applied to the muscular fibre; this body is composed of carbon, hydrogen, azote, and oxygen, which, all of them, exist in a certain proportion, and when they are united together they form the body which we call a muscular fibre, which possesses a certain set of physical and chemical properties, and also the physiological property of contractility. Contractility is said, therefore, to be as much the necessary result of the chemical elements which compose the fibre, as acidity is of the compound of oxygen and sulphur which composes sulphuric acid. In order to prove this hypothesis by experiment, an attempt has been made to show, that if by any means an alteration be made in the proportion of the elements of which the fibre is composed, without, at the same time, destroying its texture, or its physical properties, a corresponding change is brought about in its contractile power. Humboldt particularly directed his attention to this point, and endeavoured to demonstrate that a very slight change in the chemical composition of the muscle entirely destroys its contractility, while, by restoring the original composition of the muscle, the contractility is also restored. As oxygen is the most variable of the components of the muscular fibre, or at least that which is the most easily added and subtracted from it, by means of chemical re-agents, his experiments chiefly consisted in observing the effects of this substance upon contractility, and by employing galvanism as a test of the presence of the contractile power, he found that it was perceptibly affected by very slight variations in the proportion of the chemical elements of the muscle[1]. There are some interesting experiments that lead to the same conclusion in the thesis of Smith[2], a work which, in consequence of its peculiar destination, has been little known to the public, but which contains more valuable and interesting matter than many bulky volumes that have acquired a high degree of celebrity. The experiments were performed about the year 1766, long before the chemical theory of contractility was thought of. Their immediate object is to show the effect of chemical agents in increasing or diminishing the power which the muscles possess of being affected by stimulants of various kinds, and in some cases their operations were such as rather to indicate a change in the composition of the muscle than merely in its contractility.

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[1] Journ. de Phys. t. xxxvii. p. 139.

[2] Elements of Physiol. § 163; see also Blumenbach's Inst. by Elliotson, § 50, 54.

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[1] Experiences sur le Galvanisme, &c. par Jadelot; also Ann. Chim. t. xxii. p. 51; Journ. Phys. t. xlvi. p. 465; t. xlvii. p. 65.

[2] *Tent. Phys. Inaug.* de Actione Musculari, Appendix.

A train of reasoning has been brought forwards by the chemical physiologists, in favour of their views of contractility, derived from the state of the muscular fibre after death, which is found to differ very much according to the mode in which life has been destroyed. If an animal in full health be suddenly killed, the muscles are firm and rigidly contracted, and they remain a long time without undergoing the process of decomposition; whereas, on the contrary, if death ensue after violent exercise, if it be caused by lightning or electricity, or by the operation of some kinds of poisons, the muscles are relaxed and soft, have lost all their contractility, and much sooner become putrid; and it is found that, in all instances, these two states or conditions correspond to each other, viz., the degree of contractility remaining in the muscle and its tendency to putrefaction. As the decomposition of the substance of a muscle is obviously a chemical operation, and as it thus appears to be so intimately connected with its contractility, it was concluded that contractility is the necessary result of a peculiar combination of chemical elements. This argument in favour of the chemical theory has been also extended to the connexion that has been observed between the contractility of the muscular fibre and the coagulability of the fibrin of the blood. The greatest part, if not all those circumstances which affect the contractility of the muscle, are found to produce a proportionate and corresponding effect upon the coagulation of the fibrin. But the fibrin of the blood, it is said, is a mere chemical compound; any change in its coagulability must therefore depend upon an alteration in its chemical composition, and as the muscular fibre exactly resembles it in its chemical composition, and as there is a strong similarity between their respective properties of contractility and coagulability, so it is inferred that the former must likewise depend upon a chemical combination. For the facts respecting the blood we are principally indebted to J. Hunter, who, however, brought them forwards with a very different, and even a directly opposite, view, to prove that the blood, in consequence of its exhibiting properties so analogous to those belonging to the muscular fibre, is the appropriate seat of vitality. This hypothesis will be discussed in its proper place; but in the mean time I may remark, that whatever conclusion we form concerning Hunter's speculation on the life of the blood, still it indicates an intimate and necessary relation between its physiological and its chemical properties.

It would appear, then, upon the whole, that there are some striking facts and strong analogies in favour of the chemical hypothesis, and they certainly go so far as to prove that there is a very intimate connexion between the chemical composition of the fibre and its contractile power. But they do not prove any thing besides this; *they demonstrate* that a connexion exists between the two circumstances, not that one is the cause of the other. Indeed, there is an obvious and well-known fact which is decisive against this supposition. A muscle immediately after death has the same chemical composition as during life, yet if life be completely extinguished, contractility is gone and can never be restored. If it be said that a chemical change in the muscle commences immediately after death, but that it is too slight to be detected, I reply that a change which is imperceptible cannot be adduced as the foundation on which to build an hypothesis. And were it even proved that this alleged change did exist, it might still be objected that the progress of chemical decomposition bears no proportion to the alteration in the state of the contractile power, the former commencing very slowly, and by almost imperceptible degrees; the latter proceeding rapidly, and in a short time having arrived at the utmost change which it ever experiences. Besides, although it be a less direct argument against the chemical hypothesis, and one that is merely analogical, yet it possesses considerable weight, that although there are many substances that possess nearly the same chemical composition with the muscular fibre, yet no other body in nature exhibits any property which is at all similar to contractility; it does not, indeed, resemble any other quality in nature, and although exclusively attached to a substance composed of certain chemical elements, it seems more natural to suppose that it is superadded to those elements, than necessarily resulting from their combination.

Another opinion respecting the cause of contractility, which has been frequently brought forwards, although scarcely in the form of a regular hypothesis, is, that this property depends upon the mechanical structure of the fibre. We observe a body possessed of a very peculiar arrangement, unlike every other in nature, and possessed of an equally peculiar property; it is therefore concluded that the property is the necessary result of the peculiar structure or arrangement of the parts of which the body is composed. But the answer that was made to the chemical theory applies to this with still more force: that long after the contractile power is extinguished by death the structure remains unaltered, and as any material alteration in this respect would be of a more palpable nature than the former, so we may conclude, with more confidence, that it has not taken place[1].

Many physiologists of the first eminence, among whom we may include Haller and Cullen, when endeavouring to account for the cause of contractility, have thought it sufficient to say, that this attraction between the particles of the fibre, which causes it to contract, is nothing more than a peculiar mode or species of the attraction which subsists between the particles of all matter. And this appears to be nearly the opinion of Fordyce, when he ascribes contractility to what he calls the attraction of life[1], and of those physiologists who speak of it as an attraction depending upon the operation of the vital principle. Many facts and experiments have been adduced in favour of the hypothesis of attraction, which all tend to show that there exists a greater degree of cohesion between the particles of the muscular fibre during life than immediately after death, and before we can conceive that it has experienced any material change in its chemical or physical constitution. Observations of this kind occur not unfrequently in the writings of the older physiologists; among the moderns, Sir G. Blane attempted to prove this diminished cohesion by a direct experiment on the muscles of the thumb[2], and remarks of the same kind have been made by Sir A. Carlisle[3] and by Bichat[4]. With a view to the same conclusion it has been observed, that when a muscle is ruptured during life it is the tendinous part which is disposed to give way, while, on the contrary, after death, the fleshy part is always weaker than the tendon[5]. Although perhaps it is impossible to perform any very decisive experiments upon this subject, yet we admit the truth of the position, that during muscular action the particles of the fibre are more strongly attracted together, and indeed the very nature of the operation almost necessarily implies that this must be the case. We must also bear in mind that mechanical violence is itself a stimulus, and that in consequence of the admirable arrangements of the animal œconomy, the very circumstance, which would otherwise tend to disorganize the muscles, has the immediate effect of preventing this disorganization from taking place. Yet, although we admit the existence of this increased attraction as an actual fact, it affords no explanation of the cause of contractility. The hypothesis, when stripped of the peculiar language in which it is conveyed, amounts to nothing more than the expression of the fact in new terms, for still the fundamental difficulty remains, what it is which determines the attraction between the particles of matter to exert their power in this peculiar manner and under these peculiar circumstances.

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[1] This view of the subject may derive some confirmation from the fact discovered by Hunter, that a muscle may have its fibres much shortened without any diminution of its contractility; Home's Lect. p. 40. I may also observe, that in many of the zoophytes, which exhibit the most decisive indications of contractility, no fibrous structure can be detected by the most *powerful* microscopes.

As I had occasion to give an account of the hypothesis of Prevost and Dumas on the efficient cause of muscular contraction, so I must now refer to a recent speculation, which has been brought forwards by Dutrochet, on the cause of contractility.

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[1] Phil. Trans. for 1788, p. 25.

[2] Blane's Select Dissert. p. 237.

[3] Phil. Trans. for 1805, p. 3; the author remarks that the diminished cohesion after death occurs only in the direction of the fibre.

[4] Anat. Gén. t. ii. p. 398.

[5] Carlisle, in Phil. Trans. *for 1805, p. 4.*

He commenced his researches by endeavouring to explain the mode in which the sap is distributed through vegetables, and conceives that he has, by this means, discovered what he terms "le mécanism secret du mouvement vital," which is equally applicable to animal as to vegetable existence. He traced the parts of the plant which contain the sap to the organ where the fluid appears to be admitted in the first instance, and by making a series of microscopical observations on the action of these parts, a singular phenomenon was detected. The minute fibres of these organs were seen to emit a number of globules, while the space which these globules occupied was filled up by an equal bulk of water. This singular effect the author ascribes to what he conceives to be a general principle, that is intimately concerned in the operations of both animal and vegetable life, according to which, whenever a cavity containing a fluid is immersed in another fluid, less dense than that which is in the cavity, there is a tendency in the cavity to expel the denser and absorb the rarer fluid. This is to be regarded as an ultimate fact, one which cannot be referred to any of the other known operations of nature: upon this new power Dutrochet bestows the name of *endosmose*, "dedans impulsion"[1]. The existence of this "physico-organic or vital" action having been thus detected in the spontaneous operations of nature, a series of experiments were undertaken for the purpose of illustrating its operation. The experiments consisted in filling membranous bodies, as the intestine of a chicken, with milk or some other dense fluid, and immersing it in water, when it was observed, that the milk left the intestine while the water entered it. And it was afterwards found that a reverse operation took place; if the internal fluid was rarer than the external, the transmission took place in the opposite direction, depending upon what is termed *exosmose*, "dehors impulsion." It appeared that in both cases the energy of the action was in proportion to the difference between the specific gravities of the two fluids; and also that, independent of their gravity, their chemical nature affected their power of transmission. The author here calls in aid of his hypothesis a very curious experiment, which was performed by Mr. Porrett[2], according to which, by a certain arrangement of the galvanic apparatus, water was caused to pass through a membranous substance; and, connecting this fact with the known operation of the electric currents in the decomposition and transfer of various substances, Dutrochet does not hesitate to draw the conclusion, that "l'endosmose et l'exosmose dependent entièrement de l'electricité."[1] The cavities in which these changes take place he conceives to be analogous to Leyden phials, having their two surfaces charged with the two electricities; the ultimate effect, or the direction of the current, being determined by the excess of the one over the other. The principle being once established, the application is comparatively easy. The turgidity of the minute extremities of the organized bodies causes the discharge of their contents, and the necessary entrance of the water from the action of endosmose. The turgidity, upon which the whole operation seems to hinge, is, however, effected by what is termed "adfluxion," which would appear to be a previous step to *endosmose*, and would seem to be dependent upon a vital action, different from any of those that are generally recognized. The result is the entrance and subsequent progression of the fluid; and the organ in which this double effect is produced is said to be "l'origine d'impulsion et but d'adfluxion"[2]. As was remarked above, the action of endosmose is supposed to be at least as important in the animal, as in the vegetable œconomy. It is a principal agent in the circulation, the action of the capillaries being supposed to consist, not in their contractility, but in endosmose. Endosmose is also the main agent in the action of the absorbents, and it is inferred that nutrition, secretion, and indeed all the functions by which a change is induced in the composition of the body, are, in a great measure, to be referred to this source. The above may be considered as affording a very brief, although I think not an incorrect, view of the leading points of Dutrochet's hypothesis. The foundation on which it rests is of so novel an aspect, while the application that is made of the new principle is so extensive and so important, that it appears very essential, that the observations and experiments be multiplied and confirmed, both by M. Dutrochet himself and by other physiologists, so that any doubt that may attach to it may be removed. Until this be accomplished I shall think myself justified in asserting that, in the present state of our knowledge, contractility ought to be regarded as the unknown cause of known effects, a quality attached to a particular species of matter possessed of properties peculiar to itself, and which we are not able to refer to any general principle[3].

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[1] L'Agent immédiat du mouvement vital, p. 115.

[2] The experiment of Mr. Porrett's here referred to, consisted in fixing a piece of bladder perpendicularly in a glass jar, so as to divide the jar into two cells; one of these is nearly filled with water, while the other contains a few drops only. A galvanic apparatus is then employed, so as to induce the positive electricity upon the larger quantity of water, when, after some time, it is found that a considerable portion of the water has transuded through the bladder, until the level of the fluid in the negative cell is higher than that in the positive cell; Ann. Phil. v. viii. p. 75, 6. The experiment is certainly a very curious one, but I apprehend that many additions will be necessary, before we can draw the inference from it which Dutrochet is inclined to do.

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[1] P. 139.

[2] P. 168.

[3] Since the publication of his first treatise, Dutrochet has considerably extended his experiments on this very curious subject, and has given his additional observations to the public in two papers in the Ann. de Chim. t. xlix. p. 411 et seq. et t. li. p. 159, and, still more lately, in a separate essay, Nouvelles Recherches sur l'Endosmose et l'Exosmose. The author controverts an opinion, which had been entertained by Poisson, that the phenomena might be referred to capillary attraction, and he also concludes that they are not

electrical. He describes an ingenious instrument, which he styles an endosmometer, by means of which he is enabled to measure the degree of effect produced under various circumstances, and by various chemical agents. These, he finds, differ considerably from each other, so as to render it highly probable, that the peculiar action is connected with their chemical constitution. We have a good detail of Dutrochet's experiments and hypothesis in the Ed. Med. Journ. v. xxxi. p. 369 et seq. See also Brewster's Journ. v. ix. and x.; Instit. Journ. v. ii.; Ann. Sc. Nat. t. xxiii.; and Dr. Hodgkin's Trans. of Edwards, p. 414 et seq. I must not omit to refer to Raspail's remarks, in his new system, p. lxii...lxv.; he appears disposed to refer the phenomenon observed by Dutrochet, to the aspiration and expiration which, according to Raspail, perform so important a part in the operations of vitality.

CHAPTER IV.

OF THE NERVOUS SYSTEM.

In the last chapter I gave an account of one of the appropriate powers of the animal body, contractility, and of the organs by which it is exercised; I now proceed to the other property that belongs exclusively to animal life, sensibility[1]. As contractility is always attached to the muscular fibre, so sensibility is always attached to the nervous system; it is found in no other part; and wherever the nervous matter can be traced, sensibility, in a greater or less degree, may always be detected[1].

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[1] One of the objections that were urged above against the use of the word irritability, to designate the appropriate power of the muscles, applies to sensibility, as expressing the power attached to the nervous system, that besides its technical and physiological sense, it is frequently employed in a general way, to indicate a peculiar state of the feelings or character. Were I to venture to introduce a new term, I should propose that of *sensitivity*, which might bear the same relation to the nervous system, that contractility does to the muscular. Cuvier, in his report on the experiments of Flourens, which will be more particularly noticed hereafter, remarks upon the ambiguity which, in the French language, attaches to the word "sensible;" this term being applied to a body capable either of receiving, of producing, or of conducting sensations. In English, part of the difficulty may be removed by employing the word "sentient" in the first, and "sensitive" in the third of these cases; but we have still a fourth, and that the most ordinary use of the word sensible, as expressing the state of the intellectual powers. This paper contains many valuable observations upon the importance of a correct nomenclature in the science of physiology. As an illustration of the inaccurate or indeterminate mode in which the terms connected with this subject are employed, I may remark that Helvetius supposes the mind, "la faculté de penser," to be entirely composed of "sensibilité physique" and memory; De l'Esprit, dis. 1. ch. 1; while Richerand conceives that there may be sensibility independent of the presence of nerves; Physiol. Inst. p. 22. Destutt Tracy, a writer of considerable acuteness, in his Elém. d'Idéologie, p. 26, lays down the position, that "penser, c'est sentir" either resemblances, perceptions, or desires, while p. 30, he defines sensibility to be the faculty by which we receive impressions and are conscious of them. He afterwards, ch. 11, endeavours to establish the position, that all the operations of the soul, "ame," are modifications of sensation. We have some useful remarks on the terms employed in describing the functions of the nervous system by Buzareingues, in the commencement of his Philos. Physiol. As further illustrating the same point, I may refer to Dr. Abercrombie's philosophical work on the intellectual powers, a work equally remarkable for the extent and the correctness of its information, sect. 1 et alibi; to Dr. Elliotson's Physiol. p. 25, 6, and to Rullier, art. "Sensations" and "Sensibilité," in Dict. de Méd. t. xix. The term *innervation* has been employed by some of the French writers very nearly in the same sense with the physiological sensibility of the English; see Adelon, Physiol. t. iv. p. 146, and his art. "Innervation," Dict. de Méd. t. xii. p. 299 et seq.; also Cloquet, Anat. t. iii.; but it has been scarcely so far sanctioned by general use, as to admit of its being substituted for the ordinary term. Gerdy, when he divides irritations into those that are perceived and those that are not perceived, may be considered as nearly approaching to the perceptions and the sensations, as stated above; Class. de phén. de la vie, p. 12. The 7th chapter of the 3d part of Dr.

In treating upon this subject I must begin by a brief description of the nervous system, and its anatomical structure, together with its physical and chemical properties. I shall, in the second place, consider its vital powers or faculties, and the mode of their operation, and this will naturally lead me to make some observations upon the use of the nervous system. I shall next inquire into the nature of the connexion that subsists between the muscles and the nerves, and I shall endeavour to distinguish between their effects upon the animal œconomy in general, and upon the different parts of which it is composed. When we have thus taken a view of both the powers which characterize animal existence, we shall be prepared for forming a classification of the functions, ascertaining in what degree they depend upon the muscular or the nervous systems, and what is the nature of the relation that they bear to each other[2].

SECT. 1. *Description of the Nervous System.*

The nervous system consists of four principal parts or organs;

Roget's Bridgewater Treatise, on Perception, contains much interesting matter; I must remark, however, that his nomenclature does not entirely coincide with that which is employed in the text.

[1] I have remarked above, p. 9, that Dr. Elliotson and Prof. Tiedemann extend the faculty of sensibility, as well as that of contractility, to vegetables; Physiol. p. 3; this, however, I cannot but regard as a premature generalization.

[2] Willis and Vieussers may be considered as the first among the moderns who investigated with much success the structure and functions of the nervous system. The former published his Cerebri Anatome in 1664; the latter his Neuralgia Universalis in 1685. Boerhaave paid comparatively but little attention to it: Haller again studied it with much minuteness; and since his time we may select the names of Vicq-d'Azyr, Sœmmering, the Wenzells, Gall and Spurzheim, Bellingeri, Rolando, Tiedemann, Flourens, Serres, Desmoulins, Foville, Magendie, Bell, Philip, and Mayo, as among the most distinguished of those who have investigated the structure and functions of the nervous system. Nor must we omit to acknowledge the obligations which we owe to Mr. Swan, for his elaborate engravings. An interesting, and, as far as I am able to judge, a very accurate abstract of the successive discoveries that have been made on the subject of the brain and nerves, is given by Sprengel, Hist. de la Méd. t. iv. sect. 12. c. 4. I may also refer my readers to the 8th section of Dr. Alison's physiology, and to his dissertation appended to the Cyclopædia of Medicine, p. 5, 6, for a judicious summary of the most important facts and opinions that we possess respecting the nervous system. Mr. Mayo's 9th chapter, "On the functions of the Nervous System," I consider as one of the most valuable portions of his work. For the comparative anatomy of this system, it may be sufficient to refer to the work of Carus; Gore's translation, v. 1. p. 262..277, and plate 19, where we have a description of the brain and nerves of the mammalia.

the brain, the spinal cord, the nerves, and the ganglia[1]. The brain is a body of a pulpy consistence, resembling a soft coagulum filling the hollow bone called the skull, which gives the form to the head. It is of an extremely irregular figure, having a number of projections and depressions, corresponding partly to the irregularities of the skull, and partly produced by convolutions and cavities in the brain itself. It is covered externally with various membranes, of which the most important are two, an external one, thick and dense, which lines the skull, and an internal one, more thin and delicate, which closely invests the cerebral mass, follows its surface into all its depressions and cavities, and conveys to the brain the numerous vessels that are distributed through it[2]. To these the older anatomists give the names of the Dura and Pia Mater, in conformity to a whimsical hypothesis, that these two were the origin of all the other membranes of the body. The internal cavities of the brain, which are called ventricles, are lined with a serous membrane, secreting an albuminous fluid, which in the healthy state of the organ, is removed by absorption as rapidly as it is produced, but which is occasionally collected in considerable quantity, giving rise to the formidable disease of hydrocephalus[3].

Much obscurity still subsists with respect to the use of the different parts of the brain, as well as of its various projections and depressions; but there are two points connected with its form, that it is important to notice, as they seem to be materially connected with its physiology and the nature of its functions. These are, first, the division of the encephalon or cerebral mass, into the cerebrum, or brain properly so called, and the cerebellum, or lesser brain; and secondly, its division into the two hemispheres. The greater part of the nervous matter within the skull composes the proper brain or cerebrum; it occupies the whole of the upper part of the head, and is separated by a dense membrane from the lesser brain, except at the common basis of both, where they are united. There is a dense membrane, projecting directly downwards to a considerable depth, from the upper part of the skull, and extending from the fore to the back part of the head, which divides the brain into the two hemispheres as they have been called; the cerebellum is likewise divided by a similar membrane into two hemispheres.

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[1] It may be proper to remark, that whenever the terms "nervous system," "nervous matter," "nervous power," &c. are employed, without any addition or restriction, they are to be understood in a general sense, as including or referring to all the four parts mentioned above.

[2] We have an excellent representation of this membrane in the first of Vicq-d'Azyr's plates, and in the 176th of Cloquet's (J.) Manuel. See also Cloquet (H.), Anat. p. 561 et seq.

[3] For the general representation of the various parts of the nervous system, the following works may be referred to, as deserving of our attention, either from their correctness or the beauty of the engravings; Vicq-d'Azyr; Monro on the Nervous System; Walter, Tabulæ Nervorum; Scarpa, Tabulæ Neurologicæ; Cloquet (Jules), Anatomie, t. ii. and Manuel, p. 151.. 176; Cloquet (H.), Anatomie, p. 527 et seq.; Laurencet, Anat. du Cerveau; Bell (C.), the Nervous System of the Human Body; Swan's Demonstrations of the Nerves; Mayo's Engravings of the Brain and Spinal Cord. Dr. Hooper's "Morbid Anatomy of the Human Brain," although indirectly connected with its physiology, may be properly noticed in this place, as containing a number of well executed representations of this organ in its various morbid conditions. It may be not uninteresting to compare the elaborate plates of the modern anatomists with the coarse, but not unexpressive woodcuts of Mondini; see his "Anatomia," p. 51 et seq. We have an ample catalogue of works on the nervous system by Foville, appended to his article "Encéphale," in Dict. de *Méd. et Chir. Prat. t.* vii.

When we cut into the interior of the brain we find it to be composed of two substances, that differ in their colour and consistence; these have obtained the names of the cortical or cineritious and the medullary matter. The cortical, as its name imports, is on the outside[1], and is of a reddish-brown colour; it is obviously of a softer consistence than the medullary part, and it leaves by desiccation a smaller quantity of solid residuum. In the fœtus it is considerably less firm, and at this period bears a larger proportion to the medullary matter than it does in the adult. It evidently contains a greater number of blood-vessels; and more may be brought into view, when it is examined by the microscope. On this account it was conceived by Ruysch to be composed entirely of blood-vessels, with the connecting cellular membrane, an opinion which was at one time very generally adopted, and to which Haller inclines[2], although the mere inspection of the part would seem to prove its inaccuracy. Malpighi supposed that he had detected a glandular structure in this portion of the brain[3], an idea which was embraced by many eminent anatomists, and which may be thought to receive some confirmation from the microscopical observations that have been lately made upon this organ[4].

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[1] This remark applies principally to the great bulk of the cerebral hemispheres; in many parts of the interior of the brain the order is reversed, or the two substances alternate with each other; Bell's Anat. v. ii. p. 28.

[2] El. Phys. x. 1. 12.

[3] Exer. de Cerebro, in Manget, Bib. Anat. vol. ii. p. 56.

[4] The medullary matter, both from its aspect and relative position, is generally considered as constituting the nervous substance in its most perfect state; and Gall and Spurzheim have conjectured that the use of the cineritious is to form or secrete the medullary part; Recherches sur le Système Nerveux, § 2. The particular facts from which they derive their hypothesis are, that the nerves appear to be enlarged when they pass through a mass of cineritious matter, and that masses of this substance are deposited on all the parts of the spinal cord where it sends out nerves. Prof. Tiedemann, however, remarks, in opposition to the above opinion, that in the fœtus, the medulla is formed before the cortex, and he limits the use of the latter to the conveyance of the arterial blood which may be necessary to support the energy of the more perfect nervous matter; Anatomie du Cerveau, par Jourdan, p. 128, 9, and by Bennet, p. 126, 7. The name of this author stands so high among the physiologists of the present day, that it is unnecessary to offer any eulogy upon whatever proceeds from his pen. His experimental investigations appear to have been pursued with persevering industry, so that there seems to be nothing wanting to complete our know-

To the base of the brain, connected with it by the intervention of the medulla oblongata[1], is attached what has been commonly called the spinal marrow, but which is more correctly termed, by the later anatomists, the spinal cord. Like the brain, it is enclosed in membranes, it possesses both cineritious and medullary matter, although their respective position is reversed; it has a longitudinal furrow, dividing it imperfectly into two halves, analogous to the hemispheres of the brain[2].

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ledge of those topics to which he has directed his attention, while his conclusions are formed with that cautious spirit, which seldom leaves any room for doubt or hesitation. I have inserted a short account of some of the most important parts of this work at the end of the chapter. A very ample analysis of it will be found in the Ed. Med. Journ. v. xxiii. p. 81..126. We have also an abstract of it in the Med. Repos. v. xv. p. 315 et seq. Foville, as the result of his late interesting researches into the physiology and pathology of the brain, is led to conclude, that the cortical part is the seat of its more active faculties, while the medullary matter serves principally as a conductor, and is more immediately connected with motion; art. " Aliénation mentale," Dict. Méd. Chir. Prat. t. i. p. 559; Phil. Mag. v. v. p. 337; Bright's Med. Rep. v. ii. p. 687; Prichard on Insanity, p. 226.

[1] Some of the later anatomists, and among others Flourens, Expér. sur le Syst. Nerv. § 1., include the medulla oblongata, as well as the cerebellum, under the general denomination of the brain. But I conceive it to be a more correct nomenclature, and one which will give rise to less confusion of language, to restrict the term to the cerebrum, and the cerebellum, and to consider the medulla oblongata as a distinct organ, as far at least as respects its anatomical relations. We have a brief, but clear and well digested account of the various parts of the nervous system, and their relation to each other, in Dr. C. Henry's report, read to the British Association, at their meeting in 1833, p. 50 et seq.; see also the art. " Encéphale," by Cloquet, Dict. de Méd. t. vii. p. 486 et seq. I may likewise refer to the portions of Dr. Craigie's " Elements," which treat of the brain, and those pages, 301 ..376, where he gives a minute account of this organ, and of the names which have been given to its various parts by the modern anatomists. The statical experiments of Sir William Hamilton; Ed. Med. Journ. v. xxxvii. p. 414..6; which are prefixed to Prof. Monro's late work on the anatomy of the brain, and still more the paper of Dr. Sims, on hypertrophy and atrophy of the brain, in Med. Chir. Tr. v. xix. p. 315 et seq., contain many important facts respecting the weight of the brain at different ages, &c. The brain of the healthy adult varies much in weight, but 2000 grs. appears to be about the average. Dr. Sims's remarks lead to the conclusion, that the weight of the brain is intimately connected with some important pathological deductions. His paper contains the result of 253 dissections.

[2] We are indebted to Prof. Bellingeri for a series of important researches on the structure and functions of the spinal cord. It is not a little remarkable, that although his works had been published for some years, they were, until lately, altogether unknown in this country, and probably also in France and Germany. They were introduced to our notice by an able abstract, which appeared in the 42d and 43d volumes of the Ed. Med. Journ. The treatise De Medulla Spinalis abounds in novel and curious matter; the following are some of the most important points which it announces. The central part of the cord is composed of cineritious matter, in the form of two segments of circles, convex towards each other, forming of course four projections, which are termed cornua; cap. 1. art. 1. p. 6 et seq., and tab. 1, 2, 3. The white or medullary matter, is in the form of six cords or strands, two anterior, divided from each other by a deep furrow, two posterior, likewise divided by a deep furrow, and one on each side of the cineritious mat-

To the lower part of the brain, or the medulla oblongata, are attached a number of small white cords, called nerves, composed of medullary matter, possessing a distinct fibrous structure, and enclosed in sheaths of membrane. These principally pass from the brain to the organs of the external senses, and bodies of a similar kind pass from the spinal cord to the muscular parts; the former have been called the cerebral, the latter the spinal or vertebral nerves; both of them are disposed in pairs, and proceed in corresponding directions to the two sides of the body. At their commencement from the brain or spinal cord, anatomists generally reckon nine pair of the former nerves and thirty of the latter [1], but they soon divide into nu-

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ter. These cords or strands are supposed to be connected with different parts of the encephalon, the anterior with the cerebrum, the posterior with the cerebellum, and the lateral cords with the restiform processes; these are termed respectively the cerebral, the cerebellic, and the restiform parts of the cord. The white or medullary matter is said to be fibrous, while it is stated, that the grey or cineritious matter is globular; ubi supra, cap. 2 et alibi. Blainville, without, as it may be presumed, any knowledge of Bellingeri's observations, had adopted an opinion of the structure of the cord, which, in many respects, coincides with it, although less minutely developed, and the same appears to have been the case with Rolando, Induct. Physiol. et Pathol. p. 197. I may remark, that Dr. Alison differs somewhat from Bellingeri with regard to the connexion of the different strands of the cord with the respective parts of the brain; Physiol. p. 133. The structure of the spinal cord, its connexion with the brain, and of its different parts with each other, has been attentively studied by Foville; Phil. Mag. v. v. p. 331 et seq. He conceives that the central parts of the brain bear a strong analogy to the different parts of the spinal cord, and may be considered as directly connected with the protuberances which are formed at its termination; the corpora pyramidalia with the cerebrum, the corpora olivaria with the corpora quadrigemina, and the corpora restiformia with the cerebellum. We have much valuable information respecting this part in the recent work of Prof. Tiedemann, which was referred to above, and in the 3d section of the 9th chapter of Mr. Mayo's Outlines. For figures of the spinal cord, see Cloquet's Anatomie, pl. 132, 3, and his Man. pl. 149, 0, and 175, the latter taken from Gall.

[1] The arrangement and enumeration of the cerebral nerves, which has been generally adopted, is the one which was originally proposed by Willis, but many alterations and improvements have been suggested by the moderns. Those, for example, which he termed the 7th pair of the cerebral nerves, actually consists of two pairs, that differ materially in their structure and functions. The 8th pair may also be divided into two, or even into three distinct pairs. Cloquet has since encreased the number of the cerebral nerves to 13, making the total number 43; Anat. de l'homme, t. iii. p. 326. It may be necessary to remark, that Willis's 10th pair of cerebral nerves has, since the time of Haller, been generally reckoned the 1st cervical pair. We have a useful table of synonimes in Vicq-d'Azyr, and in Bell's Anatomy, v. iii. p. 113, 4. In the 4th plate of Sir C. Bell's treatise on the nervous system, we have a representation of the base of the brain, the medulla oblongata, and the origin of the cerebral nerves, and in the ninth we have a view of the upper part of the spinal cord. See also Sœmmering's plate in his treatise De Basi Encephali, also Vicq-d'Azyr's 17th plate, and the 10th of Mr. Swan's Demonstrations; we may contrast this elaborate work with the early productions of Eustachius, see tab. 18. I may remark in this place, that the first nine plates of Mr. Swan's Demonstrations consist

merous branches, which are distributed to all parts of the body. In their passage they frequently anastomose or communicate with each other, and these communications are sometimes so numerous and intricate as to form a complete network, to which the name of plexus has been applied. From these plexuses new nerves originate, which seem to be independent of those which produced them. When the nerves arrive at their ultimate destination, they generally ramify into small branches, which become more and more minute, until they seem at length to be melted down into a kind of pulp, and are no longer visible to the eye[1].

In speaking of the relation which subsists between the brain and the nerves, it has been usual to describe the latter as derived from the former, or as productions of its substance. This manner of viewing the subject probably arose, in some measure, from the hypothesis of the animal spirits, which were supposed to be lodged in a series of tubes, that served as a receptacle for them, and conveyed them to all parts of the body[2]. And even since the doctrine of the animal spirits has been called in question, the same kind of language is maintained, and the nerves are spoken of, in a vague way, as fibres actually continued from

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of views of the great sympathetic nerve, and of the nerves of the thoracic and abdominal viscera, with the ganglia and plexuses; the plates 10..16, are views of the cerebral nerves, and the remaining plates 17..25, of the spinal nerves. As far as I am able to form an opinion on a question of minute anatomy, I should pronounce them to be entitled to the character of great accuracy and perfect fidelity.

[1] Besides these two classes of nerves, the cerebral and the spinal, there is one nerve, or set of nerves, that appears to hold an intermediate relation between the two, or to have a direct connexion with both the brain and the spinal cord; this is the intercostal nerve, with its ramifications. Some nervous twigs that descend from the brain unite with the branches that are sent off from the spinal cord; these form series of ganglia on each side of the spine, from which numerous nerves proceed that are distributed over all the thoracic and abdominal viscera. From the way in which the intercostal nerve is composed, it would seem adapted to combine the influence of all the parts of the nervous system, and to afford a supply of this influence to each individual organ, which, in this way, have a direct nervous communication, and it is from this circumstance that its popular name of sympathetic is derived. It must, however, be remarked, that both the anatomical and the physiological relation which this nerve bears to the other parts of the system have been the subject of much discussion. See Bichat sur la Vie et la Mort, p. 249 et seq., and Mr. Shaw's Strictures upon Bichat in Lond. Med. Journ. v. xlix. p. 456. Richerand has some good observations upon this part of the nervous system; Phys. t. i. p. 108. See also the remarks of Adelon, Physiol. t. i. p. 203..6, and t. iv. p. 147 et seq.; of Béclard, Elém. d'Anat., Sect. 3. p. 629 et seq.; of Desmoulin's Anat. des Syst. Nerv. p. 501 et seq.; and of Mr. Mayo, p. 264, 5. For figures and descriptions of this nerve, I may refer to Walter's Tab. Nerv. No. 3; to the trans. of the same, pl. 1; to Cloquet, (J.) Anat. pl. 175, and Manual, 200..2, which are taken from Walter; and to Swan, pl. 1..9. We have a minute account of this nerve by Ollivier, Dict. de Méd. t. xx. p. 143 et seq. See also the remarks of Prof. Carus, Gore's trans. v. i. p. 246, 253, and 261, on its comparative anatomy.

[2] Monro on the *Nervous System, p. 24.*

those of the medulla of the brain. Of late, however, the directly contrary opinion has been advanced by Drs. Gall and Spurzheim, that the brain is an appendage to the spinal cord, or that it is to be regarded as a kind of large tubercle or ganglion, connected with it, in the same way as other ganglia are connected with the nerves that are contiguous to them[1]. This view of the subject has been ingeniously defended by Prof. Tiedemann, by a reference to the progressive development of the nervous system in the fœtus, in which we find that the spinal cord is formed before the brain, and also by the analogy of the inferior animals, where, as we pass on from the most perfect organization to that which is less so, the brain disappears before the spinal cord[2]. Perhaps this is more a verbal distinction than an actual difference in the conception of the object, for when anatomists speak of the nerves as being productions of the brain, they probably mean no more than that the brain is the centre to which the affections of the nervous system are to be referred, employing the phrase rather in a physiological, than in an anatomical sense[3].

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[1] Recherches sur le Système Nerveux, sect. 1. As I have remarked above, the nature of the connexion between the brain and the spinal cord, and their relation to each other, form some of the most important parts of the researches of Bellingeri and Foville; see also the remarks of Adelon, Physiol. t. i. p. 142 et seq., where we have a perspicuous view of the opinions of his contemporaries. We have some valuable observations by Dr. Copland on this subject, more especially on the comparative anatomy of the sympathetic nerves, and their connexion with the other parts of the nervous system; Trans. of Richerand, p. 556 et seq.

[2] Anatomy of the fœtal brain, by Bennett, p. 149 et alibi. This observation of Prof. Tiedemann is sanctioned by the authority of Serres, who states that the spinal cord is formed before the brain in all classes of animals; Anat. comp. du Cerveau, p. xxxviii. The particular object of this treatise is to give an account of the brain, in the four classes of the vertebrata, and from the observations made upon them, to ascertain the respective functions of the several parts. In the prosecution of this object, the author has produced a work of very considerable value, accompanied with numerous engravings, the whole affording very ample testimony of his skill and industry. On account of its importance, I shall insert a brief abstract of it in the appendix to this chapter. See also Béclard, Add. à Bichat, p. 44. We have some judicious observations on the general question, and on the opinions that have been brought forward by various physiologists in Dr. Copland, ubi supra, p. 654..664. The result of all the observations is, that in the human fœtus, and in that of all the animals which bear any considerable analogy to man, the spinal cord is the part of the nervous system which is first formed, afterwards the medulla oblongata, then the cerebellum, and lastly, the cerebrum.

[3] There is a remark made by Desmoulins on this subject, which appears to me so just and appropriate, that I shall quote the paragraph at full length. "Malgré les subtilités et les dénégations de quelques personnes, ces mots, *origine, naissance, productions*, impliquent donc dans le langage des auteurs qui s'en servent, l'idée qu'une partie que l'on dit née d'une autre, produite par une autre, est réellement sortie de cette partie qui l'aurait formée, poussée par une acte de vegetation. Cela est evident dans tout l'ouvrage de Tiedemann. Il a réellement pris à la lettre, et au sens propre et non figuré, les mots origine, naissance, production. Tel est aussi le sens qu'y *attachent manifestement* MM. Gall et Serres"; Anat. des Syst. Nerv. p. 241.

The ganglia are small knots or masses of nervous matter, which are situated along the course of the nerves, generally where two or three of them form an angle, and especially in the different parts of the thorax and abdomen. They are composed of a mixture of two substances, which appear analogous to the cineritious and medullary matter of the brain; they are of a redder colour and are more copiously supplied with arteries than the nerves; they are also of a firmer consistence, and are covered with a denser membrane. Anatomists are generally agreed that the nerves which proceed from a ganglion are larger than those which enter into it, as if, in their passage through it, they had received an additional quantity of matter[1]. With respect to their texture we are informed by Monro[2], and the account which has been more lately given by Scarpa[3] is fundamentally the same, that the filaments of the different nerves which compose the ganglion proceed individually without interruption, but that they are all twisted together into an irregular bundle, and that filaments from different nerves are united in the formation of a *new* nerve. In this way it would appear that a mechanical connexion is established between the parts that receive their nerves from the ganglia, and we may presume that this will contribute to a sympathy between their actions[4].

With respect to the distribution of the nerves, it may be remarked, that the greatest part of the nervous matter is sent to the organs of sense and of voluntary motion, that the viscera are much more sparingly supplied with nerves, the glands have still fewer, while some of the membranous parts appear to be entirely without them[5]. Generally speaking, the nerves which supply the organs of sense seem to proceed immediately from the base of the brain, or rather from the medulla oblongata, while the muscles receive their nerves from the spinal cord; but there are exceptions to this rule. There is much more irregularity with respect to the course of the nerves that go to the viscera; they generally take their immediate origin from some of the ganglia and plexuses that form part of the intercostal system, and they are connected with each other in a great variety of ways, apparently for the purpose of producing a direct nervous communication between all the viscera, as well as between each viscus and the other parts of the body. It is worthy of notice that the nervous system generally, including the brain, the spinal cord, and all their ramifications, is so disposed, that if the body be divided into two lateral halves, by a plane passing perpendicularly through its centre, the nerves of the two parts will be almost exactly similar to each other, while, at the same time, they are so united by plexuses and anastomoses of various kinds, as to ensure a complete connexion between the two parts and an entire correspondence of their sensations.

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[1] Haller, El. Phys. x. 6. 11; Sœmmering, Corp. Hum. Fab. t. iv. § 157; Béclard, Anat. Ch. x. Sect. 3. p. 627 et seq.; also note to Bichat, t. i. p. 324..6; Cloquet, Man. pl. 130; Alison's Physiol. p. 132. The ganglia with their various connexions are well displayed in the different parts of Mr. Swan's elaborate work.

[2] On the Nervous System, c. 19.

[3] De Nervorum Gangliis, § 6, 7 et alibi, tab. 2.

[4] We have a very full abstract of all that refers to the ganglia in Johnstone's "Essay," with a copious list of references. I may also refer my readers to the recent treatise of Brachet of Lyons, Sur le Système Nerveux Ganglionique; we have an account of this work in the Edin. Med. Jour. v. xlv. p. 163 et seq.

[5] Haller, El. Phys. x. 6. 9; Sœmmering, Corp. Hum. Fab. t. iv. § 131; Blumenbach's Inst. Phys. § 210.

A circumstance connected with the anatomical structure of the brain that deserves to be noticed is the great quantity of blood which is transmitted to it by the arteries. Haller made a calculation, from which he concluded, that one-fifth of all the blood sent out of the left ventricle of the heart is carried to the head, although the weight of the brain in the human subject be not more than one-fortieth of that of the whole body[1]. This estimate has been thought to be too large, but even if we reduce the quantity of blood to one-tenth, according to the idea of Monro[2], it will be a very great over-proportion. There are many curious contrivances, connected with the circulation through the head, for preventing this great quantity of blood from producing any injurious effects upon the brain by its pressure or its unequal distribution, in consequence either of its stagnating in the vessels, or being too violently propelled through them, but the description of these is rather the province of the anatomist than the physiologist. Many conjectures have been formed respecting the use of this great quantity of blood, and it gives a degree of plausibility to an opinion, which was entertained by Hippocrates[3], that the brain has some analogy to a secreting organ. It has been conceived that one use of the ventricles, as well as of the various internal convolutions of the brain, is to afford a more extended surface, by which the blood-vessels may enter its substance at a greater number of points, and consequently, in smaller quantity at any one part, while, at the same time, they are more firmly supported in their passage by the greater quantity of investing membrane[4].

Both the chemical and the physical properties of the nervous matter are obviously peculiar to itself, unlike what we meet with in any other of the constituents of the body, but wherever it is found, it exhibits nearly the same properties. Its general appearance is too well known to require any description, but there is one circumstance which has lately been the subject of much discussion, how far it is to be considered as being composed of proper fibres. It is generally agreed that the medullary part of the brain, when examined in its most perfect and recent state, especially after it has been artificially hardened or condensed by the action of heat or certain chemical substances, if it be carefully scraped with a blunt instrument, exhibits the appearance of fibres of considerable magnitude, with furrows between them [1]. These furrows or striæ are, for the most part, placed in such a direction as to converge towards the base of the brain, and it has been a question, whether these fibres merely unite, forming what are termed commissures, or whether they actually cross each other, and pass on to the opposite sides of the body. That this decussation takes place with respect to some at least of the fibres, near the union of the cerebrum and cerebellum, is an opinion of ancient date [2], and has occasionally been announced in modern times, as the direct result of anatomical observation; but for its full establishment and clear demonstration we may consider ourselves as indebted to Gall and Spurzheim [3]. Still, however, the quantity of fibres which can be seen to decussate is so small compared to the whole mass of cerebral matter, as to leave some doubt whether it be sufficient to explain all the pathological consequences that have been deduced from it. I allude to the well-known fact, that an injury inflicted on one side of the brain exhibits its effects on the opposite side of the body, proving, at all events, the transmission of the nervous influence in this particular direction, whatever may be the physical structure of the organ. The spinal cord, as well as the brain, possesses a fibrous texture, but it differs from the brain in the effects resulting from disease or injury, which are generally observed to produce paralysis on the same side of the body with that on which the injury has been inflicted [1].

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[1] El. Phys. x. 5. 20.

[2] On the Nervous System, p. 3.

[3] De Glandulis, Opera, t. i. p. 272. l. 17.

[4] The conjecture of Sir Everard Home is not without plausibility, that the fluid which the ventricles contain, varying in its quantity, may serve to equalize internal pressure; Phil. Trans. for 1814, p. 471, and for 1821, p. 32. We have some interesting experiments by Professor Meyer of Bonn, on the effects produced upon the brain and its functions by tieing one or both of the carotid arteries; Ed. Med. Journ. v. xliii. p. 468 et seq.

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[1] Haller, El. Phys. x. 1. 13; Cullen's Phys. § 29. The fibrous structure of the brain was the foundation of a great part of Descartes' hypothetical opinions respecting the animal spirits.

[2] For the opinions that have been entertained on this point from the time of Aretæus to the present day, see Sœmmering de bas. Enceph. p. 19; also Dr. Cooke's elaborate work on Nervous Diseases, v. ii. p. 109. It is remarkable that the question concerning the decussation of the fibres of the optic nerves appears still to be undecided, although it is a part which, from its size and situation, might have been supposed peculiarly favourable for the purpose; see Vicq-d'Azyr, p. 51, pl. 17, fig. 1, No. 32. This question will be considered more fully in a subsequent part of the work.

[3] See especially their sixth sect. and the observations made upon it by the members of the Institute, who were selected to report on their memoir; from this, and from various papers which were published in consequence of the controversy that ensued on the originality of the observations of Gall and Spurzheim, we may conclude that the connecting fibres had been occasionally observed by anatomists, but that the circumstance had been little attended to, and was not an opinion at that time generally received. We have a good summary of the opinions of the modern anatomists on the direction of the fibres of the brain, and the nature of their connexion in Adelon's Physiol. t. i. p. 158 et alibi, and in Alison's Outlines, p. 134, 5.

Of late years we have had many microscopical observations on the minute structure of the brain. Prochaska, by employing a powerful lens, found it to be composed of a pulp, containing a number of small globules or rounded particles; the pulp itself appeared to consist of flocculi, likewise formed of globules connected together by fine cellular substance, the ultimate globules being of a tolerably firm consistence, and about eight times less than the red particles of the blood [2]. These observations, in their more essential parts, have been confirmed by the still more recent and elaborate examination of the Wenzels, who by using higher magnifiers detected more clearly the constitution of the brain, as composed of a series of these small globules, which were apparently of a cellular texture, and which constituted the whole solid mass of the organ [3]. It may seem remarkable that neither Prochaska nor the Wenzels could perceive any specific difference between the minute structure of the medullary and the cineritious matter, as we can scarcely doubt that the latter is more vascular, and it may be inferred to be so from the observations of Mr. Bauer, who confirms the existence of the globules, and remarks that they are disposed in lines, so as to give the brain its fibrous appearance. We are further informed by him, that the diameter of the globules varies from $\frac{1}{2400}$ to to $\frac{1}{4000}$ of an inch, the general size being $\frac{1}{3200}$; they are both larger and in greater proportion in the medullary than in the cortical part of the brain.[1]

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[1] See Monro on the Nervous System, c. 9; also Yelloly, in Med. Chir. Trans. v. i. p. 187 et seq.; this valuable paper evinces the uncertainty which prevailed, even among the first anatomists, respecting a matter of fact apparently of easy determination. The experiments of Flourens seem to prove that the corpora quadrigemina produce what he terms the "effet croisé," like the cerebrum and cerebellum, while the medulla oblongata and the spinal cord produce their effect on the same side of the body with that on which the injury has been received; Recherch. Expér. Mem. 2, § 9..15, p. 100, 122. Foville, however, informs us, that there is an evident decussation of the fibres which enter into the composition of the corpora pyramidalia; Phil. Mag. v. 5, p. 332; and Prof. Meyer states that the decussation may be always observed in the corpora pyramidalia of the human subject, but that in many of the other classes of the mammalia it either does not exist, or in a very slight degree only. There appears, indeed, to be a great irregularity in this respect, and which seems to bear no relation to the anatomical or physiological character of the animal; Ed. Med. Journ. v. xliii. p. 487, 8. This statement is confirmed by Mr. Mayo, who informs us, that the decussation of the fibres of the corpora pyramidalia may be easily demonstrated in man, and in various genera of the mammalia; Outlines, p. 237, 8. Rolando, in an elaborate paper, discusses at some length the question, whether the fibres connected with the medulla oblongata decussate; he refers to the observations of Chaussier in proof of the negative, and to this opinion he assents; Magendie's Journ. t. iv. p. 317 et seq. See also the remarks of Adelon, Physiol. t. i. p. 146, and of Dr. Alison, p. 143; I may refer also to a case in Dr. Bright's Reports, v. ii. p. 341. Sir C. Bell, in his late communication to the Royal Society, maintains, that the parts of the brain which are connected with the nerves both of motion and of sensation "join and decussate in the medulla oblongata;" Phil. Trans. for 1834, p. 473, see pl. 19, 20, and 21.

[2] *Op. Min. t. i. p. 342.*

[3] De Structura Cerebri, p. 24 et seq.

According to Sir Everard Home these globules are connected together by a peculiar gelatinous substance, which he conceives to act a very important part in the animal œconomy. He goes so far as to state that "there can be no doubt that the communication of sensation and volition, more or less, depends upon it;" he even regards it as the very essence of life, and, referring to Hunter's doctrine of the materia vitæ, he remarks, "this grand idea of Mr. Hunter's Mr. Bauer, by his discovery of this transparent mucus, has realized."[2]

The nervous matter has been recently made the subject of microscopical observation by Dr. M. Edwards and M. Dutrochet. According to Dr. Edwards it is composed of lines of globules, of the same size with those which form the membranes and the muscles, which have been described above, but holding an intermediate place between these bodies, as to the regularity of their disposition, and having a fatty matter interposed between the rows of globules[3]. Dr. Edwards's observations differ from Mr. Bauer's in one essential respect, Dr. Edwards conceiving the cerebral globules to be all of the same size, while Mr. Bauer supposes that they exist of various sizes. It is probable that the fatty matter, which Dr. Edwards observed between the fibres, is the same, with the gelatinous substance described by Sir E. Home[4].

Dutrochet commences his account of the nervous matter by remarking, that former physiologists had ascertained it to be composed of globules, or, as he terms them, "corpuscules globuleux," and particularly refers to the observations of Dr. Edwards. He, however, advances a step farther than Dr. Edwards, who simply announced them to be globules, and describes them as "des cellules d'une excessive petitesse, lesquels contiennent une substance médullaire ou nerveuse;" thus, as it appears, conceiving of the elementary globule of Dr. Edwards, as a body containing other matter, which, if globular, must be composed of particles very much more minute. This structure, we are informed, is very obvious in the ganglia which surround the œsophagus of some of the mollusca, where we can distinctly perceive globular cells, to the interior parietes of which are attached globular corpuscles, these corpuscles being themselves cells filled with medullary or nervous matter[1]. The nerves of a frog are found to be provided with these globular corpuscles; but here they appear to be attached externally to transparent fibres, the fibres being tubes filled with a fluid, which fluid is conceived to perform some important office in the functions of the nervous system[2]. These same globular corpuscles are dispersed irregularly through the substance of polypi, and are supposed to constitute their nervous system[3]. With respect to the structure of nerves, as distinguished from that of the brain, Dutrochet contends that the elementary fibres which enter into their composition, are not composed simply of rows of globules, according to the opinion of Dr. Edwards, "mais que ce sont des cylindres d'une substance diaphane dont la surface est hérissé de corpuscules globuleux, lesquels tantôt sont en contact et placés à la file, tantôt sont séparés les uns des autres. Comme ils couvrent toute la surface du cylindre, on est porté, dans l'observation microscopique, à croire qu'ils le composent interieurement."[4] Upon this difference between the structure of the brain and the nerves, the one being principally destined for the production of nervous power, and the other for its transmission, or, as it is termed, for nervimotion, at the same time that the former is principally composed of nervous corpuscles and the latter of nervous fibres, the author builds an hypothesis of the respective uses of these two structures. And he farther conjectures, that as in vegetables, "la nervimotion est transmise par l'intermediaire du liquid séveux," so in animals these nervous fibres must be tubes filled with a peculiar fluid, and that it is through the intervention of this fluid that the transmission of the *nervimotion* is effected[5].

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[1] Phil. Trans. for 1818, p. 176; for 1821, p. 27 et seq. He, however, informs us that when a portion of the brain, in its recent state, consisting both of the cortical and the medullary matter, is viewed by a high magnifier, the rows of globules pass, without any interruption or change of direction, from one part to another; Phil. Trans. for 1824, p. 3, pl. 1, fig. 3.

[2] Phil. Trans. for 1821, p. 32, 33. It may appear not a little remarkable, that so zealous and intelligent a disciple of the Hunterian school, and one whose pursuits and acquirements so well qualify him for judging of its tenets, should have thus broached the most direct system of materialism that has been given to the world. I shall have occasion hereafter to state the arguments which have induced me to adopt the immaterial hypothesis, but the example and authority of Sir Everard Home should certainly operate as a strong motive with those who embrace this view of the subject for exercising perfect candour towards their opponents.

[3] Sur la Struct. Elém. p. 19.

[4] The account of the peculiar substance is as follows:—"Si on écrase la masse médullaire, on aperçoit, outre les globules primitifs, des globules ou gouttelettes dont la forme et la volume varient, et qu'on reconnaît facilement pour être de la graisse."

The fibrous structure of the nerves appears to be more obvious than that of the medulla of the brain. Monro[6] and Fontana[7] have examined the nerves with the microscope, and have described them as being composed of a number of longitudinal cylinders, connected together by cellular substance, which, like the muscular fibres, may be divided into portions that are more and more minute, until at length we arrive at the primitive or ultimate nervous filament. This, according to the latter of these authors, is about twelve times greater than the fleshy fibre, and may be easily distinguished from it by its texture, as well as by its size. It is of a waved or tortuous form, and is composed of a cylindrical canal, containing a viscid pulpy matter, evidently different from the substance of the canal itself. Monro describes the ultimate nervous filament as a brownish pulpy matter, surrounded by a number of white transparent bands, but it would seem that this appearance of bands was merely an optical deception, produced by the effect of light acting upon the waved surface of the cylinder[1]. In speaking of the shape of the nerves I have employed the term cylindrical, in conformity with the account which is usually given of them, but we are informed by Sœmmering, who must be regarded as one of the highest authorities, that they are of a conical form, the apex being at the part where they are sent off from the brain, or that they gradually increase in diameter as they proceed from their origin to the organs for which they are destined[2]. With respect to the general structure of the nerves it may be further remarked, that the intercostal nerve and the par vagum are said to differ from the other nerves in the disposition of their fibres, which, instead of being straight and parallel, are irregularly connected to each other and twisted together[3].

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[1] Recherches Anat. p. 166.
[2] P. 168. pl. 2. fig. 22.
[3] P. 170. pl. 2. fig. 29.
[4] P. 169.
[5] P. 170.
[6] On the Nervous System, c. 13, and Tab. 13, fig. 1..14.
[7] *Sur les Poisons*, t. ii. p. 18 et seq. pl. 3 et 4.

The ultimate nervous fibre, as described by Fontana, is however very much smaller than the fibres that seem to compose the substance of the brain, when we scrape it with a blunt instrument; so that if we are to believe in the reality of both these formations, and to suppose that the minute structure of the brain is similar to that of the nerves, we must conjecture that the visible striæ or fibres of the brain are analogous to the lacerti or larger masses that enter into the composition of the muscles, and that, were their consistence sufficiently solid, they might be resolved into smaller primitive fibres, like those that are found in the muscles[4]. This view of the subject seems to receive some confirmation from the observations of Reil on the nerves, which, as they are among the latest, so likewise are probably to be regarded as among the best that we possess upon the subject. He describes these bodies as composed of very fine filaments, that seem to differ in thickness, from that of a hair to the finest fibre of silk. These filaments are each of them enclosed in a delicate sheath, called neurilema, and in their course down the nerve, they divide, subdivide, and unite again, in the most varied manner, producing a perfect connexion among themselves in every part. A number of these filaments forms a larger bundle or fasciculus, which is always enclosed in its sheath or neurilema, and these fasciculi divide and unite in the same way with the primitive filaments. Most of the nerves consist of several of these fasciculi, although there are nerves which contain only a single one, and perhaps some of the smallest consist only of an individual filament. The different filaments, as well as the fasciculi, are tied together by the substance which forms their sheaths, and the same body seems to compose the general sheaths or covering of the whole nerve, presenting altogether a structure which is considerably analogous to that of the muscle[1].

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[1] On the Nervous System, c. 13 and 22; See Sœmmering, Corp. Hum. Fab. t. iv. § 138.

[2] § 144.

[3] Wilson's Lectures on the Skeleton, p. 7.

[4] For Raspail's account of the intimate structure of the nerves, see his "New System," § 510..518, and pl. 9. His opinion is nearly that which is ordinarily adopted, that they consist of one or more trunks or cords of an homogeneous texture, the whole and each individual part being surrounded by the neurilema.

For our knowledge of the chemical composition of nervous matter we are indebted, in the first instance[2], to Thouret[3] and Fourcroy[4], who gave us some important information respecting it, and what they left imperfect has been more lately supplied by Vauquelin[5]. The general result of these experiments is that the medullary matter is a peculiar chemical compound, unlike any other of the constituents of the body; that in some respects it resembles a saponaceous substance, being miscible with water, and forming with it an emulsion, which remains for a long time without being decomposed. Fourcroy was not able to procure any proper oil from the cerebral substance, nor to obtain any decisive indication of the presence of oil as entering into its composition, but Vauquelin has found in it two species of adipocerous matter, which are soluble in alcohol; likewise the peculiar animal principle which is called osmazome, with a quantity of albumen, sulphur, and saline matter, and a portion of phosphorus[1]. The albuminous matter is capable of being partially coagulated both by heat and by acids, but either it is in a state of combination, which gives it specific properties, or it is an essentially different kind of albumen from that which exists in the blood. If brain be gradually heated, a great proportion of its weight, especially of the cineritious part, is evaporated in the form of water, so that the solid matter which is left amounts to no more than about one-fourth of the whole; this forms a half-solid friable mass, which may be again reduced to an emulsion by the addition of water. Brain is found to contain a quantity of saline matter, which, however, seems to be less than in many other of the components of the body; it consists principally of the phosphates of lime, soda, and ammonia.

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[1] De Structura Nervorum, c. 1..4, and plates. Mr. Mayo has conferred an obligation upon the student of anatomy, by presenting him with a translation or abstract of many of Reil's treatises, with the accompanying plates. These, which are in some measure to be regarded in the light of diagrams or plans of the brain, are characteristic and expressive, but they appear to me to exaggerate the fibrous structure of the parts, even after they have undergone the action of the chemical re-agents by which their substance is hardened, and their natural divisions rendered more distinct. In speaking of the anatomical structure of the nerves it may be proper to advert to the curious experiments of Dr. Haighton on the re-production or reparation of nerves; Phil. Trans. for 1795, p. 190 et seq. It appears from them that after a nerve has been completely divided, and its functions totally suspended, it gradually resumes its powers, and the ends are found to be connected by the formation of a new substance. We should not previously have suspected that a part possessed of such delicate functions could have been so easily restored, or that the newly-formed portion, which is obviously different from the other parts of the nerve, would have proved adequate to perform the office of the organ in its original state. See also some observations on the same subject by Blandin, in his edition of Bichat, t. i. p. 279 et seq.; by Flourens, Ann. Sc. Nat. t. xxii. p. 225 et seq.; and by Prof. Tiedemann, Jameson's Journ. v. xiv. n. s. p. 187..9.

[2] It will be amusing, and may not be altogether uninstructive, to the student of animal chemistry, to peruse Lemery's account of the chemical analysis of the brain, written about the commencement of the last century; Course of Chemistry, p. 506..10. Lemery was an intelligent and industrious experimentalist, to whom the science lies under considerable obligations.

[3] Journ. de Physique, t. xxxviii. p. 334. Thouret particularly pointed out the circumstance of the little comparative tendency of cerebral matter to undergo decomposition; p. 329.

[4] Ann. Chim. t. xvi. p. 282.

[5] Ann. Chim. t. lxxxi. p. 37; Thomson's Ann. v. i. p. 332.

Sect. 2. *Vital Powers or Faculties of the Nervous System, and the Mode of their Operation*[2].

When we consider the brain and nerves, as forming a part of the living system, our first inquiry must be, what properties they possess in common with the other organs of the body, and what powers they have that are peculiar to themselves. The answer to this question may be anticipated from what has been already stated; of the two specific powers that distinguish living from dead matter, spontaneous motion and sensation, the first is confined entirely to the muscles, while the latter is equally confined to the brain and nerves. When a nerve is acted upon in such a manner as that its appropriate power is excited, motion is not necessarily produced, nor any other visible change, but the animal feels. On the other hand, there are many cases in which motion is produced that is unattended with sensation; of this kind are most of the minute operations that compose the internal functions, of which, in a state of health, we are perfectly unconscious, and which are only known to us by their effects[1]. These two powers, therefore, motion and sensation, although in a great number of instances they are connected together, being reciprocally the cause of each other, are not, however, necessarily connected; either of them may exist separately, and when they are connected it is not in any regular proportion. We conclude, therefore, that it is the office of the nervous system to produce sensation; but the way in which this is accomplished, or the succession of changes by which it is immediately preceded, we shall find it extremely difficult to ascertain. With respect to the relation which the different parts bear to each other, it has been generally supposed that the brain is the centre of the nervous system, or that part to which all the others are subservient, and that the nerves receive impressions from external objects and transmit these impressions to the brain, where they become sensible to the mind, constituting perceptions[2]. This view of the subject is, in the main, correct, although the experiments and discoveries of the modern anatomists have led to some modifications which must be noticed.

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[1] We are informed by Couerbe, that the quantity of phosphorus in the brain varies with the state of the intellect, being in the proportion of 1 to 1½ in idiots, 2 or 2½ in persons of sound intellect, and 3, 4, or 4½ per cent. in maniacs; Turner's Chem. p. 1013. It is to be desired that so curious a circumstance should not rest upon the authority of a single individual, however respectable. We have a recent analysis of the substance of the brain by John; he finds it to consist of water, albumen, a peculiar white, and a red fatty matter, osmazome, lactic acid, neutral salts, and earthy phosphates. The white portion of the brain differs from the red portion principally in the former containing less water and more of the white fatty matter; British and Foreign Med. Rev. v. i. p. 288.

[2] For some general remarks on the properties of the nervous system I may refer to Buzareingues, Phil. Physiol, sub. init. and to Bourdon, Princ. de Physiol. ch. 2. We are indebted to Foville for some valuable suggestions on the method of studying the properties and functions of the nervous system, Phil. Mag. v. v. p. 3 et seq. See also the art. "Encéphale," by Adelon, Dict. de Méd. t. vii. p. 513 et seq.; the arts. "Sensation" and "Sensibilité," by Rullier, Ibid. t. xix. p. 250 et seq.; and the art. "Sensibilité," by Piorry, Dict. Sc. Méd. t. li. p. 88 et seq. There are various remarks that bear upon this point in Rolando's "Inductions," but I conceive that his terms *are not, in all cases*, employed with sufficient accuracy.

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[1] The separation of the two vital powers is well exemplified in the interesting experiments of Sir B. Brodie on the action of poisons on the animal system; Phil. Trans. for 1811, p. 178 et seq. We learn from them that certain substances have the effect of destroying the sensibility and the power over the voluntary muscles, while the action of the heart and of the organic functions appears to be affected only, as it were, in an indirect manner. The same distinction is still more amply and extensively established by the experiments of Dr. Philip, to which frequent reference will be made in the subsequent parts of this work. A similar conclusion may be drawn from an experiment of Magendie's on the effect of prussic acid; Quart. Journ. v. iv. p. 350.

[2] I have ventured to employ the terms "sensation" and "perception" in a sense somewhat different from their ordinary acceptation, but by so doing it appears to me that we avoid part of the obscurity which attaches to the subject. Sensation is generally used to express the effect produced on the sensorium by an impression transmitted to it by a nerve, whereas I think it will be found more convenient to extend it to all the actions of the nervous system. It will, therefore, include both the organic and animal sensibility of Bichat, and the nervous and sensorial powers of Dr. Philip, while perception, as I would propose to use the term, constitutes a mode or species of sensation, and is the result of the latter of these only, corresponding, to a certain extent, with Bichat's animal sensibility, and more nearly with Dr. Philip's sensorial powers; Quart. Journ. v. xiii. p. 97. Condillac employs the term "perception" nearly in the sense to which I propose to restrict it, but he does not distinguish it sufficiently from sensation; Traité des Sens, par. 1. The "sentiment" of Magendie and some other French writers, is nearly synonymous with "perception;" Physiol. t. i. p. 142; and Bichat uses the word

All questions respecting the action of the nervous system are involved in much obscurity, which, in some measure, attaches to the nature of the subject. Although we have found that there are many difficulties connected with the complete understanding of muscular contraction, yet we may form a plausible conjecture concerning its mode of action, and can distinctly trace its operation from its commencement in the fibre to its effect on the part that is moved. We do not indeed see how the action of the stimuli that are applied should cause the particles of the fibre to approximate, but we can clearly see the connexion between the approximation of the particles and the consequences that ensue. In the nervous system, however, we have no phenomena of this kind to guide our reasoning, and although we can prove that the nerves are the media by which external impressions are conveyed to the brain, we are totally at a loss to account for the manner in which the conveyance is managed.

In proportion to the deficiency of our knowledge upon any topic, so is generally the obscurity of our language, and the terms which we employ, when speaking of the nervous system and its actions, being originally metaphorical, and being used in different senses on different occasions, increase the difficulty of obtaining accurate ideas upon the subject. The word sensibility, which is employed by physiologists to express the peculiar power of the nervous system, is applied in common language to a certain state of the mind or character, so that before we employ it in scientific discussions we must begin by discarding our accustomed associations[1]. Physiological sensibility may be defined, the power which the nervous system possesses of receiving and transmitting certain impressions, and producing corresponding changes in the sensorium, but it is essential to notice that these two operations are not necessarily connected together, or that it is no necessary part of this sensibility for these impressions to be perceived by the mind, or to become perceptions[2].

"tact" in nearly the same sense; Sur la Vie, &c. p. 83. Legállois however, employs the word "sentiment" as correlative to "mouvement," expressing nervous action generally, p. 2 et alibi. The circumstance of applying the same term to a different faculty from that to which it had been usually appropriated by physiologists, is of itself an objection to my nomenclature, but I know of no other method of expressing my meaning clearly, unless by the invention of some new term, to which I feel a still stronger objection. Locke; Essay, b. ii. ch. 1. sect. 3; and the other modern metaphysicians, as far as I am acquainted with their works, make sensation a mode of perception, the difference between the terms referring rather to some difference in the degree in which the understanding is affected, than in the part of the nervous system which is called into action. Mr. Mayo's observations on this subject I conceive to be just and appropriate; he uses the terms very nearly in the mode proposed above; Physiol. pp. 185, 6.

[1] See note in p. 123.

[2] Although the existence of sensation without perception has been ad-

In what particular cases these powers are exercised separately, or where nervous action is not succeeded by perception, will be considered hereafter, but the possibility of the occurrence is generally admitted [1].

Assuming it, therefore, as an established fact, that the brain and nerves are the primary seat of sensibility, we must inquire into the mode by which this faculty operates. The operation we shall find to be of two kinds; the first depending upon the action of external bodies on the nervous system, the second upon the re-action of the nervous system itself on some of the corporeal organs [2]. The body is furnished with certain instruments, denominated organs of sense, consisting essentially of two parts, a peculiar conformation of an organized substance, which is specifically adapted to receive and modify certain impressions, and a quantity of nervous matter suitably disposed for the reception of the impressions after they have been thus modified. The nervous matter that belongs to the organs of sense is connected by nerves

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mitted by various physiologists, especially by Cullen, Whytt, and Bichat, the subject has generally been rendered somewhat obscure, either in consequence of the hypotheses that have been connected with it, or the terms that have been employed. Cullen uses the word "sensation" in a sense considerably different from the one proposed above, but, it must be admitted, that on this, as on every other occasion, he expresses himself with great perspicuity; Institutions, § 32, 36 et alibi. Bichat has involved the subject in his complicated doctrine of the two vital principles, one serving for organic, the other for animal life. Scarpa correctly defines "simple sensation" to be nervous action which is not attended with consciousness; Tab. Neur. § 21. The subject is clearly stated by Dr. Park; he, however, substitutes the term "reflection" for "perception;" but I prefer the latter as being less metaphorical; Quart. Journ. v. i. p. 155 et seq. Richerand correctly divides sensibility into perceptibility, and into sensation without perception; but he afterwards describes this latter too vaguely, as being common to every thing that has life, and being diffused through both animals and vegetables; El. Physiol. by De Lys, p. 27. The existence of nervous action without perception is one of the points which Sir C. Bell establishes as marking the difference between his divisions of the nerves; Phil. Trans. for 1821. The term *innervation*, which, as I remarked above, has been lately introduced by some of the French physiologists, expresses the influence of the nervous system over the organic functions; perception is at least no necessary part of this process.

[1] If a new term be thought necessary to express the power which certain parts of the nervous system possess of exciting perceptions, the analogy of our language would suggest *perceptivity*: but I have not ventured to introduce either this term or *sensitivity* into the text. Richerand has employed the word "perceptibilité" in the same sense; El. Physiol. t. i. p. 44.

[2] Flourens appears to regard these rather as two distinct powers of the nervous system, than as different modes of the operation of the same power; See his "Recherches Expérimentales," of which a brief abstract is inserted in the appendix to this chapter. We have an analysis of it by Cuvier, Ann. Chim. et Phys. t. xx. The recent investigations of Sir C. Bell lead us to a conclusion very similar to that of Flourens, as we find that different nerves, or at least different nervous filaments, are concerned in these operations. A remarkable instance of the degree in which the judgment is perverted by preconceived hypothesis occurs in the writings of Baglivi, a physiologist of genius and originality, who supports the doctrine, that the proper sensibility of the nerves resides in their membranous coats; De Fibra Motrice Spec. lib. i. cap. 5. corol. 4.

to the brain[1], and these nerves possess the power of conveying the impressions along their course to this organ, where they produce perceptions. In this operation there are three distinct stages, the original impression on the sentient nervous extremities, the transmission of the sensation along the trunk of the nerve, and the reception of it by the brain[2]; and it may be laid down as a point, proved by the most ample deduction of facts, that an external impression cannot be perceived by the mind, without going through the successive steps of this process.

One of the most important of the external senses is the touch; it is extended over a great part of the surface of the body, but its most delicate seat is the points of the fingers. When a substance presses upon the finger, some peculiar change is induced upon the expansion of nervous matter, which is connected with the cutis; a certain effect is immediately propagated along the nerves that lead from the hand to the brain, and a third change is then produced in the brain itself. That these three successive changes are all concerned in the operation is proved by daily experience, in which we find that if either the organ itself be injured, the nerve be interrupted in its course, or the brain be in any way deranged, the proper effect does not follow from the application of the impression. The example of the eye, another of the organs of sense, may be adduced, as affording a still clearer conception of the subject, the impressions of sight being of a more distinct and specific kind than those derived from the touch. The eye is an optical instrument, consisting of a lens, which is adapted for receiving the rays of light, and bringing them into a proper state for forming an impression on the retina, an expansion of nervous matter, that is situated at its posterior part. The action of the lens upon the rays of light is entirely mechanical, and differs in no respect from the effect that is produced upon them by a transparent substance of the same shape and density. The nervous expansion at the back of the eye is connected with the optic nerve, and this communicates directly with the under part of the brain. Now it is found as necessary for vision that the nerve should be in a perfect state as the eye itself, and we always find, that although both the eye and the nerve be perfect, if the brain be diseased, the correct perception of sight is not excited. It is very difficult to perform direct experiments upon those organs of the external senses that are situated in the immediate vicinity of the brain, as the eye and the ear, in consequence of the short course of their nerves, and the impossibility of coming into contact with them, without deranging parts immediately essential to life; but there are many pathological facts which prove the necessity for the entire state both of the organ of sense and the communicating nerve. Blindness and loss of hearing are as certainly produced by an affection of the optic and auditory nerves, or by any circumstance which prevents them from performing their accustomed actions, as by a disease of the eye and the ear itself, and without any physical derangement of the part, we have frequent examples, where mere pressure upon the nerves produces the same effect, and where, upon the removal of the pressure, the faculties of the organ are again restored.

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[1] Dr. M. Hall, in his late investigations respecting the spinal cord, conceives that he has discovered a new relation between this part and the nerves that proceed from it, or rather a new function of the spinal cord itself, to which he gives the name of reflex. The position on which it rests is, that a nerve may convey an impression to the spinal cord, and that the cord may transmit the impression so conveyed to another nerve, without the intervention of the brain: that a stimulus, for example, may be applied to a sentient nerve, and that this stimulus may produce the contraction of a muscle, which is connected with the nerve through the intervention of the central part of the nervous system, and that this may take place after the brain has been destroyed. It may, I think, be doubted, whether the doctrine be altogether so new as Dr. Hall conceives it to be, and whether, admitting the hypothesis, he is warranted in all the conclusions which he deduces from it. He, however, states his opinion with clearness and precision, and brings forwards many interesting facts in its support. Prof. Bellingeri's doctrine of what he terms nervous antagonism, which forms a principal subject of his "Osservazioni Patologiche," bears a considerable resemblance to Dr. Hall's reflex function. See also his Treatise de Med. Spin.; cap. 2, art. 3, p. 98 et seq.

[2] Cullen's Physiol. § 29, 30; see also Dr. Thomson's life of Cullen, p. 269 ..325, where we have an ample detail of the opinions of this physiologist on the properties and functions of the nervous system.

The second mode in which the nervous system operates is by its re-action on some of the organs of the body, an operation which, with respect to the succession of events, is the reverse of the one which has been described above. Of the actions of this description one of the most important to our existence, and the most frequently exercised, is the faculty of voluntary motion. Here the affection originates in the brain, in which some change takes place; this is transmitted down the nerve into the muscle, where an effect is produced on the fibre, which causes it to contract, and in this, as in the former case, all the three stages are equally essential. I do not at present enter into any discussion concerning the nature of these changes, but propose merely to point out the order of their succession, and their dependence upon each other. Now, in this instance, in consequence of the space which intervenes between the parts where the action commences and terminates, we have the most ample means of observing the necessity of the integrity of the nerve as the medium of communication. If the nerve be divided in its course we may exert the volition, and produce the necessary change in the brain, but no motion will ensue in the muscle; at the same time our own feelings will not indicate to us that any thing has occurred out of the ordinary course of events, and we are only aware of the defect by finding ourselves unable to produce the desired contraction. And we have it in our power to prove that, in this case, the defect does not depend upon the morbid condition of the muscle, because if we irritate the nerve just below the point where it is divided, we find that the muscle will contract, in the same manner as if a similar kind of irritation had been applied to the nerve in its entire state. We may also extend the same kind of trial to the brain, for by irritating the upper part of the nerve, above its division, we shall have a sensation produced in the brain, similar to what would have followed the application of the same stimulus to the remote extremity of the nerve.

We perceive that the two modes in which the power of sensibility operates, as far as the order of the phenomena is concerned, are exactly the reverse of each other, but that the same parts are called into action, and are equally connected together[1]. We may then conclude that sensibility is the appropriate and exclusive faculty of the nervous system, and that it has two distinct modes of action, the one originating from external impressions, which are propagated from the extremities to the centre, the other depending upon a change in the brain itself, which proceeds in the contrary direction, from the centre of the nervous system to its extreme parts. Besides these physical functions of the nervous system, there are others, which either belong to it, or are, at least, always connected with it, of an intellectual or moral kind, which constitute the science of metaphysics; so far, however, as they are attached to the corporeal frame or affect its functions, they will be considered in a subsequent part of the work.

Having ascertained that the nervous system is the organ of sensibility, either as proceeding from external impressions carried along the nerves to the brain, or transmitted by them in the contrary direction, from the brain to the voluntary muscles, our next subject of inquiry must be, in what manner is this operation effected? The question may be thus stated in direct terms. When an impression made upon an organ of sense is transmitted by a nerve to the brain, or when the exercise of volition is communicated to the nerve, so as to produce the corresponding effect upon the muscle, what change does the nerve experience, or in what way is it acted upon, so as to admit of this transmission? Three hypotheses have been invented to account for this power of the nerves; the one which is the oldest, and has been the most generally received, is, that the brain and nerves are provided with a certain fluid, called the animal spirits, which serve as the medium of communication between the different parts of the nervous system; the second supposes that this transmission is effected by means of the vibrations or oscillations of the particles of the nervous matter itself; while the third ascribes the action of the nerves to the operation of electricity.

The hypothesis of the animal spirits is popularly ascribed to Descartes, and he may, perhaps, be considered as the person who reduced it to a regular form, and contributed, by his authority, to its general reception, although traces of it may be found in the writings of Hippocrates[1]. The principal ground of this hypothesis seems to have been the idea that the brain is a secretory organ, an idea which was suggested by the great quantity of blood sent to it, and by some supposed resemblance in its structure to other secreting glands[2]. Yet, as nothing cognizable by the senses is produced by it, it was concluded that it must secrete something of a subtile or ethereal nature, peculiarly suited to the performance of the functions which belong to the brain, and which are so unlike those of other material substances. It must be recollected, that about two centuries ago, everything that could not be otherwise explained was referred to the agency of some kind of refined spirit, an idea which appears to have been originally derived from the alchemists, and after being incorporated with the metaphysics of the age, gave rise to a long train of mysticism[3]. Upon this slender foundation was built the hypothesis of the nervous fluid, or the animal spirits, as they have been termed; yet their existence was assumed as an ascertained fact, and even their different affections and diseases were spoken of with as much confidence as if the authors had been treating upon something which was the immediate object of their senses, and with which they were perfectly familiar[4]. The doctrine of the animal spirits has likewise become a subject of popular belief, and has given rise to a variety of expressions, that are every day employed in our common language. There does not, however, appear to be the least shadow of proof of their existence, either from experiment or observation; there is no analogy in their favour, the structure and physical properties of the nerves do not seem adapted to the office that has been assigned them; and in short, the whole is an hypothesis entirely unfounded and quite gratuitous[1].

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[1] This remark must be understood in a general sense only, as from some recent discoveries there is reason to conclude, that these two powers or operations are actually exercised by *different portions* of nervous matter.

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[1] On this point it will be sufficient to refer to the learned work of Dr. Good, Study of Medicine, v. ii. p. 22 et seq.

[2] Descartes, Tractatus de Homine, sect. 14.

[3] I may refer my readers to the acute remarks of the illustrious Harvey, who on this subject, as in so many others, rose superior to the opinions of his contemporaries; De Motu Cordis, Exer. 3. p. 234. See also the observations of Mr. Whewell on Newton's hypothesis of his ether as the efficient cause of gravity, in his Bridgewater Treatise, p. 223 . . 5. The hypothesis is contained in the 21st of the queries appended to his optics; Opera, a Horsley, t. iv. p. 242, 3.

[4] Haller devotes no less than ten pages of his great work, El. Phys. x. 8. 11 . . 16, to learned discussions respecting the nature of this imaginary agent, inquires whether it be albuminous, spirituous, acid, sulphureous, aëriform, or ethereal, and concludes that it bears a resemblance to what has been termed the spiritus rector of plants, a substance nearly as little understood as the one which it is intended to illustrate. The respect which must always attach to whatever comes from Haller's pen prevents those reflections which we might be inclined to make on the occasion, and we are induced rather to lament the low state of physical science when he wrote, than to impute to him any deficiency of judgment. Stuart, in his learned dissertation, on the structure and action of muscles, thus defines the nervous fluid; "tenuissimum, dulcissimum, mobilissimum, et minime cohœrens, aut coagulationi obnoxium sanguinis;" C. v. sect. 13. Its existence is advocated by Sabatier, t. iii. p. 224, and Boyer, t. iii. p. 311. Plenk devotes a section to the description of its physical properties, many of which, however, it must be allowed are negative; Hydrologia, p. 49. It is indirectly admitted even by Cuvier, Regne Anim. t. i. p. 31, and more directly by Dr. Good, v. iii. *p. 24 . . 27.*

The hypothesis of vibrations had been imperfectly stated by many of the earlier physiologists, but it was so much detailed and embellished by Hartley, as to be, by common consent, connected with his name[2]. According to this doctrine the action of the nerves consists in a vibration of the particles of which they are composed, by which impressions are transmitted along them, and conveyed to and from the brain in perception and volition respectively[3]. This hypothesis has the advantage over that of the animal spirits, inasmuch as it does not assume the existence of any imaginary agent, but it may perhaps be questioned whether it possesses any other recommendation. We have no more direct evidence of the vibration of the nervous matter than of the fluid of the Cartesians, and we may remark that the general aspect and structure of the nerves appear perhaps less adapted to vibration than to secretion. The principal arguments that have been adduced in favour of the Hartleian hypothesis are certain facts, in which it seems that when an impression has been made upon an organ of sense, the effect is continued for some time after the impressing cause is removed, and that it gradually subsides in a way which was thought most analogous to a vibratory motion[1]. The facts to which I refer occur particularly with respect to the sense of sight, and will be detailed when I come to give an account of this faculty, and I shall defer to that part of the work the further consideration of the hypothesis of vibrations, as its merits will be better understood when we have made ourselves more fully acquainted with the nature of the external senses.

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[1] The curious fact which has been established in the late controversy respecting the effect of dividing the eighth pair of nerves, that the nervous influence may be transmitted along a divided nerve, even when the parts are one-fourth of an inch asunder, affords a direct argument against the idea of this influence depending upon the passage of a subtile fluid; see Quart. Journ. v. xi. p. 325, and v. xii. p. 17.

[2] The hypothesis of vibrations was very explicitly laid down by N. Robinson, who published his treatise on the spleen some years before Hartley's "Observations" appeared; to the general idea of the vibratory action of the nerves he also subjoins the additional speculation of the "machinulæ," which bear a close resemblance to the "vibratiuncles;" New system of the Spleen, &c. p. 1. c. 7. Some of the French metaphysicians, especially Condillac, preceded Hartley in supporting the doctrine of vibrations: Condillac's work on human knowledge, where he speaks of the agitation of the fibres of the brain, was published about two years before Hartley's Observations.

[3] Strictly speaking, the hypothesis of vibrations should be subdivided into the opinions of those who suppose that the particles of the medullary matter itself are the agents, or that there is diffused or dispersed through them a subtile ether which acts the sole or the principal part. Hartley adopts the supposition of the intermediate action of the ether, p. 21, and by thus encumbering his hypothesis with this imaginary agent deprives it of its only recommendation, that of simplicity. Dr. Young's view of the subject coincides, in a considerable degree, with Hartley's, except that for the hypothetical ether, he substitutes the electric fluid; See Med. Lit. p. 99, 100; and Lect. v. i. p. 740. Blumenbach more directly and explicitly admits the plausibility "of the doctrine of a nervous fluid, which is thrown into oscillatory vibrations by the action of stimulants;" and, as if not satisfied with the two hypotheses, he argues in favour of the similarity of nervous action and the electric influence. Nor does he stop even here, but goes on to state that by the oscillations of this ether, Hartley "very ingeniously explains the association of ideas, and again, by the assistance of this, most of the functions of the animal faculties;" Physiol. § 226. It cannot but excite some surprise to observe the facility with which so eminent a physiologist and naturalist becomes involved in such an intricate tissue of unfounded speculations. His judicious and intelligent commentator, Dr. Elliotson, very candidly admits the futility of the whole train of deductions; see his note in loco. With respect to the hypothesis of vibrations I may remark, that the fact alluded to above, of the transmission of the nervous influence through the interval between the parts of a divided nerve, seems more decisive against this speculation than against that of a nervous fluid. The solution of continuity must certainly put an effectual barrier to the propagation of the vibratory or oscillatory action.

The electric hypothesis is of modern origin. It principally rests upon various experiments that have been made by Dr. W. Philip, and other English physiologists, to elucidate the laws of the nervous system, in which it appeared that when a nerve was divided, so as entirely to intercept the transmission of its action, the place of the nerve might be supplied by a galvanic apparatus[2]. The further examination of this hypothesis must be likewise deferred, until we have had an opportunity of considering more minutely the nature of the facts on which it is supported, especially those connected with the functions of secretion and digestion[3].

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[1] Hartley on Man, c. i. § i. prop. 3 et seq.; Belsham's Elements, c. iii. § 4; Alison's Physiol. p. 159.

[2] Valli's speculations on the action of the two metals upon the parts of living animals led him to assert the identity of electricity and of the nervous fluid; Journ. de Phys. t. xli. passim. The same opinion is, to a certain extent, countenanced by Dr. Young, Lect. v. i. p. 740, and was formed previously to the experiments of Dr. Philip. Mr. Abernethy goes still further; for he seems strongly inclined to regard some subtile fluid, analogous to electricity, not merely as the prime agent in sensation, but as even constituting the essence of life itself: singular as it may appear, we find this highly respectable and intelligent writer sliding into materialism, at the very time when he is directing the force of his genius against this doctrine; see Lectures on Hunter's Physiol. p. 26, 30, 35, 80 et alibi. It is scarcely necessary to observe that, metaphysically speaking, the subtile or ethereal agents that are called in to aid us in our explanation of the vital phenomena, are as truly *material* as the densest stone or metal.

[3] Although the full consideration of this hypothesis is deferred until we come to that part of the work which treats more immediately of the facts from which it is derived, I may anticipate the discussion so far as to remark, that, before the electric hypothesis can be considered as proved, two points must be demonstrated; first, that *every* function of the nervous system may be performed by the substitution of electricity for the action of the nerves; and secondly, that *all* the nerves admit of this substitution. We must not rest satisfied with its apparent action upon the stomach, which is at best a dubious case, as far as the operation of the nerves is concerned; we must show that volition can be transmitted by the electric fluid, and that this fluid is equally capable of stimulating the nerves of the involuntary, as of the *voluntary* muscles.

SECT. 3. *Use of the Nervous System*[1].

The uses of the nervous system, the subject which we are next to consider, may be resolved into two; first to maintain our connexion with the external world, by receiving external impressions and producing voluntary motions; and secondly, to unite the different parts of the animal frame into one whole[2].

Although it is possible to conceive of a kind of independent existence being carried on, at least for a limited space of time, in which the animal should be cut off from all surrounding objects, and in which the exercise of his functions should be confined to the simple continuance of life, this state of insulation could not be long maintained, and even while it lasted would be attended with the suspension of all those circumstances which characterize animal existence. We are, at every instant, receiving impressions from the objects which surround us, some of them for the immediate purpose of supplying our corporeal frame with the materials necessary for its support, and others acting more directly upon the mental faculties, and producing a species of re-action upon some of the organs of the body, by which we are led to accomplish those objects, which are scarcely less essential to our present state of existence, than what contributes to its immediate physical support. Now in consequence of its power of receiving and transmitting the impressions of external objects, the nervous system is the great apparatus by which these effects are accomplished.

Nor is the second use of the nervous system less important, that of uniting the various parts of the animal frame into one connected whole. The different functions which depend upon contractility, such as the circulation, respiration, and digestion, have all no doubt a necessary connexion with each other. The circulation could not be carried on unless the digestion produced the materials of which the blood is composed; respiration must cease unless the heart propelled the blood through the lungs; and digestion can only be performed by the blood being conveyed to the minute arteries of the stomach, after it has received its proper action in the lungs. But still, if we may use the expression, the dependance of these functions upon each other is a kind of mechanical dependance. We may conceive of a being that should have all these operations going forward, according to their respective laws, yet that there should be no consciousness of identity, and no connexion between these operations, except the physical relation which they bear to each other. If, for example, we could, by a mechanical operation, propel the blood along the arteries, and, at the same time, by artificial means, produce the alternate motions of the lungs, we might imagine it possible to procure a supply of arterial blood, which, when conveyed to the stomach, might act there so as to cause this organ to digest the substances contained in it, and to prepare from them a quantity of nutritive matter for the purpose of sanguification.

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[1] On this subject, as well as on all the questions connected with the nervous system, we have many interesting observations in the Anat. Gén. of Bichat, mixed, however, as I conceive, with much unfounded speculation. I would direct the attention of the reader to the judicious notes of Blandin on this part of Bichat's work; t. i. p. 151 et seq.

[2] The researches of Sir C. Bell lead to the conclusion that these two functions of the nervous system are exercised by different descriptions of nerves; the first by certain cerebral and spinal nerves, which pass directly from the brain or spinal cord to the organ which receives the impression; the second by these which pass from one organ to another, including the ganglionic and sympathetic nerves, or what he terms the superadded system of nerves.

Probably the life of vegetables consists in this kind of physical connexion between the different functions, in which, by mechanical and chemical actions alone, a succession of changes takes place merely depending upon the physical operation of various external agents. This may be considered as nearly coinciding with the organic life of Bichat[1]. In the supposed case of the animal, as stated above, if we only conceive a force to be applied, so as to set the fluids in motion, we might imagine the rest to be accomplished by the ordinary properties of matter, exercising its attractions and affinities, as the different substances are brought within the sphere of their mutual action. But still the being would be no more than a species of automaton, without homogeneity and destitute of consciousness. The nerves, on the contrary, pervade every part, and give to the whole set of organs and functions a necessary vital dependance upon each other, so as to bestow upon the animal the feeling of individuality, and to connect all its operations without any visible change in its structure and composition. A great part of the sciences of medicine and of pathology consists in tracing the operation of this nervous connexion between the different parts of the body, and observing the effects which are propagated to distant organs or functions by the affection of any single organ or function. And this connexion is not of that kind which we may denominate physical, where the change is extended to remote parts in consequence of an alteration in the mechanical or chemical constituents of the body, but it is to be referred to that sympathetic connexion between the parts, which can be accounted for upon no other principle but the operation of the nervous energy.

The information which we gain by investigating the anatomy of different kinds of animals, and comparing them with the human subject, confirms and illustrates this idea of the use of the nervous system. It is remarked by Blumenbach, that in the cold-blooded animals, where the size of the brain bears only a small proportion to that of the nerves proceeding from it, there is much less sympathy between the different organs and functions of the body, while, at the same time, each separate part possesses a greater share of individual vitality. This we see exemplified in the length of time during which life remains attached to their limbs when divided from the body, the power which some of them possess of re-producing parts that have been removed, and, as we descend lower in the scale of organization, the still more extraordinary power of being multiplied, like vegetables, by mechanical division[1].

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[1] Sur la Vie et la Mort, p. 3; see the remarks of Bourdon, Prin. de Physiol. p. 34. Dr. Elliotson, however, conceives that a nervous system is essential to animal life, under any form; Physiol. p. 3, 4. We have an interesting view of this subject in Tiedemann's Comp. Physiol. Part 1. Book 2.

This view of the subject will serve, in a great measure, to answer a question, which was formerly the subject of much controversy among physiologists, whether there be, what has been termed a sensorium commune, a part of the nervous system, from which volition originates, and to which all impressions are referred or conveyed, before they excite perceptions. The question has been proposed in another form, although essentially of the same import, whether, when an impression be made upon an organ of sense, as, for example, upon the eye, the perception exists in the eye or the brain? Is the last change which takes place, immediately previous to perception, an action of the nervous matter that is connected with the eye or of the brain itself? The general result of our experience leads us to conclude that there is a common centre of perception, and that in the human species it exists exclusively in the brain[2].

The proof of the existence of a common sensorium depends upon the facts that have been referred to above, where impressions made upon an organ of sense are not followed by a perception, provided the nervous communication between the organ and the brain be destroyed or injured. And the same conclusion seems to be confirmed by a series of facts the reverse of these, where, when an effect has been produced upon the brain, similar, as we may suppose, to one which had, on some former occasion, been transmitted to it from an organ of sense, it has excited the idea of an external impression, although the organ of sense may have been destroyed. This is the case with persons who, after having arrived at maturity, have had the eyes entirely destroyed, yet such individuals continue to dream of visible objects, and are able to recal visible ideas with perfect facility. It is partly also upon the principle of the actions of the brain producing effects similar to those that follow from impressions upon the extremities of the nerves, that we account for the mistaken perceptions that are experienced after the loss of a limb, which are frequently not to be distinguished from those that formerly existed in the part[1].

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[1] Blumenbach, Specim. Physiol. p. 20; Ebel, Observ. Neur. in Ludwig. Scrip. Neur. t. iii. p. 152.

[2] The word *brain* is here employed in its most extensive sense, to signify all the parts of the nervous system except the nerves and the spinal cord. The late experiments and researches of Rolando, Bellingeri, Flourens, Foville, and others lead to the conclusion, that the medulla oblongata is more immediately essential to certain nervous operations than either the cerebrum or cerebellum.

In man, as has just been stated, the sensorium appears to be exclusively confined to the brain, but as we descend in the scale of beings, to those whose functions, and especially whose nervous functions, are less perfect, it would appear that the sensorium is more extended. In some of the amphibia we may conjecture that the spinal cord partakes with the brain in all its faculties, and, as we advance to animals that have a still simpler organization, the brain entirely disappears, and the spinal cord seems to be substituted in its place. There is, however, reason to doubt whether, in this case, the animal possesses any degree of what can properly be called perception, and whether the sole object of its nervous system may not be to convey impressions from one part to another, which are necessary for the functions of the animal, but which do not excite any ideas of consciousness.

The same kind of communication by means of nerves, which we have found to be necessary with respect to the brain, is equally so with respect to the spinal cord, which may be regarded as a common centre for the greatest part of the nerves that supply the muscles of voluntary motion. When the spinal cord is compressed or divided in any part, the limbs that are supplied with nerves which branch off from it below the injury, are palsied. If the injury take place near the lower extremity of the spine, the lower limbs alone become insensible, and, as none of the functions essential to life are affected, the patient lives with all his faculties and powers unimpaired, except that of loco-motion. The nearer to the head the injury is situated, so much more extensive is the derangement of the different functions; and there are cases upon record, where, after a dislocation or fracture of some of the cervical vertebræ, all power has been lost over the voluntary muscles, and the functions of the abdominal and thoracic viscera have been nearly suspended; yet, for the time that life was capable of being continued under these circumstances, the cerebral functions and the mental faculties have remained in a sound state[2].

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[1] Porterfield on the Eye, v. i. p. 364. The peculiar feelings experienced by those who have lost a limb, which is described by the author as occurring in his own person, probably depends in part upon another cause, the comparison which they make between the sound and the mutilated extremity; an individual who was born without legs, or who had lost both of them in infancy, would never have these false perceptions.

[2] We have an excellent illustration of the functions of the spinal cord in the case of the Italian, Dominico Valetto, who, in consequence of an injury of the spine, has remained, for many years, with the complete abolition of the functions of the lower part of the body, and of most of the abdominal *viscera*, while he enjoys a state of general good health, and has the full exer-

Considering, therefore, the brain as the centre of perception, it necessarily follows, that an injury to this organ is attended with a diminution or loss of sensation to the whole system, although each of the organs of sense and motion may be individually in a sound state. This is proved by our daily experience of the effects of external violence upon the brain, and of various diseased states, either of the nervous matter itself, or of other bodies in its vicinity, such as tumours of the skull, thickening of its membranes, or effused fluids of any kind, pressing upon the surface of the brain or contained within its cavities. One of the most frequent causes of the loss of nervous power is pressure, and this may take place without any permanent injury to the part compressed, for we frequently observe that when the pressure is removed, the organ resumes its ordinary functions[1].

A question has been asked respecting the organs of voluntary motion, which is analogous to the one that has been noticed above respecting the organs of sense. When we exercise our volition, and produce muscular contraction, is the first effect of volition some change in the brain itself, or does the will act immediately upon the muscles? The question may perhaps be regarded as merely a verbal one, or at least as involving more of a metaphysical than a physical inquiry, and it must be acknowledged that, like many others connected with the nervous system, it is one to which we are unable to give a decisive answer. Analogy is, however, strongly in favour of the opinion that some change ensues in the brain itself, and that this change is the cause of a subsequent change in the nerve, and this again of the change in the muscle. The division of the communicating nerve produces the same loss of voluntary power that it does of perception, and although the process be reversed, as to the order in which the parts are affected, each separate step in the process may be supposed to be equally essential in the one case as in the other.

Although this mode of reasoning has been generally adopted by the modern physiologists, a contrary doctrine was maintained by many of the writers of the last century, and especially by the Stahlians. It was conceived to be a necessary consequence of their hypothesis, respecting the connexion between the muscular and the nervous systems, that the soul, as they termed it, is co-existent with the different parts of the body, and is

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cise of the functions of the upper part of the body; Ed. Med. Journ. v. xiii. p. 277 et seq.

[1] The case of the Parisian beggar, which has been brought forwards to explain the nature of sleep; Hartley on Man, v. i. p. 46; although it does not correctly apply to that state, is a good illustration of the effect of pressure in producing a temporary abolition of the nervous functions. To the same cause may probably be referred, in part at least, the coma which was observed to ensue in the experiments of Rolando upon deep-seated injuries of the brain, which must have been necessarily attended with a considerable effusion of blood in the interior of the organ; *Magendie, Journ. de Physiol.* t. iii. p. 95 et seq. and p. 155.

extended through all the organs of sense and the parts subservient to motion. The disciples of Stahl supposed the soul to act directly upon every part of the body, and to be immediately concerned in every vital function, whereas the opponents of this doctrine maintained that the soul acts only upon the brain, and is immediately concerned in the sensitive and intellectual functions alone[1]. I shall have occasion to state my objections to the Stahlian hypothesis hereafter, but I may remark in this place, that even were it to be admitted, the above consequence does not necessarily follow from it.

We have now, therefore, proceeded so far as to conclude that the brain is the common centre of the nervous system, to which all the impressions of external bodies on the extremities of the nerves are referred, and from which originate all the actions that are executed by the organs under the control of the will[2]. Physiologists, however, have not been satisfied with assigning the brain generally as the sensorium commune; they have been anxious to find out some particular portion of it which might be regarded as the more essential organ to which all the rest are subservient. The investigation is a curious one, and, although it may have been rendered ridiculous by the whimsical opinions to which it has given rise, it is in itself a legitimate object of inquiry. It may be regarded as essentially the same, although expressed in more correct language, with the discussion which occupied so much of the attention of the older metaphysicians and physiologists, respecting the seat of the soul, by which word, as far as they had any accurate notions upon the subject, they appear to have intended to express the organ of perception and volition, or rather that material organ to which these faculties are attached, or through which they operate. There are many circumstances with respect to the structure and organization of the brain, which have led to the supposition, not only that its various parts must each of them exercise some peculiar function, but that certain portions of it possess the specific powers of the nervous system in a much greater degree than others. The fibrous or striated appearance of the brain, which has been lately so much attended to by anatomists, seems also to lead to the same conclusion, as the uniform direction of these striæ and their regular disposition, converging to certain parts of the cerebral mass, would induce us to regard them as analogous to the fibres which compose the nerves, and intended to convey the nervous influence to some particular organ, which is more essentially or necessarily concerned in perception.

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[1] See Haller, El. Phys. x. 8. 24.

[2] Haller, El. Phys. x. 8. 23 . . 25. The agency of the brain is here extended to every operation of the nervous system in which either perception or volition is concerned; in simple nervous action, as, for example, in that which is carried on between the different abdominal viscera, we have no evidence that any parts are concerned except the nerves themselves, or the *ganglia connected* with them.

manner as if a similar kind of irritation had been applied nerve in its entire state. We may also extend the same f trial to the brain, for by irritating the upper part of the above its division, we shall have a sensation produced in ain, similar to what would have followed the application same stimulus to the remote extremity of the nerve.

perceive that the two modes in which the power of sensi- operates, as far as the order of the phenomena is concerned, actly the reverse of each other, but that the same parts lled into action, and are equally connected together[1]. ay then conclude that sensibility is the appropriate and ive faculty of the nervous system, and that it has two t modes of action, the one originating from external im- ns, which are propagated from the extremities to the , the other depending upon a change in the brain itself, proceeds in the contrary direction, from the centre of rvous system to its extreme parts. Besides these physical ns of the nervous system, there are others, which either to it, or are, at least, always connected with it, of an ctual or moral kind, which constitute the science of hysics; so far, however, as they are attached to the cor- frame or affect its functions, they will be considered in a uent part of the work.

ving ascertained that the nervous system is the organ of ility, either as proceeding from external impressions carried the nerves to the brain, or transmitted by them in the con- lirection, from the brain to the voluntary muscles, our next t of inquiry must be, in what manner is this operation effect- The question may be thus stated in direct terms. When pression made upon an organ of sense is transmitted by e to the brain, or when the exercise of volition is commu- d to the nerve, so as to produce the corresponding effect the muscle, what change does the nerve experience, or in way is it acted upon, so as to admit of this transmission? hypotheses have been invented to account for this power nerves; the one which is the oldest, and has been the generally received, is, that the brain and nerves are pro- with a certain fluid, called the animal spirits, which serve medium of communication between the different parts of rvous system; the second supposes that this transmission ted by means of the vibrations or oscillations of the par- of the nervous matter itself; while the third ascribes the of the nerves to the operation of electricity.

hypothesis of the animal spirits is popularly ascribed to rtes, and he may, perhaps, be considered as the person duced it to a regular form, and contributed, by his author- its general reception, although traces of it may be found

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in the writings of Hippocrates [1]. The principal ground of this hypothesis seems to have been the idea that the brain is a secretory organ, an idea which was suggested by the great quantity of blood sent to it, and by some supposed resemblance in its structure to other secreting glands [2]. Yet, as nothing cognizable by the senses is produced by it, it was concluded that it must secrete something of a subtile or ethereal nature, peculiarly suited to the performance of the functions which belong to the brain, and which are so unlike those of other material substances. It must be recollected, that about two centuries ago, everything that could not be otherwise explained was referred to the agency of some kind of refined spirit, an idea which appears to have been originally derived from the alchemists, and after being incorporated with the metaphysics of the age, gave rise to a long train of mysticism [3]. Upon this slender foundation was built the hypothesis of the nervous fluid, or the animal spirits, as they have been termed; yet their existence was assumed as an ascertained fact, and even their different affections and diseases were spoken of with as much confidence as if the authors had been treating upon something which was the immediate object of their senses, and with which they were perfectly familiar [4]. The doctrine of the animal spirits has likewise become a subject of popular belief, and has given rise to a variety of expressions, that are every day employed in our common language. There does not, however, appear to be the least shadow of proof of their existence, either from experiment or observation; there is no analogy in their favour, the structure and physical properties of the nerves do not seem adapted to the office that has been assigned them; and in short, the whole is an hypothesis entirely unfounded and quite gratuitous [1].

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[1] On this point it will be sufficient to refer to the learned work of Dr. Good, Study of Medicine, v. ii. p. 22 et seq.

[2] Descartes, Tractatus de Homine, sect. 14.

[3] I may refer my readers to the acute remarks of the illustrious Harvey, who on this subject, as in so many others, rose superior to the opinions of his contemporaries; De Motu Cordis, Exer. 3. p. 234. See also the observations of Mr. Whewell on Newton's hypothesis of his ether as the efficient cause of gravity, in his Bridgewater Treatise, p. 223 . . 5. The hypothesis is contained in the 21st of the queries appended to his optics; Opera, a Horsley, t. iv. p. 242, 3.

[4] Haller devotes no less than ten pages of his great work, El. Phys. x. 8. 11 . . 16, to learned discussions respecting the nature of this imaginary agent, inquires whether it be albuminous, spirituous, acid, sulphureous, aëriform, or ethereal, and concludes that it bears a resemblance to what has been termed the spiritus rector of plants, a substance nearly as little understood as the one which it is intended to illustrate. The respect which must always attach to whatever comes from Haller's pen prevents those reflections which we might be inclined to make on the occasion, and we are induced rather to lament the low state of physical science when he wrote, than to impute to him any deficiency of judgment. Stuart, in his learned dissertation, on the structure and action of muscles, thus defines the nervous fluid; "tenuissimum, dulcissimum, mobilissimum, et minime cohœrens, aut coagulationi obnoxium sanguinis;" C. v. sect. 13. Its existence is advocated by Sabatier, t. iii. p. 224, and Boyer, t. iii. p. 311. Plenk devotes a section to the description of its physical properties, many of which, however, it must be allowed are negative; Hydrologia, p. 49. It is indirectly admitted even by Cuvier, Regne Anim. t. i. p. 31, and more directly by Dr. Good, v. iii. p. 24 . . 27.

The hypothesis of vibrations had been imperfectly stated by many of the earlier physiologists, but it was so much detailed and embellished by Hartley, as to be, by common consent, connected with his name [2]. According to this doctrine the action of the nerves consists in a vibration of the particles of which they are composed, by which impressions are transmitted along them, and conveyed to and from the brain in perception and volition respectively [3]. This hypothesis has the advantage over that of the animal spirits, inasmuch as it does not assume the existence of any imaginary agent, but it may perhaps be questioned whether it possesses any other recommendation. We have no

[1] The curious fact which has been established in the late controversy respecting the effect of dividing the eighth pair of nerves, that the nervous influence may be transmitted along a divided nerve, even when the parts are one-fourth of an inch asunder, affords a direct argument against the idea of this influence depending upon the passage of a subtile fluid; see Quart. Journ. v. xi. p. 325, and v. xii. p. 17.

[2] The hypothesis of vibrations was very explicitly laid down by N. Robinson, who published his treatise on the spleen some years before Hartley's "Observations" appeared; to the general idea of the vibratory action of the nerves he also subjoins the additional speculation of the "machinulæ," which bear a close resemblance to the "vibratiuncles;" New system of the Spleen, &c. p. 1. c. 7. Some of the French metaphysicians, especially Condillac, preceded Hartley in supporting the doctrine of vibrations: Condillac's work on human knowledge, where he speaks of the agitation of the fibres of the brain, was published about two years before Hartley's Observations.

[3] Strictly speaking, the hypothesis of vibrations should be subdivided into the opinions of those who suppose that the particles of the medullary matter itself are the agents, or that there is diffused or dispersed through them a subtile ether which acts the sole or the principal part. Hartley adopts the supposition of the intermediate action of the ether, p. 21, and by thus encumbering his hypothesis with this imaginary agent deprives it of its only recommendation, that of simplicity. Dr. Young's view of the subject coincides, in a considerable degree, with Hartley's, except that for the hypothetical ether, he substitutes the electric fluid; See Med. Lit. p. 99, 100; and Lect. v. i. p. 740. Blumenbach more directly and explicitly admits the plausibility "of the doctrine of a nervous fluid, which is thrown into oscillatory vibrations by the action of stimulants;" and, as if not satisfied with the two hypotheses, he argues in favour of the similarity of nervous action and the electric influence. Nor does he stop even here, but goes on to state that by the oscillations of this ether, Hartley "very ingeniously explains the association of ideas, and again, by the assistance of this, most of the functions of the animal faculties;" Physiol. § 226. It cannot but excite some surprise to observe the facility with which so eminent a physiologist and naturalist becomes involved in such an intricate tissue of unfounded speculations. His judicious and intelligent commentator, Dr. Elliotson, very candidly admits the futility of the whole train of deductions; see his note in loco. With respect to the hypothesis of vibrations I may remark, that the fact alluded to above, of the transmission of the nervous influence through the interval between the parts of a divided nerve, seems more decisive against this speculation than against that of a nervous fluid. The solution of continuity must certainly put an effectual barrier to the propagation of the vibratory or oscillatory action.

more direct evidence of the vibration of the nervous matter than of the fluid of the Cartesians, and we may remark that the general aspect and structure of the nerves appear perhaps less adapted to vibration than to secretion. The principal arguments that have been adduced in favour of the Hartleian hypothesis are certain facts, in which it seems that when an impression has been made upon an organ of sense, the effect is continued for some time after the impressing cause is removed, and that it gradually subsides in a way which was thought most analogous to a vibratory motion[1]. The facts to which I refer occur particularly with respect to the sense of sight, and will be detailed when I come to give an account of this faculty, and I shall defer to that part of the work the further consideration of the hypothesis of vibrations, as its merits will be better understood when we have made ourselves more fully acquainted with the nature of the external senses.

The electric hypothesis is of modern origin. It principally rests upon various experiments that have been made by Dr. W. Philip, and other English physiologists, to elucidate the laws of the nervous system, in which it appeared that when a nerve was divided, so as entirely to intercept the transmission of its action, the place of the nerve might be supplied by a galvanic apparatus[2]. The further examination of this hypothesis must be likewise deferred, until we have had an opportunity of considering more minutely the nature of the facts on which it is supported, especially those connected with the functions of secretion and digestion[3].

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[1] Hartley on Man, c. i. § i. prop. 3 et seq.; Belsham's Elements, c. iii. § 4; Alison's Physiol. p. 159.

[2] Valli's speculations on the action of the two metals upon the parts of living animals led him to assert the identity of electricity and of the nervous fluid; Journ. de Phys. t. xli. passim. The same opinion is, to a certain extent, countenanced by Dr. Young, Lect. v. i. p. 740, and was formed previously to the experiments of Dr. Philip. Mr. Abernethy goes still further; for he seems strongly inclined to regard some subtile fluid, analogous to electricity, not merely as the prime agent in sensation, but as even constituting the essence of life itself: singular as it may appear, we find this highly respectable and intelligent writer sliding into materialism, at the very time when he is directing the force of his genius against this doctrine; see Lectures on Hunter's Physiol. p. 26, 30, 35, 80 et alibi. It is scarcely necessary to observe that, metaphysically speaking, the subtile or ethereal agents that are called in to aid us in our explanation of the vital phenomena, are as truly *material* as the densest stone or metal.

[3] Although the full consideration of this hypothesis is deferred until we come to that part of the work which treats more immediately of the facts from which it is derived, I may anticipate the discussion so far as to remark, that, before the electric hypothesis can be considered as proved, two points must be demonstrated; first, that *every* function of the nervous system may be performed by the substitution of electricity for the action of the nerves; and secondly, that *all* the nerves admit of this substitution. We must not rest satisfied with its apparent action upon the stomach, which is at best a dubious case, as far as the operation of the nerves is concerned; we must show that volition can be transmitted by the electric fluid, and that this fluid is equally capable of stimulating the nerves of the involuntary, as of the voluntary muscles.

SECT. 3. *Use of the Nervous System*[1].

The uses of the nervous system, the subject which we are next to consider, may be resolved into two; first to maintain our connexion with the external world, by receiving external impressions and producing voluntary motions; and secondly, to unite the different parts of the animal frame into one whole[2].

Although it is possible to conceive of a kind of independent existence being carried on, at least for a limited space of time, in which the animal should be cut off from all surrounding objects, and in which the exercise of his functions should be confined to the simple continuance of life, this state of insulation could not be long maintained, and even while it lasted would be attended with the suspension of all those circumstances which characterize animal existence. We are, at every instant, receiving impressions from the objects which surround us, some of them for the immediate purpose of supplying our corporeal frame with the materials necessary for its support, and others acting more directly upon the mental faculties, and producing a species of re-action upon some of the organs of the body, by which we are led to accomplish those objects, which are scarcely less essential to our present state of existence, than what contributes to its immediate physical support. Now in consequence of its power of receiving and transmitting the impressions of external objects, the nervous system is the great apparatus by which these effects are accomplished.

Nor is the second use of the nervous system less important, that of uniting the various parts of the animal frame into one connected whole. The different functions which depend upon contractility, such as the circulation, respiration, and digestion, have all no doubt a necessary connexion with each other. The circulation could not be carried on unless the digestion produced the materials of which the blood is composed; respiration must cease unless the heart propelled the blood through the lungs; and digestion can only be performed by the blood being conveyed to the minute arteries of the stomach, after it has received its proper action in the lungs. But still, if we may use the expression, the dependance of these functions upon each other is a kind of mechanical dependance. We may conceive of a being that should have all these operations going forward, according to their respective laws, yet that there should be no consciousness of identity, and no connexion between these operations, except the physical relation which they bear to each other. If, for example, we could, by a mechanical operation, propel the blood along the arteries, and, at the same time, by artificial means, produce the alternate motions of the lungs, we might imagine it possible to procure a supply of arterial blood, which, when conveyed to the stomach, might act there so as to cause this organ to digest the substances contained in it, and to prepare from them a quantity of nutritive matter for the purpose of sanguification.

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[1] On this subject, as well as on all the questions connected with the nervous system, we have many interesting observations in the Anat. Gén. of Bichat, mixed, however, as I conceive, with much unfounded speculation. I would direct the attention of the reader to the judicious notes of Blandin on this part of Bichat's work; t. i. p. 151 et seq.

[2] The researches of Sir C. Bell lead to the conclusion that these two functions of the nervous system are exercised by different descriptions of nerves; the first by certain cerebral and spinal nerves, which pass directly from the brain or spinal cord to the organ which receives the impression; the second by those which pass from one organ to another, including the ganglionic and sympathetic nerves, or what he terms the superadded system of nerves.

Probably the life of vegetables consists in this kind of physical connexion between the different functions, in which, by mechanical and chemical actions alone, a succession of changes takes place merely depending upon the physical operation of various external agents. This may be considered as nearly coinciding with the organic life of Bichat[1]. In the supposed case of the animal, as stated above, if we only conceive a force to be applied, so as to set the fluids in motion, we might imagine the rest to be accomplished by the ordinary properties of matter, exercising its attractions and affinities, as the different substances are brought within the sphere of their mutual action. But still the being would be no more than a species of automaton, without homogeneity and destitute of consciousness. The nerves, on the contrary, pervade every part, and give to the whole set of organs and functions a necessary vital dependance upon each other, so as to bestow upon the animal the feeling of individuality, and to connect all its operations without any visible change in its structure and composition. A great part of the sciences of medicine and of pathology consists in tracing the operation of this nervous connexion between the different parts of the body, and observing the effects which are propagated to distant organs or functions by the affection of any single organ or function. And this connexion is not of that kind which we may denominate physical, where the change is extended to remote parts in consequence of an alteration in the mechanical or chemical constituents of the body; but it is to be referred to that sympathetic connexion between the parts, which can be accounted for upon no other principle but the operation of the nervous energy.

The information which we gain by investigating the anatomy of different kinds of animals, and comparing them with the human subject, confirms and illustrates this idea of the use of the nervous system. It is remarked by Blumenbach, that in the cold-blooded animals, where the size of the brain bears only a small proportion to that of the nerves proceeding from it, there is much less sympathy between the different organs and functions of the body, while, at the same time, each separate part possesses a greater share of individual vitality. This we see exemplified in the length of time during which life remains attached to their limbs when divided from the body, the power which some of them possess of re-producing parts that have been removed, and, as we descend lower in the scale of organization, the still more extraordinary power of being multiplied, like vegetables, by mechanical division[1].

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[1] Sur la Vie et la Mort, p. 3; see the remarks of Bourdon, Prin. de Physiol. p. 34. Dr. Elliotson, however, conceives that a nervous system is essential to animal life, under any form; Physiol. p. 3, 4. We have an interesting view of this subject in Tiedemann's Comp. Physiol. Part 1. Book 2.

This view of the subject will serve, in a great measure, to answer a question, which was formerly the subject of much controversy among physiologists, whether there be, what has been termed a sensorium commune, a part of the nervous system, from which volition originates, and to which all impressions are referred or conveyed, before they excite perceptions. The question has been proposed in another form, although essentially of the same import, whether, when an impression be made upon an organ of sense, as, for example, upon the eye, the perception exists in the eye or the brain? Is the last change which takes place, immediately previous to perception, an action of the nervous matter that is connected with the eye or of the brain itself? The general result of our experience leads us to conclude that there is a common centre of perception, and that in the human species it exists exclusively in the brain[2].

The proof of the existence of a common sensorium depends upon the facts that have been referred to above, where impressions made upon an organ of sense are not followed by a perception, provided the nervous communication between the organ and the brain be destroyed or injured. And the same conclusion seems to be confirmed by a series of facts the reverse of these, where, when an effect has been produced upon the brain, similar, as we may suppose, to one which had, on some former occasion, been transmitted to it from an organ of sense, it has excited the idea of an external impression, although the organ of sense may have been destroyed. This is the case with persons who, after having arrived at maturity, have had the eyes entirely destroyed, yet such individuals continue to dream of visible objects, and are able to recal visible ideas with perfect facility. It is partly also upon the principle of the actions of the brain producing effects similar to those that follow from impressions upon the extremities of the nerves, that we account for the mistaken perceptions that are experienced after the loss of a limb, which are frequently not to be distinguished from those that formerly existed in the part[1].

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[1] Blumenbach, Specim. Physiol. p. 20; Ebel, Observ. Neur. in Ludwig. Scrip. Neur. t. iii. p. 152.

[2] The word *brain* is here employed in its most extensive sense, to signify all the parts of the nervous system except the nerves and the spinal cord. The late experiments and researches of Rolando, Bellingeri, Flourens, Foville, and others lead to the conclusion, that the medulla oblongata is more immediately essential to certain nervous operations than either the cerebrum or cerebellum.

In man, as has just been stated, the sensorium appears to be exclusively confined to the brain, but as we descend in the scale of beings, to those whose functions, and especially whose nervous functions, are less perfect, it would appear that the sensorium is more extended. In some of the amphibia we may conjecture that the spinal cord partakes with the brain in all its faculties, and, as we advance to animals that have a still simpler organization, the brain entirely disappears, and the spinal cord seems to be substituted in its place. There is, however, reason to doubt whether, in this case, the animal possesses any degree of what can properly be called perception, and whether the sole object of its nervous system may not be to convey impressions from one part to another, which are necessary for the functions of the animal, but which do not excite any ideas of consciousness.

The same kind of communication by means of nerves, which we have found to be necessary with respect to the brain, is equally so with respect to the spinal cord, which may be regarded as a common centre for the greatest part of the nerves that supply the muscles of voluntary motion. When the spinal cord is compressed or divided in any part, the limbs that are supplied with nerves which branch off from it below the injury, are palsied. If the injury take place near the lower extremity of the spine, the lower limbs alone become insensible, and, as none of the functions essential to life are affected, the patient lives with all his faculties and powers unimpaired, except that of loco-motion. The nearer to the head the injury is situated, so much more extensive is the derangement of the different functions; and there are cases upon record, where, after a dislocation or fracture of some of the cervical vertebræ, all power has been lost over the voluntary muscles, and the functions of the abdominal and thoracic viscera have been nearly suspended; yet, for the time that life was capable of being continued under these circumstances, the cerebral functions and the mental faculties have remained in a sound state[2].

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[1] Porterfield on the Eye, v. i. p. 364. The peculiar feelings experienced by those who have lost a limb, which is described by the author as occurring in his own person, probably depends in part upon another cause, the comparison which they make between the sound and the mutilated extremity; an individual who was born without legs, or who had lost both of them in infancy, would never have these false perceptions.

[2] We have an excellent illustration of the functions of the spinal cord in the case of the Italian, Dominico Valetto, who, in consequence of an injury of the spine, has remained, for many years, with the complete abolition of the functions of the lower part of the body, and of most of the abdominal viscera, while he enjoys a state of general good health, and has the full exer-

Considering, therefore, the brain as the centre of perception, it necessarily follows, that an injury to this organ is attended with a diminution or loss of sensation to the whole system, although each of the organs of sense and motion may be individually in a sound state. This is proved by our daily experience of the effects of external violence upon the brain, and of various diseased states, either of the nervous matter itself, or of other bodies in its vicinity, such as tumours of the skull, thickening of its membranes, or effused fluids of any kind, pressing upon the surface of the brain or contained within its cavities. One of the most frequent causes of the loss of nervous power is pressure, and this may take place without any permanent injury to the part compressed, for we frequently observe that when the pressure is removed, the organ resumes its ordinary functions[1].

A question has been asked respecting the organs of voluntary motion, which is analogous to the one that has been noticed above respecting the organs of sense. When we exercise our volition, and produce muscular contraction, is the first effect of volition some change in the brain itself, or does the will act immediately upon the muscles? The question may perhaps be regarded as merely a verbal one, or at least as involving more of a metaphysical than a physical inquiry, and it must be acknowledged that, like many others connected with the nervous system, it is one to which we are unable to give a decisive answer. Analogy is, however, strongly in favour of the opinion that some change ensues in the brain itself, and that this change is the cause of a subsequent change in the nerve, and this again of the change in the muscle. The division of the communicating nerve produces the same loss of voluntary power that it does of perception, and although the process be reversed, as to the order in which the parts are affected, each separate step in the process may be supposed to be equally essential in the one case as in the other.

Although this mode of reasoning has been generally adopted by the modern physiologists, a contrary doctrine was maintained by many of the writers of the last century, and especially by the Stahlians. It was conceived to be a necessary consequence of their hypothesis, respecting the connexion between the muscular and the nervous systems, that the soul, as they termed it, is co-existent with the different parts of the body, and is

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cise of the functions of the upper part of the body; Ed. Med. Journ. v. xiii. p. 277 et seq.

[1] The case of the Parisian beggar, which has been brought forwards to explain the nature of sleep; Hartley on Man, v. i. p. 46; although it does not correctly apply to that state, is a good illustration of the effect of pressure in producing a temporary abolition of the nervous functions. To the same cause may probably be referred, in part at least, the coma which was observed to ensue in the experiments of Rolando upon deep-seated injuries of the brain, which must have been necessarily attended with a considerable effusion of blood in the interior of the organ; Magendie, Journ. de Physiol. t. iii. p. 95 et seq. and p. 155.

extended through all the organs of sense and the parts subservient to motion. The disciples of Stahl supposed the soul to act directly upon every part of the body, and to be immediately concerned in every vital function, whereas the opponents of this doctrine maintained that the soul acts only upon the brain, and is immediately concerned in the sensitive and intellectual functions alone [1]. I shall have occasion to state my objections to the Stahlian hypothesis hereafter, but I may remark in this place, that even were it to be admitted, the above consequence does not necessarily follow from it.

We have now, therefore, proceeded so far as to conclude that the brain is the common centre of the nervous system, to which all the impressions of external bodies on the extremities of the nerves are referred, and from which originate all the actions that are executed by the organs under the control of the will [2]. Physiologists, however, have not been satisfied with assigning the brain generally as the sensorium commune; they have been anxious to find out some particular portion of it which might be regarded as the more essential organ to which all the rest are subservient. The investigation is a curious one, and, although it may have been rendered ridiculous by the whimsical opinions to which it has given rise, it is in itself a legitimate object of inquiry. It may be regarded as essentially the same, although expressed in more correct language, with the discussion which occupied so much of the attention of the older metaphysicians and physiologists, respecting the seat of the soul, by which word, as far as they had any accurate notions upon the subject, they appear to have intended to express the organ of perception and volition, or rather that material organ to which these faculties are attached, or through which they operate. There are many circumstances with respect to the structure and organization of the brain, which have led to the supposition, not only that its various parts must each of them exercise some peculiar function, but that certain portions of it possess the specific powers of the nervous system in a much greater degree than others. The fibrous or striated appearance of the brain, which has been lately so much attended to by anatomists, seems also to lead to the same conclusion, as the uniform direction of these striæ and their regular disposition, converging to certain parts of the cerebral mass, would induce us to regard them as analogous to the fibres which compose the nerves, and intended to convey the nervous influence to some particular organ, which is more essentially or necessarily concerned in perception.

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[1] See Haller, El. Phys. x. 8. 24.

[2] Haller, El. Phys. x. 8. 23 . . 25. The agency of the brain is here extended to every operation of the nervous system in which either perception or volition is concerned; in simple nervous action, as, for example, in that which is carried on between the different abdominal viscera, we have no evidence that any parts are concerned except the nerves themselves, or the ganglia connected with them.

It would be an unprofitable waste of time to relate the various notions that have been entertained upon this subject, as they are, for the most part, purely hypothetical, and destitute even of a shadow of proof. It may be proper, however, to notice an opinion that prevailed at one time among some eminent physiologists, that the immediate seat of perception is not in the brain itself, but in the investing membrane, an opinion evidently connected with the mistaken hypothesis derived from the ancients, respecting the sensibility of membrane generally, to which I have already had occasion to refer, and which, as we shall afterwards find, was applied to many other parts of the animal œconomy. There is also another opinion on this subject that may be noticed, but certainly more in consequence of the celebrity of the name to which it is attached, and to the favourable reception which it experienced among men of science, than of its intrinsic merit. I allude to the idea of Descartes, who pointed out the pineal gland as the peculiar organ of the nervous functions, or, as it was termed, the seat of the soul[1]. The pineal gland is a small projection at the basis of the brain, which, in many respects, is curiously organized, and appears to be carefully protected from external injury. It was therefore conjectured that it must serve some important purpose, and this conjecture appeared to be confirmed by the circumstance that, upon examining the brains of certain idiots, they were found to contain a quantity of earthy matter. This sand was supposed to be an extraneous substance which, from accident or disease, was lodged in the part, and impeded its functions, and, a connexion thus appearing to exist between a disease of this part and an imperfection in the nervous powers, it was concluded to be the immediate seat of these faculties. There was some plausibility in this reasoning. But Descartes, although a man who very effectually promoted the progress of knowledge, lived before the full establishment of the inductive method of philosophizing. He neglected to inquire into the natural state of the gland; it has been since found that, in the adult human subject, earthy matter is always present in it, and indeed composes a considerable part of its substance[2].

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[1] Tractatus de Homine, Pars Quinta. This organ is thus described by Muraltus; "Hæc (glandula pinealis) radicibus quatuor, aliquando binis, aliquando unica, insignibus medullaribus, i. e. nervis omnibus in compendio junctis suffulcitur: hæc omnium objectorum motus excipit: anima in hac sola per hos motus sensilia externa, et omnes ideas, quæ a sensibus proficiscuntur, apprehendit, tanquam in centro, &c.;" Clavis Medicinæ, p. 508. (1677.) A perspicuous abstract of Descartes' system may be found in Sprengel, Hist. de la Méd. t. v. sect. 5. ch. 4. He informs us, contrary to what is commonly related, that the existence of earthy matter in the pineal gland was first detected by Huet. For the form of the organ and its connexion with the other parts of the brain, see Vicq-d'Azyr, pl. 8. fig. 1. Nos. 17, 18; pl. 12, No. 14; pl. 13. No. 14; pl. 14. No. 20; pl. 16. No. 45; pl. 27, fig. 7. taken from Sœmmering. This splendid performance may be admitted almost to supersede every other reference.

[2] Blumenbach's *Physiol. p. 125; and* Comp. Anat. by Lawrence, p. 296.

The investigation of the particular seat of nervous sensibility has been diligently prosecuted by some of the modern anatomists, and they have undoubtedly proceeded upon a more correct plan, if they have not been more successful in their result. They have not been satisfied with mere conjecture, but, in order to discover this supposed seat of sensibility, they have adopted two modes of inquiry.

According to the first, they have examined the brain after it has been injured by accident or disease, and have noticed what effects have been produced upon its faculties; whether the destruction of any particular part has been followed by the loss of any particular faculty; or whether there is any one part, the destruction of which seems to be necessarily connected with the total loss of the sensitive functions. But although many accurate examinations have been made, and many curious facts that bear upon the question, have been brought forward, we have arrived at scarcely any conclusions, which can be considered as fully established, except that the medullary matter in general possesses the appropriate faculties of the nervous system in a higher degree than the cortical[1]. It seems likewise to be proved that the sensibility of the medullary part itself increases as we proceed nearer to the centre of the brain, where we also find a much more elaborate system of organization and a much greater variety of separate parts, all of which we may fairly conclude serve some appropriate purpose connected with the nervous powers. Indeed, the result of our examination of the brain, after it has been injured or diseased, is that it is capable of undergoing a much greater degree of disorganization in its mechanical structure, than could previously have been supposed compatible with the maintenance of its functions, without their being very materially affected. With respect to the more external portion of the brain, it is well known that it may be pierced or cut, or even that large masses of it may be removed without any very considerable effect being produced upon the perceptive faculties[2], and we frequently find that large abscesses are formed

Sœmmering, De Açervulo Cerebri Dissertatio, in Ludwig, Scrip. Neur. t. iii. p. 322. An analysis is given of the earthy matter, but it is not sufficiently accurate to enable us to ascertain its nature; it seems to indicate that it contains lime, and it is stated that the oxalic acid enters into its composition, p. 337. See also Sœmmering, Corp. Hum. Fab. t. 4. sect. 52, et Wenzel de Penit. Struct. Cereb. p. 316

[1] I have stated above, that even on this point, the contrary doctrine has been maintained by Foville.

[2] This was very remarkably exemplified in the experiments of M. Legallois and Dr. Philip, and more particularly in those that have been made more lately by M. Flourens. Sir Everard Home's experience leads him to a conclusion still more singular, that all the functions of the brain remain after the destruction of the whole of its medullary matter; Phil. Trans. for 1821, p. 31. See likewise Monro on the Brain, p. 38, with the References; and Dict. des Scien. Méd. Art. "Hydrocéphale," t. xxii. p. 243 et alibi, by Itard. This is also the inference that we are to draw from an amusing, although not very scientific paper in the Edinburgh Review, vol. xxiv. p. 434;

in it, or tumours and excrescences of various kinds, which, if they do not compress the remaining part of the brain, seem to produce little injury to its functions. There are also many curious and well authenticated pathological facts on record, as well as the results of experiments, where different parts of the medulla of the brain have been destroyed, and even those which, from their situation with respect to the organs of sense, might have been supposed the most essential, and yet the nervous powers have remained nearly in their ordinary state[1].

The facts that have been observed with respect to hydrocephalus bear immediately upon this question, and lead to conclusions that are very unexpected. When the water collects in the ventricles that are near the base of the brain, if the skull yields to the distending force from within, and the pressure be not too suddenly applied, the bones separate and the skull becomes enlarged to an immoderate size. When the head is examined after death, the cavity of the skull is found to be filled with a fluid, surrounded by a kind of bag of cerebral matter. Until lately it was assumed as an obvious and well ascertained fact, that, in these cases, a considerable part of the brain was actually removed by the absorbents; but even if, according to the statement of Gall and some of the continental anatomists, the substance of the brain is not actually diminished, still its texture and organization must be very materially deranged[2]. From the

if we adopt implicitly all the statements that are brought forward in this article, it would be very difficult to assign any use for the brain. Of the two most remarkable cases quoted, that taken from Quin, it may be remarked, is vaguely and briefly related, and is moreover anonymous; On Dropsy of the Brain, p. 104. The other case, which was very minutely described by Heysham, is, on many accounts, deserving of our attention, and serves to show, in a remarkable manner, the independence of the contractile functions upon the nervous system, but it does not throw much light on the connexion between the brain and the sensitive functions. I may be allowed to observe that the reasoning of Dr. Hull would have been more perspicuous had the terms employed been used in a more definite manner; the essay is, however, a valuable collection of facts; Manchester Mem. vol. v. p. 475.

[1] So many facts of this description have been lately brought to light, that Dr. Alison conceives it to be "satisfactorily ascertained that no part of the brain, higher than the corpora quadrigemina, nor of the cerebellum is essentially concerned in sensation;" Physiol. p. 167. We have a remarkable case, which bears upon this point, related by Combette, of a girl who lived for nine years, and who was found on examination after death to be totally without the cerebellum, and where, according to Magendie's opinion, the defect was congenital; the state of the senses was much less imperfect than might have been anticipated; Journ. t. xi. p. 27 et seq.

[2] The opinion, that in those cases of hydrocephalus where the skull allows of the extension of the size of the head, and the consequent formation of a large central cavity, the substance of the brain is not actually removed, but has only the relative situation of its parts changed, was maintained by Sir Everard Home, probably before it had been promulgated by the continental anatomists. In the Phil. Trans. for 1814, p. 474, after giving an account of a case in which the head had acquired an enormous size, while the mental faculties were but little impaired, he adds, "The cerebrum is made up of thin convolutions of *medullary and cortical* substance, surrounding the two

gradual way in which the symptoms of the disease manifest themselves, we may be certain that, for a long time before death, the head must have been nearly in the same state in which it is found upon dissection, yet the faculties, both physical and intellectual, have remained in tolerable perfection, and the patient has rather suffered from general indisposition, and from the inconvenience of an unwieldy head, totally disproportioned to the rest of the body, than from a defect of the powers of the nervous system[1].

Another method which has been employed in order to ascertain the seat of the sensorium commune, is to endeavour to trace up the nerves of the different organs of sense to one spot within the brain, which might be considered as their origin. But although this plan has been attempted by the most skilful anatomists, it has not been successful; many of the nerves may indeed be traced up to the base of the brain, or to some part immediately connected with the medulla oblongata, but this cannot be accomplished with respect to the whole of them. It would appear, therefore, that no anatomical centre of this kind has yet been detected, and when we find that the accurate Sœmmering, as the result of his researches, has fixed upon the halitus, or fluid in the ventricles, as the primary seat of sensibility[2], we can scarcely expect that it ever will be discovered.

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lateral ventricles, which are unfolded when the cavities of these ventricles are enlarged, and in this unfolded state the functions belonging to this part of the organ can be carried on." As the brain in this case was not examined, we may infer that the above conclusion was derived from dissections of morbid brains that had been previously made, and it will hence afford us another instance in which Sir Everard Home has anticipated Drs. Gall and Spurzheim in what has been supposed among the most novel of their doctrines. A well marked case of this description has been lately published by Dr. Craigie, in the Ed. Med. Journ. v. xxxviii. p. 45 et seq. Dr. Alison, however, believes that there is occasionally in this disease an actual absorption of cerebral matter; Physiol. p. 139. Some remarks are made by Morgagni on this subject, Book 1. letter 12. art. 13 et seq., which show that he had a somewhat similar idea, although probably less precise than that of Sir Everard Home. He speaks of "the substance of the cerebrum itself" adhering to the skull "in the form of a membrane;" of "the brain being extended almost to the thinness of a membrane;" but, at the same time, he conceives that there are many cases where the brain is actually destroyed. It does not exactly appear whether he thought the loss of the sensitive and intellectual functions were in proportion to the *actual destruction* of the brain.

[1] Dr. Male has detailed one of the most remarkable cases of this kind which we have on record in the Ed. Med. Journ. v. ix. p. 398. On this point, as well as on every thing which respects the physiology or pathology of this disease, I may refer my readers to the elaborate article "Hydrocéphale chronique," by Breschet, in Dict. de Méd. t. xi. p. 319 et seq.

[2] He admits that every part of the brain has been injured, without any corresponding injury having been perceived in its functions, and he, at the same time, objects to the doctrine that all the brain in its whole extent is to be regarded as the seat of the mental powers; Corp. Hum. Fab. t. iv. § 98. Hence he conceived himself reduced to the dilemma of fixing upon the fluid of the ventricles as the appropriate organ of the noblest faculty which is possessed by man. In § 59, he says, "peculiare organum sensorii communis si

And besides the difficulties, both pathological and anatomical, which we have found in our attempts to fix upon any spot within the cerebral mass as the immediate seat of the perceptive faculties, there are some circumstances which lead us to doubt whether any organ of this kind actually exists. There are certain considerations which have induced many physiologists to conclude that, although the brain is to be regarded as the common sensorium, yet that the expression can only be employed in a general way, when we speak of the cerebrum and cerebellum as contrasted with the nerves and spinal cord. An opinion has long prevailed that different portions of the brain are subservient to different offices, as, for example, that some are more peculiarly connected with the organs of sense, some with voluntary motion, and others with the different vital functions. Willis was among the first who distinctly pointed out certain phenomena that were supposed to lead to this conclusion. His idea was, that the cerebrum or proper brain is the organ of the perceptions derived from the external senses, and of voluntary motion, while the cerebellum is the source of the involuntary and vital functions[1]. This opinion, which was embraced and zealously defended by Boerhaave, and many of his disciples[2], was derived partly from observations and experiments on the effects of injuries to the two parts of the cerebral mass respectively, and partly from the investigations of comparative anatomy; many curious coincidences were indeed pointed out, yet the objections that were urged against the hypothesis by Haller and others appeared to be so decisive[3], that it was generally abandoned, as being altogether untenable.

The French physiologists, of late years, have been particularly active in this investigation, both in the way of direct experiments on living animals, and of pathological observations. Among the experimentalists, Flourens has especially distinguished himself[4]; he proceeded upon the plan of gradually removing the

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ponere fas est, vel si propria sedes sensorio communi in cerebro est, haud sine veri quadam specie hoc in humore (ventriculorum) quæri debet." On this subject see Sprengel, Inst. Med. t. ii. p. 237 et seq. The experiments of many of the modern physiologists appear, indeed, to indicate, that the removal or mutilation of the portion of the medulla oblongata, where the 5th, 7th, and 8th pairs of nerves originate, is immediately followed by the loss of the perceptive faculties, and the cessation of the vital functions. Flourens expressly states, that the origin of the 8th pair of nerves in the medulla oblongata is the essential seat of motion and perception, an opinion which he advances as the direct result of his experiments; Mém. Acad. Sc. t. ix. p. 475 et seq. § 5. But when we consider how difficult it must be to remove this part, without injury to the other cerebral organs, and how many embarrassing circumstances must necessarily attend upon such an experiment, we are perhaps scarcely warranted in the positive conclusion that has been deduced from it.

[1] Cerebri Anat. c. 15. p. 74.

[2] Boerhaave, Instit. § 401, 415. See also Haller, El. Phys. iv. 5, 8.

[3] El. Phys. x. 7, 36.

[4] The most complete account of Flourens' experiments is contained in his "Recherches expérimentales"; *a work which* embraces so many new and

brain by successive portions, and noticing the corresponding changes which were produced on the functions of the animals that had been thus treated. His experiments appear to have been numerous and to have been carefully performed; the results coincided very remarkably with those which had been previously obtained by Professor Rolando[1], but of which there is every reason to believe that Flourens had no knowledge when his work was published; they have been also confirmed, in some of their leading points, by the later experiments of Serres and Desmoulins[2], and they have, moreover, obtained, to a certain extent, the sanction of Cuvier[3]. But although we may admit, that the experiments of Flourens and his learned colleagues afford us many important and interesting results, I am disposed to regard them as less calculated to lead us to the true theory than the pathological observations which have been made by Foville and others[4], inasmuch as there are difficulties in all experiments that are performed on living animals, and especially those respecting the nervous system, which no dexterity or address on the part of the operator can entirely overcome. In the present case we have physiologists, between whose claims on our attention it would be difficult to decide, who relate the results of their experiments with minuteness, and with every appearance of candour and correctness, and yet whose conclusions are frequently at variance with each other. Flourens, for example, conceives that the cerebrum is necessary to perception, and is the immediate seat of volition and intellect, while the cerebellum appears to be the organ through which the animal exercises its voluntary power over the muscles, and combines and regulates their actions, so as to produce all the complicated varieties of voluntary motion.

We have, however, a series of experiments by Bouillaud[5],

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interesting statements, that I have thought it desirable to subjoin a somewhat detailed analysis of it in the appendix. A valuable report of the experiments, drawn up by Cuvier, is contained in the Ann. Chim. Phys. t. xx. p. 294 et seq., also in Magendie's Journal, t. ii. p. 372 et seq. There is likewise an account of some curious experiments by Flourens, on the effect of the application of various stimulating substances to the different parts of the brain, in Ann. Sc. Nat. t. xx. p. 337 et seq. The results of the two sets of experiments are, for the most part, coincident.

[1] See the appendix to this chapter.

[2] An analysis of Serres' work, on the comparative anatomy of the brain, and of Desmoulins', entitled "Anatomie des systêmes nerveux des Animaux à vertèbres," are subjoined in the appendix.

[3] In the report in Ann. Chim. et Phys. referred to above.

[4] Foville's pathological observations, and the conclusions which he deduces from them, are contained in his memoir presented to the Academy of Sciences, a translation of which, together with the report of Blainville, by Dr. Hodgkin, is inserted in the Phil. Mag. v. v. p. 278 et seq. and p. 331 et seq. See also Grandechamp and Foville sur le système nerveux; the art. "Encéphale" in Dict. Prat. Méd. et Chir.; and Prichard on Insanity, p. 219 et seq.

[5] We have an account of Bouillaud's experiments and opinions of the functions of the brain in the 10th vol. of Magendie's Journ. p. 36 et seq. He gives great praise to Flourens for his researches on the cerebellum, but *he thinks* that his ideas respecting the cerebrum are not correct. He found

the results of which are decidedly opposed to those of Flourens, with respect to the functions of the cerebrum, for we learn from him, that animals, in whom this part has been entirely removed, still gave evident marks of perception, and performed certain motions, which must be regarded as depending on habit or instinct. Bouillaud agrees generally with Flourens in regard to the functions of the cerebellum, and this view of its functions may be considered as generally confirmed by the very singular effects that were observed by Magendie and Fodéra[1] to be produced by the removal or mutilation of this part. But here again we have a series of experiments by Desmoulins, which appear equally direct with those of Flourens, but which were attended with different results[2]. Upon the whole I am disposed to conclude, that although experiments on living animals, when not too complicated and refined, may assist us in our investigations, yet that we are, upon the whole, much less liable to error by pursuing the method of pathological observation; that by this mode we avoid those counteracting and interfering circumstances, which must occur in all experiments on living

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that the removal of the cerebral lobes of a fowl produced drowsiness and want of perception; but although the intellectual powers were destroyed, the animal still appeared to retain sensation; hence he infers that these lobes are the more immediate seat of the intellect. He dissents from the conclusion of Foville that they are "the sole seat of the sensations, the instincts, intelligence, and volition," because some sensations and instincts certainly remain after their removal. He supposes that the different portions of the cerebral lobes possess distinct functions, as some of them may be destroyed while others continue. A number of experiments are then detailed, in which the lobes of various animals, birds, rabbits, and dogs, were removed, cauterized, or laid bare; the general results were, that the sensations were not destroyed, but that the intellectual powers were either destroyed or disordered. The removal of the cerebrum destroyed the power of recognizing external objects and intellectual acts depending on this power, but the animal still retained the power of motion and the use of the external senses; the destruction of the anterior part of the cerebrum produced very nearly the same effects with the destruction of the whole. The experiments were supposed to afford clear proof of the different seat of the sensations and the intellectual faculties. It would appear that the anterior lobes are more especially the seat of many of the intellectual functions.

[1] The experiments of Magendie here referred to are those in which the removal or destruction of the cerebellum produced in the animal an irresistible tendency to retrograde motion; Journ. t. iii. p. 157, and those in which, on dividing one of the crura cerebelli, the animal began to rotate with great rapidity, and continued to do so without interruption as long as it survived the operation; Journ. t. iv. p. 399 et seq. A similar kind of rotation was observed by Serres in a man, in whom after death a disease of the same part of the brain was detected; p. 405. Fodéra also witnessed the same irresistible tendency to retrograde motion after the removal or mutilation of the cerebellum; Journ. t. iii. p. 191.

[2] There is no part of the encephalon which has been the subject of more experiment, and has given rise to a greater number of speculations than the cerebellum; we have a summary of them given us by Montault, in Magendie's Journ. t. xi. p. 61 et seq. I may refer in this place to the valuable pathological paper of Serres, on the organic diseases of the cerebellum, in the same work, t. iii. p. 114 et seq.

animals, against which it is impossible to guard, and the amount of which we are unable to appreciate. And in addition to these considerations I may remark, that all experiments of this description are necessarily confined to the inferior animals, and that among these, even those which approach the most nearly to man, still differ from him considerably, both in the physical organization of the brain, and in the extent and diversity of their nervous functions, so that any analogy or inference which we may draw, must be proportionally imperfect and fallacious.

But although it may be admitted that we have not completely succeeded in our attempts to appropriate different parts of the cerebral mass to different nervous functions, either according to the arrangement of Willis, or of any other which has been proposed since his time, still there are various considerations which may be regarded as proving that this appropriation actually exists. We may regard it as fully established, that the functions of the nerves are different from, and, to a certain extent, independent of those of the brain, and accordingly Dr. Philip, who was one of the earliest physiologists who clearly recognized this distinction, has assigned them different denominations, styling them respectively the nervous and the sensorial powers[1]. It appears that the former of these consists simply in the transmission of certain effects by means of the nerves from one part of the system to another, in which the brain is not necessarily concerned, and that consequently they are not referable to the common sensorium. On the present occasion, our inquiry will be limited to Dr. Philip's sensorial power, which he supposes to be composed of the functions of perception[2] and volition. Now there is at least no antecedent improbability in the supposition, that these functions, which appear to be so clearly distinct in their nature and operation, might be attached to different parts of the brain, and this opinion would appear to be confirmed by some of the experiments and observations which have been referred to above. Admitting that the subject still requires further elucidation, and that there is still considerable uncertainty attached to it, we appear to be warranted in concluding, that the perceptive faculties have their specific seat in the medulla oblongata and its appendages, that volition has probably a more extended connexion with some other parts of the cerebral mass, perhaps with the cerebellum, while the cerebral lobes are the more immediate seat of the intellectual faculties, or that it is through this part of the nervous system that the mind acts on the physical organs[3]. But although so much doubt still attaches to our hypotheses respecting the use of the different parts of the brain, and especially respecting the specific seat of the two functions of perception and volition, the interesting researches of Sir C. Bell, Professor Bellingeri, and M. Magendie[1] have clearly demonstrated, that the transmission of these powers, from the extremities to the brain, or from the brain to the extremities, is effected by different nerves. The nerves that proceed from the spine have a double origin, or are composed of two nerves, one proceeding from the anterior, and the other from the posterior part of the cord. Now it has been found by direct experiment, that the parts to which the nerves are sent are deprived of motion and sensation respectively, according as the anterior or posterior roots of the nerves are divided[2]. From this separation of the functions of

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[1] Inquiry, p. 186; Phil. Trans. for 1815, p. 90. We have some useful observations by Bourdon on this subject, in his Princ. de Physiol.; he gives us a very copious list of references to the authors who have treated on this point.

[2] Or, as he terms it, sensation; see note in p. 140.

[3] I must not omit to state, that Dr. Philip, whose experimental researches give so much weight to his opinion, concludes, that the division of the encephalon into cerebrum and cerebellum has no relation to the actions of the *voluntary* and involuntary muscles, but may rather refer to some distinction

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between the different sensorial functions; Exp. Inq. p. 108. On the subject I may refer to Buzareingues, Phil. Physiol. p. 90; to Fodéra, Magendie's Journ. t. iii. p. 191 et seq.; to Bouillaud, ibid. t. x. p. 36 et seq.; to Montault, ibid. t. xi. p. 61; and to Foville, Phil. Mag. v. 5. According to Dr. Roget, v. 2. p. 565,6, the cerebral hemispheres are the chief instrument of the intellectual operations, the optic lobes and the medulla oblongata are principally concerned in sensation, while the cerebellum is the immediate agent in voluntary motion. Bellingeri thinks that the two great divisions of the encephalon, the cerebrum and the cerebellum, are connected respectively with the motions of flexion and extension; Osserv. Patol. cap. 3 and 4. He extends his doctrine to the anterior and posterior cords of the spinal column; cap. 6 and 7.

[1] With respect to the claims of Sir C. Bell and Professor Bellingeri to the priority of discovery, I feel myself justified in coming to the conclusion, that although Sir C. Bell may have been anticipated on some points by the Italian physiologist, yet that when he performed his experiments and published an account of them, he was entirely unacquainted with those of Bellingeri. Their coincidence, as in the case of Flourens and Rolando, must be regarded as affording a strong confirmation of their truth. With respect to the respective claims of Sir C. Bell and M. Magendie, which have been the subject of so much discussion, I feel it necessary to state, in justice to the former, that his experiments clearly appear to have been antecedent to those of M. Magendie. On this subject the papers of Mr. Shaw furnish very decisive evidence, and should be perused, as well for this as for the other valuable matter which they contain, connected with the double function of the nerves; see Lond. Med. Journ. v. xlviii. p. 343 and 457; v. xlix. p. 449. It is always a painful task to notice what may appear like disingenuousness, especially in those whose attainments are of a superior order; such a reflection cannot, however, but be suggested by the perusal of a late memoir of M. Magendie's in Ann. de Chim. et Phys. t. xxiii. p. 429. See also various papers by Magendie in the 2d vol. of his Journal; also a note in t. x. p. 1, 2. I may also refer to a note appended to Sir C. Bell's work on the nervous system, p. xxi.

[2] The theory of the distinct office of the two portions of the spinal nerves has been confirmed by some late experiments of Professor Muller of Bonn. He employed frogs and made use of the galvanic influence; Ann. Sc. Nat. t. xxiii. p. 95 et seq. Experiments of a similar kind have been performed by Professor Panizza; Ed. Med. Journ. v. 45. p. 87..97. We have an interesting case of the separation of the two powers in Dr. Bright's Med. Rep. v. ii. p. 548, 9; the patient in question had a paralysis of the motive nerves of the left extremity and the perceptive nerves of the right. I may be permitted, in connexion with this subject, to refer to a case, which fell under my own observation, where there was a complete loss of power over the voluntary muscles, while the other functions, *that depend upon the* nervous system, seemed to be

the nerves, and the appropriation of each function to a specific organ[1], we gain an analogical argument for the same kind of separation in the brain, and this conjecture would appear to be sanctioned by the experiments of Flourens and Rolando.

Sir C. Bell, in connexion with his discovery of the distinct functions of the two kinds of nerves, has brought forwards a new series of very interesting anatomical facts, which throw considerable light upon their mode of action and their connexion with the other parts of the system. There are certain circumstances in the anatomical structure and the distribution of the nerves which led to the arrangement of them into two classes, and which indicated that they each serve different purposes in the animal œconomy. From the situation of these two sets of nerves with respect to their origin and to each other, Sir C. Bell has given them the names of symmetrical or original, and irregular or superadded. The first set, which

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little, if at all affected; Med. Chir. Tr. v. ix. p. 1 et seq. The occurrence took place twenty years ago, before the distinction between the motive and the perceptive functions of the nervous system had been recognized. Although scarcely any morbid appearances were detected, on a careful examination of the brain and the parts immediately connected with it, which could account for the symptoms, it affords a very decisive example of the complete separation of the two powers. The above case, in many of its leading symptoms, presents a great resemblance to a very remarkable one detailed by Rullier, where the motion of both the upper and lower extremities was completely lost, while their sensibility remained unimpaired. Here, however, it was found upon examination after death, that some inches of the spinal cord were completely destroyed; Magendie's Journ. t. iii. p. 173 et seq. How far any destruction of a similar kind took place in the first case it is now impossible to decide, but it may be remarked, that during the life of the patient, the state of the spine was frequently attended to, and that no external indication of disease could be detected.

[1] The researches of Professor Bellingeri on the structure of the spinal cord, of which some account was given above, serve to illustrate, in a remarkable manner, the doctrine of the separate offices of the different nerves, and at the same time to point out the immediate cause of this difference in their properties, as being connected with the different parts of the cord. Magendie had suggested the idea, that the two functions of the nervous system were connected respectively with the pillars of which the cord is composed; Journ. t. iii. p. 153. Bellingeri's experiments have led him to conclude that the anterior pillars are more immediately connected with the flexion of the limbs, and the posterior with their extension, and he has applied this doctrine to the cerebrum and cerebellum themselves, in consequence of their supposed connexion with the two parts of the column; see his treatise entitled Exper. Physiol. in Med. Spin., sub finem. This distinction we may presume is, in some degree, referable to the circumstance, that the motions of flexion are more immediately connected with the contraction of the muscles, those of flexion with their relaxation. I must observe, however, that on this point the opinion of Bellingeri differs materially from that of Bell and Magendie. In the third plate of Sir C. Bell's work on the nervous system we have a view of a part of the spinal cord, with the spinal nerves proceeding from it, in which their structure is well exhibited. His latest views of the structure of the spinal cord and of the connexion of its several parts with the different portions of the encephalon are contained in his two papers read to the Royal Society in 1834 and 1835. I may remark that his descriptions do not in all respects coincide with those of Bellingeri.

might perhaps be called more appropriately, the general nerves, consists of the fifth pair of the cranial and all the spinal nerves[1]: they have double roots, one of which is connected with ganglia; they pass laterally to the two halves of the body, the two sides having no connexion with each other, and they are distributed to all the muscular parts that are under the control of the will. They appear to be the organs of perception and volition, deriving, as we may conjecture, these two functions from their double roots[2]. These we may regard as that part of the nervous system which serves the purpose of establishing our connexion with the external world.

Sir C. Bell's second set of nerves proceed by single roots from the base of the medulla oblongata, or the parts immediately connected with it; they proceed in a much more irregular manner than the former, and are distributed to all the organs which are concerned, either directly or indirectly, in the function of respiration. From this circumstance they have received the denomination of respiratory nerves, as well as that of superadded or irregular. Their course is designated by this last term, as they pass from one organ to another in the most intricate manner, connecting them together, passing across the general nerves, occasionally uniting with them, and forming the connecting link between the two halves of the body. These nerves are not under the control of the will, and are not capable of exciting perception; they are, therefore, furnished only with the faculty of transmitting the nervous influence, or with what Dr. Philip styles nervous power, in opposition to sensorial. Some very curious and important pathological deductions have been made by Sir C. Bell, from the new views which he has given us on the subject of the nerves, and we have also a number of additional remarks by Mr. Shaw, which confirm and illustrate Sir C. Bell's doctrines, and give us reason to expect that they may be applied with great advantage to the practice of medicine and surgery [1].

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[1] With respect to the other cranial nerves, the 1st, 2d, and the portio mollis of the 7th pair have distinct functions, connected with the organs of sense to which they are destined, while the 3d, the 4th, the 6th, the portio dura of the 7th, and the 9th, are more strictly a part of the symmetrical system, and are more especially appropriated to motion; the 8th pair belongs to the irregular class. I conceive that a part, at least, of the controversy which has taken place with respect to the 5th pair of nerves and the class in which it should be placed, according to the modern doctrines, depends upon its having been described as one pair, which divides into various branches; whereas the branches are really distinct nerves, dissimilar in their structure and destined for different functions, being connected merely by the accidental occurrence of proximity. This circumstance is clearly pointed out by Bellingeri, in his Dissert. Inaug. p. 2. p. 41 et seq.; see also the Ed. Med. Journ. v. xlii., and Mayo's Com. pl. 2. p. 7 . . 18, for an account of Sœmmering's opinion. The origin of this nerve is well represented in Sir C. Bell's work on the nervous system, pl. 8. In Dr. Milligan's notes to his translation of Magendie, we have a tabular view of the classification of the nerves according to the systems of Bell, Magendie, and Desmoulins; p. 549, 0. See also Quain's Anat. p. 95, 6.

[2] Prof. Mayer *conceives that a double root* may be demonstrated in the *pneumogastric, and in some other nerves,* where it had not been previously *supposed to exist; Ed. Med. Journ. v. xliii. p.* 485 . . 7.

The result of our observations upon the nervous system and its functions is, that it has two distinct powers, that of receiving and transmitting impressions, which is exercised by the nerves and spinal cord, and that of perception and volition, which is more immediately exercised by the brain [2]. Upon this principle Blumenbach has arranged the organs of these functions into the two classes of sensorial, comprehending the brain and its immediate appendages, and the nervous, properly so called, including the nerves, the plexuses, and the ganglia [3]. The sensorial organs are the exclusive seat of the powers of perception and volition, and of the intellectual faculties, while the office of the nerves is to serve as media of communication between the common centre and the organs of sense and motion [1].

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[1] See Bell, in Phil. Trans. for 1821, p. 398 et seq.; also Phil. Trans. for 1822, p. 284 et seq.; and Shaw, in Quart. Journ. v. xiii. p. 120; and Med. Chir. Trans. v. xii. p. 105; Lond. Med. Phys. Journ. v. xlviii. p. 343, and 457; v. xlix. p. 449. Sir C. Bell, in two papers in the Phil. Trans. for 1823, p. 166 and 289, has made a beautiful application of his principle to illustrate the respective offices of the different nerves that are sent to the eye and the parts immediately connected with it, and in a later paper, in the Phil. Trans. for 1829, p. 317, he has still farther illustrated the functions of the various facial nerves. Experiments similar to those of Sir C. Bell have been performed by Mr. Mayo, but with a different result as far as regards the respective powers of certain nerves with relation to the faculties of perception and motion. These differences principally refer to the functions of the portio dura of the 7th and the various branches of the 5th pairs; Comment. p. 107 et seq., and part 2. p. 2 et seq.; Outlines, p. 258 et seq. It is to be regretted that such discrepancies should arise, especially in the case of two individuals, who have conferred such substantial benefits on the science of physiology; but where truth is the only object of the inquirer, they will be, in no long time, reconciled by multiplying our observations and experiments. The acute mind of J. Hunter enabled him to detect the general principle of the specific powers of different nerves when sent to the same organ; Animal Œconomy, p. 262; but the merit of demonstrating the truth of the principle, and of its successful application, rests with Sir C. Bell. We have an excellent summary of the discoveries that have been made on this subject, and the opinions entertained respecting the different functions of the nerves, in Dr. C. Henry's Report, referred to above, p. 80 . . 91. Dr. Alison has given us a perspicuous account of this division of the nerves, in his Physiol. p. 130, 1. 141, and 161. He has offered some remarks upon the appropriation of the term respiratory to the 2d division of the nerves, and I think has been successful in showing that it is not correct, and that the anatomical connexion which subsists between these nerves is not sufficient to explain the sympathy which exists between the different organs that are concerned in the function of respiration; Ed. Med. Chir. Tr. v. ii. I think it due to Dr. Alison, to remark, that in criticizing the opinions of Sir C. Bell, he pays a high tribute to his merits as an anatomist and a physiologist, and that his observations are delivered in that candid and philosophical spirit, which indicates a mind bent rather on the attainment of truth, than on the mere establishing of a particular set of opinions. I may remark in this place, that Mr. Newport's elaborate researches into the anatomical structure of the genera Sphinx and Papilio have shewn, that these animals possess the same division of the nervous system, and the same appropriation to distinct functions, which Sir C. Bell has demonstrated in the higher animals; Phil. Trans. for 1834, p. 405 *et seq.*, with the accompanying plates.

[2] *Dr. Philip*, in Quart. Journ. v. xiv. p. 93.

[3] *Instit. Physiol.* sect. 198.

The great superiority of the intellectual faculties of man over those of other animals, has induced anatomists to investigate whether there be any thing in his anatomical structure which would serve to account for this superiority. The great size of the human brain, compared to that of other animals, was noticed by the ancients, and Aristotle laid it down as a general principle, that the faculties which were referred to this organ were in proportion to its size, compared with that of the whole body. This rule holds good with respect to many of the domestic animals, which were the best known to the ancients, and upon which we may presume that their observations were made. For example, the brain of a man, according to the calculation of Monro, is four times that of an ox, although, upon an average, the body of the ox is six times the size of the human body [2]. But there are many exceptions to this rule. It has been found by the accurate researches of the modern anatomists, that in some of the mammalia, the proportion of the size of the brain to the body is equal to that of the human subject [3], and that there are certain species of birds in which the proportional size is even greater. It further appears that some animals, which are remarkable for the comparative perfection of their sensorial powers, have the brain below the average size, as the horse and the elephant. In man, the ratio of the weight of the brain to that of the whole body has been stated at an average at about $\frac{1}{28}$, in the dog it is about $\frac{1}{160}$, in the horse $\frac{1}{400}$, in the elephant $\frac{1}{500}$ only, while on the contrary, in several of the small singing birds, and particularly in the canary, the brain is above the average of man, being as much as $\frac{1}{14}$[1], and Ebel mentions a kind of simia, where the proportion is even $\frac{1}{11}$[2].

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[1] What is stated in the text is, perhaps, all that we are warranted in concluding from the facts which are at present clearly ascertained; but it is almost impossible to proceed so far without forming some conjectures, or proposing some queries respecting what remains still to be discovered. Sir C. Bell's first set of nerves, i. e. the fifth cranial and the spinal, possess the function of communicating both perception and volition, and as they arise from double roots, it is not unreasonable to infer, that the two roots serve respectively for the two powers. But these nerves are distributed both to the voluntary muscles, and to the skin and the internal surfaces; has any anatomical difference been detected between the nerves of the two orders of parts, the one possessing the double function, the other merely serving for the transmission of perception? Sir C. Bell's second division of nerves, the irregular, are not capable of communicating either perception or volition, but serve to transmit the nervous influence from one part to another; it includes what he names the respiratory nerves and the intercostal system, and is connected in an indirect manner only with the brain, while it is principally to these nerves that the ganglia are attached. Are these nerves capable of transmitting their influence in both directions? Is it probable, that besides their ordinary office, these nerves, on certain occasions, are capable of conveying perceptions, and that the ganglia are the parts to which the perceptions are referred; for example, the perceptions of internal diseases? Besides these two classes of nerves, we have a third kind, those appropriated to specific perceptions, as the optic, the olfactory, &c.; what is there peculiar in their anatomical structure? Do not the mere *perceptive* parts of the first class belong to this division? Have not the nerves of the surfaces more analogy, at least in their functions, with these nerves, than with those which transmit volition? What relation do the nerves that convey simple sensations of pleasure and pain bear to the other part of the nervous system? The further consideration of many of these points obviously belongs to a subsequent part of the work. In order to avoid circumlocution it might be convenient to give the names of simply sensitive, perceptive, and motive, to the three kinds of nerves, according as they respectively serve for the offices of the mere transmission of nervous influence, for perception, and for voluntary motion.

[2] On the Nervous *System*, p. 25.

[3] *Buffon, v. ix. p. 247, says that the brain of* the seal is larger in proportion *to the body of the animal than that of the* human subject.

But an observation has been made by Sœmmering, to which hitherto no exception has been found, that the perfection of the sensitive functions does not depend upon the absolute size of the brain, nor upon its proportion to the body at large, but upon the proportion between the size of the brain and the aggregated bulk of the nerves that proceed from it[3]; or according to Blumenbach's nomenclature, between the sensorial and the nervous organs. As an illustration of this position the example of the horse is cited; the absolute size of the brain of the horse is only about half the size of the human brain, while the mass of the nerves of the horse at their origin is no less than ten times larger than that of man. And as we pursue our researches into comparative anatomy, we find that we are able, in most cases at least, to trace a correspondence between the perfection of the respective functions and the physical condition of the organs. Most of the inferior animals have larger nerves, and possess some of the nervous functions in a much more acute state than man, but man decidedly excels them all in the comparative size of the brain and in the perfection of his intellectual functions[4].

Ever since the time of Willis the proportion of the cerebrum to the cerebellum is a point that has been attended to by anatomists, as marking a difference in the degree of perfection of the nervous system, and it has been asserted, that the proportion of the cerebrum to the cerebellum is greater in man than in any other animal. But although this holds good in most instances, it appears from Cuvier that there are some exceptions, and, as is often the case in comparative anatomy, we find that in those points where man differs most from other animals, there are some species of simiæ which resemble the human subject. In man the weight of the cerebrum is to that of the cerebellum as nine to one, in the dog as eight to one, in the horse as seven to one, in the cat as six to one, in the sheep as five to one, while, on the contrary, in a certain species of monkey, the proportion is as much as fourteen to one[1]. It may seem somewhat remarkable that, although, in the higher classes of animals, we find a general ratio between the perfection of the nervous system and the large size of the cerebrum, compared with that of the cerebellum, yet when we descend lower in the scale, we meet with some case in which the cerebellum is entirely wanting. In these cases, however, the general form of the brain differs so much from that of man, and the animals that resemble him, that it is not easy to say what are the parts most analogous to each other, or to what portions of the brain the different appellations ought to be applied.

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[1] Cuvier, Leç. d'Anat. Comp. t. ii. p. 149 et seq. See also Lawrence's Lect. p. 191.

[2] Observ. Neurol. ex Anat. Comp. in Ludwig, Scrip. Neur. t. iii. p. 150.

[3] Corp. Hum. Fab. t. iv. § 92; de Basi Enceph. p. 14; et Tab. Bas. Enceph. Cap. 1. p. 5..11; Lawrence's Lectures, p. 192 et seq; Blumenbach's Comp. Anat. by Lawrence, p. 292. Prof. Carus, in his comparative examinations of the nervous systems in the different classes of animals, insists upon the greater proportion which the brain bears to the spinal cord in the human subject; Comp. Anat. by Gore, p. 273..7.

[4] Prof. Tiedemann, as I have already observed, has pointed out an interesting analogy between the state of the nervous system in the human fœtus and in the inferior animals, as to the respective state of its two great divisions. In the earlier stages of existence the spinal cord predominates, while the brain is relatively increased in size in proportion to the development of the various parts of the young animal.

Another comparison has been instituted, which gives a more constant superiority to the human subject, the proportionate size of the cerebrum to the medulla oblongata, although, in this instance, as well as in the former, the simiæ are found to be more analogous to man than to any other animals[2].

All these comparative observations are deserving of attention, but we may remark concerning them, that we might, *a priori*, expect the powers of the nervous system to depend as much, at least, upon the perfection of its organization, as upon its mere bulk, or upon the proportion between the size of its different parts. It does not appear that any very accurate comparative observations have been made upon the minute structure of the brain in the different classes of animals, nor perhaps ought we to expect to be able to discern any visible difference between them; but it is well known that the greatest variety exists in their general figure and anatomical structure, and although we are very frequently unable to trace the connexion between the configuration of parts and their respective functions, we may reasonably infer that such a connexion actually exists[3].

The only further observations that I shall offer on the comparative uses of the different parts of the nervous system are concerning the appropriate office of the ganglia. From the mode in which they are composed, it appeared a natural conclusion, that one office which they perform is to produce a more complete connexion and sympathy between the sensations of different parts; but this, as far as appears, might have been accomplished by the simple union of the nervous filaments, as occurs in the plexuses, without the additional apparatus which we observe in the ganglia; some eminent anatomists, as Winslow, Willis, and Vieussens, supposed them to be small brains, or independent sources of nervous power and central spots, to which perceptions are referred. This opinion is embraced by Richerand[1] and by Cuvier[2], and a doctrine essentially similar is maintained by Bichat[3], who conceives them to be the nervous centres of the organic functions. Lancisi had a fanciful notion that they promoted the flow of the animal spirits along the nerves, by a kind of muscular action[4]; Johnstone supposed that their use is to render the organs which derive their nerves from them independent of the will[5], and it has been lately conjectured that their office is to recruit the nerves that pass through them, or to add to their substance, in the same manner as the cortical part of the brain has been conceived to generate the medulla. Dr. Philip considers the ganglia as secondary nervous centres, the specific office of which is to receive supplies of the nervous influence from all parts of the brain and spinal cord, which are the active parts of the system, and transmit this collected influence, by means of the nerves, to the organs, where it is required, while at the same time, they serve to combine into one whole the different parts of the nervous system[6]. But the difficulty which has been already alluded to occurs in this hypothesis, that this combination of nervous influence, so far as we know, might have been accomplished by the mere union of the nervous filaments[7]. Upon the whole, I apprehend we must acknowledge, that the specific office of the ganglia has not been discovered.

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[1] Leç. d'Anat. Comp. t. ii. p. 152 et seq.; Blumenbach's Comp. Anat. by Lawrence, p. 312. The experiments of Sir Wm. Hamilton, referred to above, confirm the fact, as to the exceptions from the general rule being not unfrequent.

[2] Leç. d' Anat. Comp. t. ii. p. 152 et seq. The dolphin forms the only exception to this rule among the animals upon which observations have been made.

[3] This inquiry into the comparative anatomy of the brain has been prosecuted with much diligence by *Serres, in the* work to which I have referred above.

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[1] Physiologie, t. i. p. 108.

[2] Leç. d'Anat. Comp. Intr. p. 26. The author remarks that the ganglia are larger and more numerous when the brain is deficient in size.

[3] Anat. Gén. t. i. p. 200; t. ii. p. 405.

[4] Morgagni, Advers. Anat. pars. 5. p. 113.

[5] Essay on the Ganglia, p. 19.

[6] Inquiry, p. 170 et seq.; Phil. Trans. for 1815, p. 436; Quart. Journ. v. xiii. p. 266; Phil. Trans, for 1833, p. 55 et seq.; see especially his general conclusions, p. 70, 1. This opinion is also embraced by Dr. Roget; Bridgewater Treat. v. ii. p. 359, 0.

[7] For a concise view of the various opinions that have been entertained upon this subject, see Sœmmering, Corp. Hum. Fab. § 161; also Béclard, Anat. Ch. 10. § 3. In connexion with the physiology of these organs, it is important to refer to the late investigations of Sir C. Bell and others, from which we learn, that the nerves of sensation have ganglia at their roots, while those of motion are without them.

Sect. 4. *Connexion between the Muscular and Nervous Systems.*

Having considered the properties and uses of the nervous system, I proceed to inquire into the nature of the connexion between the nerves and the muscles, or, more generally, between sensation and motion. After Haller had clearly established the difference between the two essential properties of animal life, contractility and sensibility, and pointed out their mode of action, it was universally admitted that the former of them is the immediate cause of motion and the latter of sensation[1]. It was obvious that motion was, in many cases, necessarily accompanied by sensation, but the question then arose, whether it was so in all instances. The solution of this question was attended with many difficulties, and eventually gave rise to one of the most animated controversies that ever took place in physiology, and which, although no longer pursued with any degree of acrimony, subsists to the present day.

The point at issue may be thus stated. When a stimulant acts upon a muscular fibre, so as to produce contraction, does it act immediately upon the fibre itself, or does it not always act through the intervention of a nerve? The nerves are the organs of sensation; when, therefore, a muscle receives the impression of a stimulant, is not this impression always, in the first instance, received upon the nervous matter distributed through the muscle, and the impression then transferred from the nerve to the muscular fibre? Haller and his disciples thought the intervention of the nerves not to be necessary, but supposed that the irritability of the muscle, as they termed it, was, in many cases at least, alone concerned, and that, consequently, stimulants were capable of acting upon the fibre itself. His opponents, of whom Whytt was one of the most active and zealous, supported the contrary doctrine, and maintained that the muscular fibre is merely an organ of motion, that it is incapable of receiving impressions from external objects, and never contracts, except through the intervention of the nervous power[2]. This hypothesis was embraced by many of the French writers, particularly by Senac, who became one of its most able defenders, as well as by the colleagues of Whytt, Cullen, and Monro; and it was the doctrine generally embraced in the University of Edinburgh, then approaching to its most splendid period of reputation[1]. The term irritability, which had been originally used by Glisson in a physiological sense, and had been adopted by Haller, was employed by the two parties in a somewhat different meaning, a circumstance which it is necessary to bear in mind in examining the merits of the controversy. By Haller it is intended to express the power inherent in the muscular fibre (its vis incita) of being excited to contraction on the application of a stimulant; according to Whytt it means the faculty which the muscular fibre possesses of receiving impressions transmitted to it from the nerves. The experiments and arguments that have been adduced in this controversy, in support of the two opposite opinions, have filled many volumes; I shall only have it in my power at present to notice a few of the leading facts that have been stated, and the general scope of the reasoning that has been employed[2].

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[1] This was the principal object of his "Mém. sur la nature sens. et irrit. des parties du corps animal;" it contains a number of experiments performed by himself and by his friends and pupils, for the purpose of illustrating and establishing his doctrine; among the principal contributors are, Zinn, Zimmerman, Fontana, Tissot, and Caldani.

[2] On *Vital and Involuntary Motions*, sect. 1. p. 10 et alibi.

The great argument of Haller was an appeal to the anatomical structure and obvious powers of the animal body. He made a distinction between the contractile and the sensitive organs, and he endeavoured to point out to what parts of the system these terms were respectively applicable[3]. He pointed out some which were extremely contractile, but were only scantily supplied with nerves, and it was found generally, that the contractility of parts did not bear any ratio to their sensibility, or to the quantity of nerves sent to them. The example of the heart was adduced in proof of this position. Indeed, so little sensitive is this organ, not merely in its sound, but even in its morbid state, that there are many well-known instances, where it is found to have undergone great alteration in its structure, to have had its parts condensed by the effect of inflammation, or even destroyed by suppuration, and yet no pain has been felt, and nothing unusual has been experienced, except a sense of oppression, and this not depending so much upon the condition of the heart itself, as upon the difficulty which it had in transmitting the blood along the arteries.

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[1] Willis, who was one of the first that minutely attended to the operations of the nervous system, conceived that all motion depends upon, or originates in, the nerves, and that the nerves of the voluntary and involuntary organs are derived from the cerebrum and cerebellum respectively. Mayow, Boerhaave, and other eminent physiologists of the latter part of the seventeenth and the beginning of the eighteenth century, adopted Willis's hypothesis, and it may be considered as the prevailing opinion until the time of Haller. A well digested summary of Cullen's doctrines on the functions of the nervous system is contained in Dr. Thomson's work, p. 269..325.

[2] A well-digested and correct state of the hypotheses that prevailed on this subject before the time of Haller, and of his own doctrines, will be found in a report made to the French Institute on the experiments of Legallois by Percy, Humboldt, and Hallé; see Ed. Med. Journ. v. x. p. 207; and Philip, in Quarterly Journ. v. xiii. p. 98; also Dr. Cooke's introduction to his valuable treatise on nervous diseases. Cabanis, an eloquent and acute writer, and the nature of whose work might be supposed to lead him to a degree of logical accuracy, has, notwithstanding, chosen to regard this controversy as merely a verbal one, but I believe that I shall be justified in stating that he has completely misunderstood the nature of the argument. See Rapports du Phys. et Mor. de l'homme, t. i. p. 90, 91, (2d edit.) For a more particular account of Whytt's opinions, and for the share which he took in the discussions of his times, I may refer to Dr. Thomson, p. 241..258.

[3] *Mém. sur les Part.* Sens. et Irrit. Opera Minora, t. i. p. 289.

In the second place it was urged as a powerful argument in favour of the Hallerian doctrine, that muscular parts remain contractile for a long time after they are removed from the body, and when their communication with the brain is of course destroyed[1]. This is especially the case with respect to the heart, which in frogs and in cold-blooded animals generally, for many hours after its separation from the body, will still contract on the application of a stimulant; but how, it is asked, can the nervous system be concerned in this case, since the centre of the sensibility is removed?

A third set of facts, which were supposed to be still more in favour of his doctrine, was brought forwards by Haller, consisting of the accounts of fœtuses that were born with imperfect brains, or even altogether without heads, and yet had grown to their full size in the uterus, and after birth exhibited many marks of contractility, being capable of moving their limbs upon the application of stimuli, of evacuating the contents of the bladder and intestines, and, in short, of exercising the accustomed functions, as far as was consistent with their imperfect organization[2]. Indeed, it would appear that, if we could have supplied the materials for digestion, life might have been prolonged to an indefinite period. This point has been lately insisted upon by Dr. Philip, in his essay on the connexion between the muscular and the nervous powers. From some experiments that were performed by Legallois on this subject, it seemed to be proved, that the motion of the heart was independent of the brain, yet that there was a necessary connexion between the heart and the spinal cord[3]. But the experiments of Dr. Philip have demonstrated, that the spinal cord is no more necessary than the brain for the motion of the heart. He found that the total destruction both of the brain and the spinal cord did not prevent the contraction of the heart from proceeding in its ordinary manner, provided its other connexions with the system were duly maintained; he likewise adduces cases, apparently well authenticated, of fœtuses born in a state of maturity, where the spinal cord, as well as the brain, was totally wanting[4].

In the fourth place, the Hallerian doctrine has been supposed by some physiologists to receive very powerful support from the constitution of the lower classes of animals, such as the Actiniæ, and others of the larger zoophytes, which are of considerable size, and are endowed with a great degree of contractile power, yet in which the most minute researches have not been able to detect a nervous system. The limbs of these animals contract upon the application of a stimulant, and of course the contraction is brought about without the intervention of nerves, but as it appears to be precisely similar to that of animals that have a nervous system, so we may conclude that in both cases the process is conducted in the same manner, and according to the same general laws.

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[1] Haller sur les Part. Sens. et Irrit. t. i. p. 48 et seq.

[2] A more particular account of these will be given in a subsequent part of the work.

[3] Sur le Principe de la Vie, p. 138 et seq. et alibi.

[4] Philip's Inq. *p. 62.* *Lawrence, in Med.* Chir. Trans. v. v. p. 168 et seq.

A fifth argument in favour of the Hallerian doctrine has been derived from the order of time in which the different parts of the body come into existence and acquire their full powers. By minute observations made upon the fœtus, and more especially upon the chick in ovo, during the earliest periods of their existence, we learn that the formation of the heart precedes that of the brain; that the first distinct indication of life is a small beating point, the punctum saliens, as it has been called[1], which afterwards becomes the muscular substance of the ventricles; from this the large vessels gradually expand, and it is not until after some time that the brain becomes organized[2]. The successive periods at which the functions of the parts respectively commence their actions agree with these observations. The contractions of the heart proceed with perfect regularity long before we can trace any sign of the operations of the nervous system, and we find a great variety of involuntary muscular motions performed in the most perfect manner immediately after birth, while it seems necessary that a considerable length of time should elapse before any of the nervous functions are capable of being exercised[3].

Besides the above arguments, all of which seem to be derived from legitimate grounds of reasoning, other considerations have been adduced in favour of the Hallerian doctrine, which are either in themselves of less weight, or have been since superseded by more correct scientific investigations. These it will not be necessary to notice, except indeed that derived from the analogy of vegetables, which Haller himself seems to have regarded as of considerable force. Plants, he said, exhibit evident marks of irritability, they possess spontaneous motion, and obey the operation of stimulants, yet they are without nerves, so that we have here a case in which irritability exists independent of a nervous system. But this is certainly an attempt to explain what is obscure by something which is still more so; we know less of vegetable than of animal life, and when we speak of the irritability of vegetables, we employ a metaphorical expression to denote a quality, with the nature of which we are totally unacquainted.

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[1] Harvey de Generatione, Exerc. 17.

[2] From the recent observations of Sir E. Home we learn, that the rudiments of the brain and spinal cord are visible before the heart, but it may be argued, that the nervous system is at this period incapable of performing any of its functions; Phil. Trans. for 1822, p. 342.

[3] Haller, El. Phys. iv. 4. 28. On this subject I may refer to the opinion of Blainville, as detailed by Adelon, Physiol. t. i. p. 223 et seq. See also t. iv. p. 353, for the opinions that have been entertained by various physiologists respecting the successive development of the parts of the fœtus.

I must not omit to mention an argument, that would have great weight in favour of the independent contractility of the muscular fibre, were the fact on which it is founded fully established. I allude to some experiments which were performed in Italy a few years ago, on the application of galvanism to the fibrin of the blood immediately after coagulation, in which it was stated that a contraction was produced in this substance similar to that of the muscular fibre. The strong analogy which exists between the properties of the two kinds of fibrin would no doubt go far to demonstrate, that if contraction could be produced without the intervention of nerves in the one case, it might be so likewise in the other. Unfortunately, however, the fact in question is not sufficiently authenticated; the experiment, when performed in this country, has not succeeded, and although a negative experiment is not to be considered as of itself of equal force with a positive one, yet, upon the whole, the preponderance of evidence seems to be against the contractility of the fibrin of the blood[1].

Powerful and direct as the arguments may seem which were adduced by Haller and his disciples, in support of the doctrine of the independent action of the muscular fibre, the neurologists have not been remiss in replying to them, and they have further supported their side of the question by arguments, which, by many physiologists, have been thought even more convincing. In opposition to Haller they have likewise appealed to the anatomical structure of the body, and have particularly insisted upon the general diffusion of nervous matter through the substance of the muscles. Although we are not able to trace the nerves to their extreme ramifications, yet it is contended that they are in fact distributed to each individual fibre, for when we insert into a muscular part the finest point of a needle, if it be only sufficient to produce a contraction, it will excite a corresponding sensation. Indeed some of the ablest defenders of this side of the question, and particularly Smith, seem disposed to rest the issue of the question upon this sole point, whether it be possible to discover any part which is contractile, and which is, at the same time, not furnished with nerves. It seems, however, impossible to put it to the test of direct experiment, for although we might have it in our power to obtain the muscular fibre in a separate state, and might even, by the diagnostic characters pointed out by Fontana, accurately distinguish between the muscular and the nervous filaments, yet in employing the mechanical means necessary for their separation, all their vital properties would be unavoidably extinguished. With respect to the heart and the other parts which are scantily supplied with nerves, and yet possess a great degree of contractility, it was said that in these cases, the quantity of nerve, although small, is in proportion to the nature of the stimulant, and the degree of effect required, and, in short, that the nerves of the heart are adequate to the action of the organ. Then it is argued that the heart is furnished with nerves, although small, and if they are not employed in producing its contraction, for what purpose are they destined?

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[1] See also Berzelius on the progress of animal chemistry, p. 20, where we are informed that the apparent contraction of fibrin upon the application of electricity depends upon the shrinking of the body connected with its coagulation: it is not improbable that *the process* may be promoted by electricity.

With respect to the power which individual muscles possess of retaining their contractility when separated from the body, the neurologists certainly appear to have been much puzzled to account for the phenomena, and they were reduced to the necessity of adopting the opinion, which appears to involve a gross inconsistency, that the sentient principle is divisible [1], or rather that there is no sensorium commune. This inconsistency they could only escape by adopting the Stahlian hypothesis of the soul being co-extended with the body [2], as they termed it, or that the different parts of the body were able individually to perceive the effect of stimuli applied to them. Senac explicitly states it as his opinion, that the nerves and spinal cord possess all the properties of the brain, only in a less degree, and that they are capable, for a limited time, of performing all its functions [3]. According to the hypothesis of the animal spirits, which was then generally adopted, he conceives that the spinal cord and even the nerves are capable of generating these spirits, and, in short, that the whole nervous system is, to a certain extent, the seat of perception [4]. The same kind of reasoning was applied to repel the arguments that were drawn from the existence of fœtuses without heads, in which case it was said that the seat of sensibility was only in part removed, and that what was left in the spinal cord, the nerves, and the ganglia, was still competent to carry on all the functions, which are exercised by these imperfect beings.

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[1] Whytt on Sensibility and Irritability, part 2, § 2. On Vital and Involuntary Motions, § 14. We may remark how directly the opinions of this learned physiologist lead to materialism, at the very time that he is arguing against this doctrine; a striking example of the inconsistencies into which we fall when we attempt to investigate topics that are beyond our comprehension, and a powerful motive for exercising the utmost candour towards those who differ from us on such abstruse points. Mr. Mayo, who is in general remarkable for the correctness of his phraseology, may be cited as an illustration of the same sentiment. In his observations upon this much agitated question, he uses the expression, "mind and matter are logically distinct substances," thus controverting his own position in the very enunciation of it. The candid and philosophical reflections with which he concludes the chapter cannot be too highly commended; Anat. Comment. p. 8, 9.

[2] See Stuart de Motu Muscul. § 5.

[3] This appears to be the opinion likewise of Scarpa; speaking of the nerves generally, he says that they possess a power independent of the brain, and cites the cases of acephalous fœtuses in proof of this point; Tabulæ Neur. § 22.

[4] Traité du Cœur, liv. 4. c. viii. These, and other similar speculations, may be regarded as analogous to Dr. Hall's hypothesis respecting what he *terms the reflex function.*

In connexion with this argument it has been observed, that the size of the brain and the spinal cord, in the different classes of animals, bears no proportion to each other; on the contrary, as we descend to the less perfectly organized classes, the brain diminishes, and at length entirely disappears, while the spinal cord is increased in size, as if for the purpose of supplying the place of the brain. These facts are well known, but the inference to be drawn from them is not so clear. Before we can form an hypothesis to account for the action of the nervous system, derived from observations on the comparative anatomy of the inferior animals, we must previously make ourselves acquainted with the nature of their functions, which we have many reasons for supposing to be very different from those of the human species. The acephalous fœtuses, which have been employed to show the independence of the muscles upon the nervous system, have been also brought forward to prove the independence of the nerves upon the brain, for it is said that, in these cases, the nerves are perfectly formed, or even sometimes larger than natural, as if intended to supply the deficiency of the brain. But here again, although we may admit the anatomical facts, the conclusion does not necessarily follow from them, for in these instances there is no unequivocal evidence of the existence of any functions, which the Hallerians conceive to be exclusively attached to the brain as distinct from the nerves, and although the nerves exhibited their natural appearance, and seemed to possess a perfect anatomical structure, we have no means of ascertaining in what degree they were capable of exercising their appropriate functions.

The uniformity of the action of the different fibres of which the muscle consists, or what has been termed the general consent of all its parts, has been regarded by some of the neurologists as an argument of much weight in favour of their doctrine. If only one fibre be irritated, the whole muscle instantly contracts; but how can this contraction be propagated from fibre to fibre except by means of nerves? This argument, however, implies a knowledge of the intimate nature of muscular contractility which we are not entitled to assume, and it can only be regarded as endeavouring to explain a difficulty by the adoption of an hypothetical principle, which itself stands in need of proof.

The argument in favour of the Hallerian hypothesis, drawn from the absence of nerves in the zoophytes, was answered by asserting, that we are not sufficiently acquainted with the structure of these animals *to make it the foun*dation of our reasoning, and it was even alleged, that the same argument might be employed to prove the existence of contractility without the muscular fibres; for a proper fibrous structure can no more be perceived in these animals than a nervous system. In the same manner it was stated, that the order of time in which the different parts are formed was a subject involved in too much obscurity to permit us to employ it in our reasoning. It was said that the heart might be sooner visible than the brain, in consequence of its motion, of its consistence, and its other sensible properties, but that it is impossible for us to decide which of the parts is actually first brought into existence[1].

So far I have attempted to give an abstract of the manner in which the neurologists replied to the reasoning of the Hallerians; they have also employed direct arguments in support of their opinions, which must now be stated. But before I enter upon this subject I may remark concerning the heart, which, as it appears, has been appealed to by the supporters of both sides of the question, as a proof of their respective doctrines, that the structure of the nerves of this organ has itself been a subject of controversy among anatomists, and has given rise to much learned discussion and minute investigations. According to Sœmmering[2], the nerves sent to the heart are much smaller in proportion to the size of the organ than to any other muscle, and it was even contended by Behrens[3], that these nerves are not destined for the muscular part of the organ, but for its large vessels, while, on the contrary, we have the no less respectable authority of Scarpa[4] in support of the opinion that the heart is furnished with nerves in the same manner with the other muscles of the body[5]. Until the anatomical question respecting the nerves of the heart be decided, it will be impossible to build any physiological hypothesis upon them, but the comparatively insensitive state of the heart is admitted by every one, and may be adduced as an objection to the doctrine of the neurologists, even were the existence of its nerves fully established.

The argument which was brought forward by the neurologists with the most confidence was derived from experiments made with artificial stimulants, the object of which was to show that, in a great number of the most unequivocal cases, they act upon the muscles through the intervention of the nerves. The experiments of Smith are some of the earliest, as well as the most decisive that were performed on this subject[1], and he found that exactly the same effects were produced, whether the stimulant was applied to the fibres themselves, or to the nerve that is distributed through them[2]. This he observed to obtain with respect to all the different kinds of stimulants, mechanical as well as chemical, and in these cases it is easy to detach the nerve from the surrounding parts, so that the application may be made to it in the most unexceptionable manner. The experiments that have been more lately made with galvanism tend to the same conclusion. Here, when a muscle is completely separated from the body, except by the intervention of a nerve, contractions are excited by transmitting the electric influence through the nerve, and it is always found that if a trunk of a nerve form part of a circuit, it is not the muscles in the neighbourhood of the nerve that are excited, but those to which the nerve is ultimately distributed, however distant they may be.

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[1] See note in p. 174.

[2] Corp. Hum. Fab. t. iii. sect. 32.

[3] Dissert. qua demon. Cor Nervis carere.

[4] Tabulæ Neurologicæ Card. Nerv. &c. These may be considered as, in all respects, among the best anatomical plates that were ever published. They are admirably expressive of the subject, without the gaudiness of the French engravers, who appear to aim principally at effect, or the tameness of the English, who seem to think of little else except œconomy. See also Cloquet, pl. 189.

[5] See also Senac's work on the Heart, liv. i. ch. 7, where we have a minute detail of the observations of various anatomists and physiologists previous to his publication, and liv. iii. ch. 8. sect. 5.

By reversing the nature of the experiment the reverse effect was produced. If instead of applying a stimulant we make use of a sedative, such as opium or laurel water, and immerse the nerve in it, the muscles to which the nerve is sent lose their contractility as entirely as if the sedative had been applied to the muscular fibres themselves. Facts of this kind are so well substantiated as to admit of no contradiction, and they undoubtedly seem to disprove one part of Haller's doctrine, that opium and other narcotics, when they affect the muscular power, operate through the intervention of the blood-vessels and not of the nerves. Now it is said that we have certain evidence of the intervention of the nervous power in a great variety of cases; it is therefore a reasonable and fair inference that the same intervention exists in all cases, and that muscular contraction never ensues from the application of a stimulant, except through the medium of a nerve.

But the facts which have been generally thought the most decisive against Haller are those in which we observe the muscular power to be affected by mental operations, such as the passions, and this is observed to be especially the case with respect to the heart, the organ which had been selected by the Hallerians, as one that acted independently of the nervous power, and was even thought to be entirely without nerves. And besides the observations that have been made upon the body, while in a state of health, numerous pathological occurrences daily present themselves to us, where the contractility of the muscles is obviously influenced by the state of the nervous system, or by agents which can only operate through its means.

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[1] From a MS. copy of Cullen's Lectures on Physiology, of which I am in possession, it appears that it was chiefly upon the authority of Smith's experiments, that he derived his hypothesis of the identity of the muscular and nervous fibres.

[2] De Actione Musculari, *passim.*

Other considerations of less weight or founded upon less certain grounds, have been urged by the neurologists, but what I have stated seems to me to constitute the most important part of their reasoning, and so convincing did it appear, that their hypothesis has been continually gaining ground, until it came to be almost universally adopted[1].

The opinions of physiologists on this disputed question were much influenced in favour of the hypothesis of the neurologists by the very elaborate and ingenious train of experiments which were performed by Legallois, to which I have already had occasion to refer. Haller had shown that the heart can continue its action when it is detached from the nerves that proceed to it directly from the brain, and had thence concluded that it is not under the influence of the nervous system. Legallois, however, endeavoured to prove, that if, besides the removal of the brain, we also destroy the spinal cord, the motion of the heart immediately ceases, and thus, by establishing, as he conceived, a necessary connexion between the action of the heart and the spinal cord, he controverted the argument that had been deduced by Haller in favour of the independent contractility of the muscles. The experiments by which Legallois supported his opinion were apparently so direct and unequivocal, as to gain very general assent to his doctrine, and were supposed completely to subvert that of Haller, when the subject was taken up by Dr. Philip, who, by repeating and modifying the experiments of Legallois, detected a source of fallacy in them, which, as far as this question is concerned, entirely destroys their value, and subverts the conclusion which had been drawn from them. Legallois had announced that when the spinal cord is destroyed, every part of the body loses its contractility, but Dr. Philip discovered that this is only the case when the cord is destroyed suddenly; on the contrary, when the destruction is effected slowly and gradually, he found that the heart was capable of continuing its contraction[2], and hence he infers the correctness of Haller's doctrine of the inherent contractility of the muscular fibre. And, indeed, if we allow the accuracy of Dr. Philip's results, respecting which there appears no reason to entertain any doubt, they afford us the most direct confirmation of Haller's doctrine of muscular contractility being a faculty which exists independently of nervous sensibility[3].

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[1] See Dr. Alison's remarks in the Quarterly Journal, v. ix. p. 106; also the report made to the French Institute, referred to above, p. 172. Cuvier says that the objections to Haller's doctrine are every day becoming more apparent; Leç. d'Anatomie Comp. Intr. p. 22.

[2] Experimental Inquiry, p. 88 and 97.

[3] In order to obtain a clear and comprehensive knowledge of the experiments and reasoning which were adduced in the course of this discussion, which may be considered as among the most curious and important that ever engaged the attention of physiologists, the following works should be carefully perused. Legallois' work, "Expériences sur le Principe de la Vie,"

Besides establishing the general fact, that the heart retains its contractility after it has been separated both from the brain and the spinal cord, Dr. Philip performed some further experiments for the purpose of proving that the contractile power of the muscles is independent of the nervous influence. The experiments consisted in employing the corresponding muscles of the two extremities of an animal, the nerves of one of which were divided, while those of the other remained entire. Both the sets of muscles were then thrown into strong contractions by the direct application of a stimulant, when it was found that the extremity in which the nerves were left entire lost its contractile power as soon or sooner than that in which the nerves were divided[1]. This result appears fully to justify Dr. Philip's conclusion, that contractility is an inherent power of the muscles, and does not depend upon any thing which is conveyed to them through the media of the nerves[2]. About the same time when Dr. Philip was engaged in his experiments to prove the independent contractility of the heart, some valuable observations upon the same subject were made by Mr. Clift, which led him to the same conclusion. They were made on the carp, and it was found the heart of this animal is capable of supporting its contractility for some hours after its complete separation from the brain and spinal cord[3].

In concluding the review of this controversy we may observe, that, on arguing upon this subject, writers in general have not sufficiently attended to a point, which was very explicitly stated by Haller himself, that the question is not whether the nerve generally intervenes between the action of the stimulant and the contraction of the muscle, but whether its intervention can, in any instance, be dispensed with[4]. I have already had occasion to describe the process by which volition is propagated from the brain down the nerve to the muscle, and in all these cases it is sufficiently obvious, that the action of the nerve is an essential part of the process. And this is not only the case

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which contains his original Experiments; Dr. Philip's two papers in the Phil. Trans. for 1815, in which he urges his objections to Legallois' conclusions, and states the main facts on which he founds his own opinion; his "Experimental Inquiry into the Laws of the Vital Functions;" and his series of papers in the Quarterly Journal, v. xiii. and xiv. consisting of his general conclusions and a digested summary of his doctrines. A very correct and elegant abstract of the experiments of M. Legallois and Dr. Philip, and the conclusions which may be deduced from them, is given by Dr. Roget in the supplement to the Encyclopædia Britannica, article "Physiology."

[1] Experimental Inquiry, p. 99.

[2] Phil. Trans. for 1815, p. 89.

[3] Phil. Trans. for 1815. The important and decisive experiments of Sir B. Brodie, which have been already referred to, although performed with a somewhat different object, bear very directly upon this point; Phil. Trans. for 1811, p. 36 et seq. See also Mr. Mayo's Comment. p. 16.

[4] Whytt, a writer of considerable acuteness and information, argues upon the principle, that as the intervention of the nervous system is necessary for the performance of certain muscular motions, it must be so in all cases; see the Essay on Vital *and Involuntary Motions*, passim.

in all voluntary motions, but also in all those motions, either voluntary or involuntary, where the stimulant is not directly applied to the part that is ultimately moved. Here the transmission of the effect is always through the nerve, and it appears, moreover, that in many instances it is impossible to produce the same effect in any way, except by means of the nerve. A great number, however, of the most important actions of the body are performed in consequence of the direct application of stimulants to the part that is to be moved. Of this description are the vital functions generally, where different substances, partly as it appears by their mechanical bulk, and partly by certain specific properties, cause the contraction of the muscular fibres to which they are applied. These operations are most of them involuntary, apparently placed out of the direct influence of the nervous system, and seem to be examples of the excitement of muscular contraction in consequence of the direct application of a stimulus. We accordingly find that this question has been much embarrassed by not attending to the difference between the voluntary and the involuntary muscles, a difference which was seldom adverted to by the earlier physiologists, although not totally unknown to them. This difference will be more fully explained when we discuss more particularly the nature of volition; but I may remark in this place, that although there are certain muscles that act habitually in both ways, and others which may, on certain occasions, act in a manner different from that which appears to be their ordinary mode, yet that there is a decided difference between the two classes, and that this distinction depends, in a considerable degree, upon the quantity of nerve that is sent to them, and the source whence the nerves are derived. The organs of voluntary motion are more plentifully supplied with nerves, and they are derived more or less directly from the brain and the spinal cord, while the involuntary muscles have fewer nerves, and those, for the most part, proceeding immediately from the intercostal nerve, or from some of the numerous ganglia with which it is connected[1]. The ordinary motions of these parts are not only independent of volition, but they are without consciousness, which always attends the contractions of the voluntary muscles, and they may be generally said to want those characteristic circumstances which mark the intervention of the nervous system, or which indicate it to have any connexion with or co-operation in the effect produced.

In discussing this question we are therefore entirely to discard the consideration of the voluntary muscles, and to disregard all the experiments that have been performed upon them, as it is admitted on all hands that they are placed under the direct control of the nervous system[1]. The only subjects of controversy are the involuntary muscles, and here the fact seems to be, that we have on one side the most decisive proof that these muscles, such, for example, as the heart, can act when entirely cut off from all connexion with the nervous system, while, on the contrary, we have no less decisive proof that they are capable of being influenced by causes which can only operate through the intervention of the nerves. This apparent difficulty may be removed, as Dr. Philip has clearly shown, by making a distinction between the ordinary and the extraordinary action of these parts. Under ordinary circumstances the heart contracts merely by the stimulus of the blood, in consequence of the inherent contractility of its fibres, but upon certain occasions, it is liable to feel the influence of passions and other mental emotions, which can only be conveyed to it through the intervention of the nerves[2]. What determines the operation of these extraordinary circumstances, and how they are connected with the involuntary muscles, will be the subject of future consideration; but in the mean time the facts must be admitted, and they seem to offer an easy explanation of the difficulty which has so long embarrassed this question.

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[1] Bichat, Anat. Gén. t. ii. p. 405; see also Johnstone on the Ganglia, *sect. 2.*

By not attending to this distinction between the voluntary and involuntary muscles, the greatest part of the experiments on the artificial application of stimulants, which had been regarded as among the most decisive arguments in favour of the doctrine of the neurologists, will be found to be totally irrelevant to the question. In the experiments of this kind the application has generally been made to the voluntary muscles, which are not properly the subject of the controversy. If, on the contrary, the involuntary muscles be employed, we shall obtain results that favour the Hallerian hypothesis. From obvious causes it is not so easy to perform experiments on the involuntary as on the voluntary muscles; but upon the whole it seems tolerably well established, that the nerves which supply these parts are not affected by the application of artificial stimuli, or rather will not admit of the transmission of the effect to distinct muscles, in the same manner with the nerves that go to the voluntary muscles. The experiments with the galvanic apparatus, which are the most easily performed, lead to this conclusion, either that the involuntary muscles are absolutely incapable of being stimulated by the application of electricity to the nerves, or at least in so small a degree as bears no proportion to the size of the nerves, or to the contractility of the part, when the stimulus is applied directly to its substance[1].

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[1] Dr. Philip, with his usual clearness and sagacity, has pointed out the difference which exists between the voluntary and involuntary muscles with respect to the cause which stimulates them into action. This, in the former, he states to be the act of volition *alone*; but this limitation I conceive to be liable to certain exceptions, although it is obvious that the will is their appropriate and ordinary stimulant; See "Inquiry," p. 101. If we refer to the classification of the nerves which is stated above, we should say that the voluntary muscles possess both perceptive and motive nerves, the involuntary muscles possess no motive nerves, and it is probable that their nerves are principally the simply sensitive, with a small proportion however of the perceptive. This consideration may lead to the conjecture, that the electric fluid is not capable of acting upon the nerves which serve for the purpose of simple sensation, but that both the perceptive and the motive nerves are under its influence. Is it not probable that the small degree of perception which the involuntary muscles possess is not employed in their ordinary action, but serves only to convey to us the knowledge of any unnatural or morbid state of the parts, and that the ganglia are the centres to which these perceptions are referred?

[2] Exper. Inq. p. 79. See Dr. Alison, p. 145.

The final result of the controversy appears therefore to be in favour of the doctrine of Haller, although in admitting it we are obliged to introduce certain modifications, which did not enter into his original hypothesis[2].

SECT. 5. *Arrangement of the Functions.*

These observations upon the connexion which subsists between the action of the muscles and the nerves will enable us to decide upon the plan which we ought to adopt in the arrangement of the functions. The older physiologists, and many of the most eminent among the moderns, have classed them under the three divisions of animal, vital, and natural; the first signifying those which are specifically attached to animal existence, such as immediately depend upon sensibility and contractility, including the various forms of sensation and spontaneous motion; the vital, denoting those that are directly concerned in the continuance of life, such as the action of the heart and lungs; and the third class, the natural, intended to express those which are not necessary for the immediate continuance of life, but which serve to maintain the body in its proper condition, and to support all the other functions; in this class are placed digestion, secretion, and absorption. There is a foundation for this division, but it is in some respects faulty, especially in there not being any precise limits between the two latter classes, the vital and the natural; and with respect to the nomenclature of the whole it is obviously incorrect. In the first place, every one of the functions may be strictly called animal, for in every one of them the operation either of sensibility or of contractility is immediately concerned. The animal functions are also strictly entitled to the appellation of vital, for not only are they directly essential to the support of life, but they are likewise the most characteristic of its presence; with respect to the term natural, as applied to any description of functions, it is clearly without any specific meaning.

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[1] For the effect of galvanism on the involuntary muscles, see my Essay on Galvanism, p. 49; Bichat, sur la Vie et la Mort, p. 120; Bichat, indeed, states the results of his experiments in a general way only, yet we may regard his statement as sufficient to prove the different effect produced on the two kinds of muscles; also Dr. Paris's observations on the effect of electricity on the action of the heart; Med. Juris. v. ii. p. 32. I must, however, observe that Dr. Park, arguing from the same premises, is led to form an opposite conclusion; Quart. Journ. v. ii. p. 233. Because it has been found in these experiments that the involuntary muscles are very slightly affected by the galvanic influence, he contends that there is no specific difference between them and the voluntary muscles. But in answer to the objection of this intelligent physiologist, I reply, that the extremely delicate sensibility of the one class, and the small share of it possessed by the other, bears no proportion to the quantity of nerves respectively sent to them, and can only be explained upon the supposition of some essential difference in the properties of the nerves themselves, or in their relation to the muscles.

[2] Philip's Inquiry, p. 103. Dr. C. Henry has lately advocated the doctrine of the neurologists; he states his reasons in a way which proves his thorough acquaintance with the subject, and the attention which he has bestowed upon it, yet I do not feel myself disposed to change the opinion which I had previously formed respecting it. I must, however, recommend the careful perusal of his paper to all those who are desirous of obtaining a correct knowledge of the merits of the controversy; Phil. Trans. for 1830, p. 65 et seq., and Ed. Med. Journ. v. xxxvii. p. 11 et seq.

Several new classifications of the functions have been lately proposed in France by physiologists, whose works, although of various characters, have each of them obtained a considerable share of reputation; of these I shall only notice at present those of Bichat and Cuvier.

Bichat, in this, as in most other cases, has disregarded, in a great measure, the opinions of his predecessors, and founded his system upon a new principle, which appears more philosophical, although not without considerable objections. He places all the functions in two classes, to which he gives the names of animal and organic, the first coinciding nearly with the animal functions of the older physiologists; the organic embracing both their vital and natural functions. The author himself characterizes his animal functions as those which distinguish the living animal from all other beings, those which enable him to maintain his connexion with the external world, originating from, or immediately consisting in, the faculties of sensation and locomotion. The organic functions are those which serve for the support of the body individually, without any reference to surrounding objects, which are performed by certain organs that are similar, or at least analogous, to what we find in every being that is possessed of organization. This division is preferable to the old one, in being more simple, and in not attempting to discriminate between the vital and natural functions, which are so closely connected together. But I conceive that it is not unexceptionable, either with respect to the actual nature of the functions, or to the names by which they are designated. All the functions are properly animal, for they are all sufficiently distinct, both in their prime cause and their phenomena, from any which are exercised by vegetables, and they are all entitled to the name of organic, for the brain and muscles are as properly organs of sensation and motion as the heart and lungs are of the circulation and the respiration [1].

Cuvier divides the functions into three classes, animal, vital, and generative; the first coinciding with the functions so named by Bichat, and the second being also similar to his organic functions, except in separating those that are subservient to generation. This method labours under the same objections with that of Bichat, although it is perhaps somewhat improved by the separation of the generative functions, which could not with propriety be included under the organic, according to the definition which he gives of them [2].

Besides the objections that have been urged to these different arrangements, considered individually, there is one that attaches to them generally, as well as to all the others that have been proposed, that they do not make a sufficient distinction between the functions immediately resulting from the action of the nervous system, as forming one of the constituents of the body, and the mental or intellectual operations. The brain is indeed the instrument of the mind, or the medium through which the mind acts, yet it does not necessarily follow, that the mind is to be regarded as a quality or property of the brain. But whatever may be our opinion upon this point, we may without hesitation assume, that the functions which result from the immediate action of the brain and nerves are altogether so different from those of the mind or intellect, that notwithstanding the connexion which subsists between them, they should be placed in distinct classes. Perhaps one cause why writers on physiology have not made separate classes of these two orders of functions may be, that they supposed the former of them alone to be the proper objects of their attention, and that the mental or intellectual powers belonged exclusively to the science of metaphysics. This, however, is no ground for employing a defective arrangement, and it may be farther remarked, that although the object of metaphysics be very different from that of physiology, yet the operations of mind and body are so intimately connected together, and the one part of our frame has so much power over the other, that no system of physiology can be perfect, in which the mental faculties are entirely disregarded.

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[1] See the remarks of Blandin, in the note, t. i. p. ciii, where he also gives an account of the arrangement of Royer-Collard.

[2] It may be worthy of observation, that Cuvier, who is so eminently distinguished for his sagacity in classification and arrangement, should have described sensibility and muscular contractility as both of them functions of the nerves; Règne Anim. p. 33. Adelon observes, that the different actions which constitute life are named functions; of these he enumerates eleven; sensibility, locomotion, language or expression, digestion, absorption, respiration, circulation, calorification, secretion, and generation; Physiol. t. i. p. 116. Bourdon's seven characteristics of life may be regarded, in like manner, as so many functions; they are as follows: "Caloricité, nutrivité, absortivité, exhalativité, durabilité, reproductivité, et resistabilité"; Princ. de Physiol. p. 28, 9. Dr. Elliotson adopts the arrangement of Bichat, Physiol. p. 20. Béclard arranges the functions into six classes; nutrition in its most extensive sense, generation, muscular action, "les sensations," "l'action nerveuse," and the intellectual functions; Elém. d'Anat. p. 100; but I conceive that he confounds what are properly termed functions with vital powers. Dr. Roget reduces the functions to four classes, which are named mechanical, nutritive or vital, sensorial, and reproductive. The first consist of the direct effects of muscular contraction, or something equivalent to it; the second, of those which serve directly for the growth and support of the body, comprising assimilation, circulation, respiration, secretion, excretion, absorption, and nutrition; the third class, of those in which the nervous system is primarily concerned, which serve to connect the individual with the external world; while the object of the fourth is the continuance of the species; Bridgewater Treatise, passim.

The mode of viewing the subject which appears to me the most correct, is to regard contractility and sensibility as the two primary attributes of animal life, each equally characteristic of it and peculiar to it, and each performed by its appropriate organs. The functions depend upon the exercise of these powers, and although probably, in all cases, they are both of them exercised, yet generally one of them seems to be the principal agent, or the prime origin of the ensuing operation; we may consequently divide them into the contractile and sensitive functions, or those which more directly belong to contractility and to sensibility, and which of course serve respectively for motion and sensation, and to these two classes, according to the remarks that have been made above, must be added the third class of the intellectual functions [1].

The contractile functions must be considered, in the first place, as being the most independent, and what may be regarded as the prime cause of the others, for we have seen that the muscular system can act without the intervention of the nervous, but that the nervous is directly dependent upon the muscular. The heart and the involuntary muscles continue to contract, and the limbs remain sensible to the impression of stimulants, after the brain and spinal cord are totally destroyed, but if the circulation and the respiration be stopped, the nervous power is immediately extinguished.

Among the contractile functions, the essence of which consists in motion, the first in point of importance, and the one which may be regarded as the most necessary to the direct support of life, and to the indirect maintenance of all the rest, is the circulation. In all animals, whose functions and mode of existence bear any considerable analogy or resemblance to the human subject, we observe that the fluid which serves for the support of the system, and from which the materials for its nutrition are immediately derived, is kept in continual motion. In the more perfect animals the great source of motion in the circulating fluid is the heart, but as we descend in the scale of organization, the heart becomes of less relative importance, and a part of its power is supplied by the action of the great vessels, until at length we arrive at an order of animals, where its power is entirely superseded by that of the large arteries, while in the lowest tribes, which possess any thing analogous to a sanguiferous system, the large arteries themselves are not to be distinguished, nor is there any regular progression of the fluids, and we can observe nothing more than a general oscillatory motion in them, which appears to be equally extended through all parts of the body. It must, however, be observed, that the perfection of the circulating system does not hold an exact ratio with that of the organization generally. Many insects that have various distinct organs, and a variety of delicate functions, have no circulating system[1], and the office of the heart and arteries is supplied by parts which act upon a different principle.

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[1] I conceive that Dr. Alison, in his account of the vital powers, has not sufficiently discriminated between the nervous and the intellectual functions, when, in reference to Bichat's doctrine of organic and animal life, he defines the two classes of phenomena as "those which do not imply the intervention or consciousness of the mind, and those in which some act of the mind is essentially concerned"; *Physiol. p. 5, 6, also p. 151.*

Next to the circulation, in point of importance, comes the respiration, the function by which the fluid that is carried along the vessels is adapted for the purposes of life, by a certain change which it experiences from the action of the air. This function, in some form or other, is probably exercised by all classes of animals. The same kind of complicated mechanism, which exists in the higher orders, is not indeed found in a great number of the inferior tribes, but in them we may trace something which is to be regarded as a substitute for it, or as supplying its place, by enabling the air to produce its appropriate action on the nutritive fluid. After these two functions, one by which the blood is carried to all parts of the body, and the other by which it acquires its vital properties, we come to those of calorification, secretion, digestion, including assimilation and sanguification, and absorption, functions which serve, in some way or other, for the continuance of the action of the animal machine, and which preserve all its parts in their proper condition, without, however, being essential to the immediate support of life. In this class we may place the function of generation, which, although one of the most inexplicable of all the operations that are performed by the animal powers, and acting in a specific manner, of which we have no other example, may be considered as essentially consisting in secretion.

Although I have spoken of some of these functions as being more essential to life than the others, still, when we take a general survey of the animal œconomy, we find that they are all of them intimately connected together, acting, as it were, in a kind of circle. The circulation has been stated to be the prime cause of all the rest, for it is that which carries to every part of the body the fluid which endows it with its vital properties and its appropriate powers. The heart, however, would be incapable of fulfilling its functions were not its fibres furnished with a regular supply of blood, which has been carried through the lungs, and is acted upon by the air in the process of respiration, which transmission of blood through the lungs is itself effected by the action of the heart[1]. If we conceive the heart and lungs, or some organ equivalent to them, to be in action, so that the blood may receive its proper alteration from the air, and may be carried to all parts of the body, we have the essentials of life, and are in possession of those functions by which existence may be maintained, and the individual preserved, for a certain length of time, in a perfect condition. Both the solids and the fluids of the body are, however, in a constant state of change. One essential office of respiration is to discharge a quantity of carbonaceous matter from the lungs; and the blood, after it has passed through the vessels, is incapable of continuing its office without a supply of fresh materials. The heart itself, the great source of all the motions of the body, and the centre of the circulating system, would lose its powers, were not its substance gradually renewed, and the waste that is thus going forwards from various causes counteracted by the functions of digestion and assimilation. But for the performance of these functions it is necessary that certain substances should be separated from the mass of blood, constituting the process of secretion, and, after the future nourishment is prepared and elaborated, absorption is necessary to carry it to its proper receptacles. All these functions are therefore as necessary for the continued support of the body, as the circulation and respiration are for its momentary existence, but they are so in a less direct manner, and are connected more through the medium of certain mechanical and physical changes, than in consequence of any changes which seem immediately connected with the specific powers of the system.

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[1] *Lamarck*, Hist. des Anim. sans Vertéb. Intr. p. 149.

These remarks on the relative importance of the different functions must be understood only as they regard man and those animals that the most nearly resemble him in their organization. As we examine the various classes with respect to each other, taking in the whole of the scale, from the most perfect to those that are the least so, we observe a very different order to prevail respecting the connexion between the functions and their relative degrees of importance. Beginning from the human species, and descending by a gradual progression, we find that the intellectual functions are the first that disappear, the sensitive also become fewer in number or more contracted in their operations, and, according to the opinion of the most learned naturalists, many of the lowest tribes of animals are entirely without a nervous system, and in consequence are deprived of the powers that depend upon it. Among the contractile functions, the circulation, which we have considered to be the most important in man, is the one which is first found to be wanting among the lower animals; the organs of respiration, secretion, absorption, and generation, gradually become less and less distinct, until at length they can be no longer traced, while the last that remain are those of digestion, although there is a considerable number of animals in whom no distinct apparatus even for digestion can be detected. The power of producing their species is also absolutely essential to animal existence, but the manner in which this is accomplished in the most simple animals is totally different from the function of generation as exercised by the more complicated, and in the former no distinct organ for this purpose can be discovered. According to Lamarck, who has minutely attended to the gradations of animal existence, the organs of the several functions, when considered with respect to their relative necessity for the absolute support of life, and the universality of their occurrence, will stand in the following order; the organs of digestion, those of respiration, of motion, of generation, of perception, "sentiment," and lastly of the circulation [1].

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[1] Flourens relates an experiment, which serves to illustrate the connexion between these two functions. He informs us, that in fish the circulation is not destroyed by dividing the spinal cord, because in these animals the respiration does not require the intervention of this part, whereas, in other cases, where the respiration is interrupted by this division, the circulation is also destroyed; Ann. Sc. Nat. t. xviii. *p. 109 et seq.*

The sensitive functions may be divided into two classes; first, those which originate in the action of the external agents upon the nervous system, and, secondly, those of a reverse kind, which depend upon the re-action of the nervous system on these agents. In the first of these divisions are included what we call the external senses, the sight, hearing, taste, smell, and touch, and in the same division must be placed the sensation of hunger, that of temperature, and some others, which have not been correctly discriminated from general feeling, but which possess specific characters. In the second class, those functions which depend upon the re-action of the nervous system on external bodies, we must place volition, and to the same class we may also refer instinct, association, sympathy, habit, and some other faculties of a similar kind, which appear to hold, as it were, an intermediate rank between the corporeal actions and those of a purely intellectual nature. As the functions which compose the first of these classes may be all referred to a species of perception, so the latter may be considered as more or less analogous to volition; in the former, the effect upon the nervous system, whatever it be, is propagated from the extremities to the centre, in the latter it proceeds in the opposite direction, from the centre to the extremities of the body.

The intellectual functions form the third class; these are a less direct object of physiology than the two former, yet many of them are so closely connected with the physical changes of the body as to require some degree of notice in a system which professes to give a complete view of the animal œconomy. These, although intimately, and, as it would appear, necessarily connected with the nervous system, are, at the same time, so different in their phenomena and their characters from any of the properties of matter, that I conceive we are warranted in the conclusion that they originate from an essentially different source, and are of an essentially different nature. Whatever hypothesis, however, we may adopt upon the subject, it is obvious they possess the power of acting upon matter, and that they exercise a very extensive influence over the animal body, and as far as this influence extends, it will fall under our department to investigate its nature and to trace its effects. Among these intellectual operations, which possess a decided action upon the corporeal frame, we must place the passions, and in the same class we may regard that compound of mental and physical influences, from which results what we call temperament and character. We hence proceed to functions of a more purely intellectual kind, which as they recede from the corporeal, and advance towards the mental part of our frame, are less within our province, and belong more to the moralist or the metaphysician.

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[1] Anim. sans Vertèb. t. i. p. 360.

APPENDIX TO CHAPTER IV.

Abstract of Prof. Tiedemann's Work on the Anatomy of the Fœtal Brain[1].

The work consists of two parts: the first is entitled Researches on the Structure of the Brain of the Embryo, in the different periods of its development; the second consists of considerations on the different parts of the brain, with a comparative description of their state in man and in other animals. It would be impossible for me to give an account of the numerous observations which are contained in this work, without extending my remarks to a length altogether inconsistent with my general plan: I shall therefore only select a few of the most important conclusions which the author deduces from them.

We are informed, that the brain and the spinal cord do not exist in the early period of the fœtus, their place being occupied by a limpid fluid; about the fifth or sixth week after conception, a cavity may be detected, containing a whitish fluid, which may be regarded as the first visible rudiments of the nervous system. In an embryo of nearly an inch in length, and of about the period of nine weeks, the separate parts of the brain begin to be much more distinguishable; the spinal cord and the cerebellum are visible, and the part which is afterwards converted into the corpora quadrigemina, acquires a considerable bulk; the cerebrum at this time appears only under the form of a membrane. From this date the development of the brain proceeds with considerable rapidity, so that all the different parts, which enter into the composition of the organ in its perfect state, may be successively traced, although, in most cases, bearing to each other a very different relation, as to their bulk and their respective situation, from what they do at a future period. The most interesting circumstance in this part of the investigation respects the order in which the great divisions of the nervous system come into existence. It would appear, that at the end of the second month, the spinal cord, and the two anterior prolongations or peduncles of the brain form, as it were, the basis to which the other parts are subsequently attached. The cerebrum can scarcely be said to exist, while the cerebellum and the tubercles are little more than layers of membrane, which are connected with the cord and peduncles; no nerves can be observed passing off from any part of the brain or spine, nor can any fibres be detected in them by the aid of the microscope; they seem to consist entirely of small globules[2].

The second part of the work contains an account of each part of the brain and its appendages considered separately, tracing them from their first formation to their perfect state, and instituting a comparison between the human brain and that of the inferior animals. In describing the spinal cord and the medulla oblongata, the Professor takes occasion to animadvert upon the opinion of Gall, with respect to the relation which the cortical and the medullary substances bear to each other; in opposition to the doctrine of this anatomist, our author maintains that the medullary matter of the spine is distinctly visible for some time before any of the cortical substance can be detected in it[3]. Professor Tiedemann also opposes another opinion of Dr. Gall's, that the spinal cord is composed of a series of ganglia, corresponding to the nerves which proceed from it; had this been its structure, it is probable, as he remarks, that it would have been apparent in the fœtal state; yet nothing of this kind can be detected[4].

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[1] We have an elaborate and judicious analysis of this work in the 82d number of the Edin. Med. Journ. It has been translated by M. Jourdan and by Dr. Bennett.

[2] Jourdan, p. 24, 5; Bennett, p. 23, 4.

[3] Jourdan, p. 128, 9; Bennett, p. 125, 6.

[4] *Jourdan, p. 134*; Bennett, p. 131, 2.

The question is discussed at some length, whether, according to the common opinion, the spinal cord be a production of the brain; or whether, according to the doctrine which has been supported by Gall, the brain should not rather be considered as an appendage to the cord, and as a production of it. Our author conceives that he has clearly proved this last to be the correct view of the subject, resting his opinion principally upon the progressive development of the parts, which is related in the first division of the work. With regard to the fact, as to the respective periods in which the parts come into existence, the observations of the Professor are decisive; but, I confess, that the controversy appears to me, in a great measure, a verbal one; the only important inference that we can derive from the fact is, that the functions of the brain, whatever they may be, cannot be exercised until after those of the cord.

Some of the most interesting observations in the second part of the work are those which respect the progressive formation of the cerebrum. By tracing its development in the fœtus, and comparing this with its structure in the different classes of animals, we learn, that as we advance from the fœtal to the more perfect state of the brain in the human subject, or from the inferior animals to those that possess a more complete organization of the nervous system, the cerebral lobes become gradually more elevated and arched, and that their convolutions and sinuosities are progressively developed, so as to give the brain its elliptical and almost globular form. Thus the human brain is distinguished from that of all animals, both by the size and the elevation of its hemispheres, and by the greater number of its convolutions.

Abstract of M. Flourens' Researches on the Nervous System[1].

This work principally consists of various memoirs, which were read before the Royal Academy of Sciences, during the course of the years 1822 and 1823; the object of these, as expressed in the preface, is to ascertain the properties of the nervous system, and the functions which its different parts respectively exercise in voluntary motion. The nervous system consists of the nerves, the spinal cord, and the brain; and the brain may be considered as made up of the cerebrum, the cerebellum, the corpora quadrigemina, and the medulla oblongata; parts which differ so much in their structure and organization, as to render it highly probable that they exercise different functions. Before he enters upon the detail of his experiments, the author gives an account of the nomenclature which he proposes to employ; a point always of considerable importance for the correct conception of the subject, and particularly so in this case, where he adopts a language which is, in some degree, peculiar to himself.

He conceives that the nervous system possesses three distinct properties: that of volition and perception, which he regards as the same function, and terms sensibility; that of directly producing muscular contraction, which is termed excitability; and a third, which is said "coordonner les movements," and is consequently named "coordination." The leading doctrine of the author is, that these three functions are exercised respectively by the lobes of the cerebrum, by the nerves and the spinal cord, and by the cerebellum; and the main object of his experiments is to substantiate and illustrate this position. The method which he adopted to prove his hypothesis consisted in removing the different parts of the nervous system, or mechanically irritating them, and carefully noticing the effect produced upon the animal. A great part of the value of the experiments consists in the gradual manner in which the author proceeded from one part to another, and the corresponding observations which he made upon the state of the animal, at each step of the process, by which, at least in some cases, we are enabled to fix the limit of the seat of the different functions with very considerable accuracy. One of the most important conclusions that we may deduce from the experiments is, that mechanical injury of the cerebral lobes does not cause pain or excite muscular contraction, but that these effects always ensue from injury of the nerves, the spinal cord, the medulla oblongata, and the corpora quadrigemina. The cerebellum agrees with the cerebrum in the absence of pain or muscular contraction, when it is subjected to mechanical injury. By pursuing the same method of inquiry, with respect to the especial use of the cerebral lobes, we arrive at the conclusion, that "la mémoire, la vision, l'audition, la volition, en un mot, toutes les sensations disparaissent avec les lobes cérébraux. Les lobes cérébraux sont donc l'organe unique des sensations." [1]

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[1] Recherches Expérimentales sur les Propriétés et les Fonctions du Systême Nerveux dans les Animaux vertèbrés.

This conclusion is principally founded upon the fact, that the senses of sight and hearing seem to be destroyed by the mutilation or removal of the cerebrum, and that a general state of sopor is induced, which renders the animal incapable of the exercise of voluntary motion. The especial functions of the cerebellum are then examined, when, upon mutilating or irritating this part, a variety of very singular and irregular motions were produced, which did not appear to be properly convulsive, and which seemed to consist in a loss of the power of connecting and regulating the contractions of the muscles, so as to produce the natural and appropriate actions of the animal. The especial result of the removal of the corpora quadrigemina appears to be the loss of sight; the contractile power of the iris is also destroyed, which it is said still remains after the removal of both the cerebrum and the cerebellum. As far as can be judged from the experiments, where a certain degree of injury must unavoidably be inflicted on the other parts of the nervous system, it would not appear that voluntary motion, or the external senses, except that of sight, are necessarily destroyed by the removal of these tubercles [2].

Another subject to which the author particularly directed his researches, was to determine what parts of the nervous system act by what is termed "effet direct," and what parts by "effet croisé." It is well known, that an injury of the cerebrum or the cerebellum is manifested by a loss of the functions of the opposite side of the body; but with respect to the spinal cord, the medulla oblongata, and the tubercles, the point was still undecided. The experiments of Flourens seem to prove that the tubercles act like the cerebrum and cerebellum, while the spine and the medulla oblongata produce their effect on the same side of the body with that on which they have been injured [3].

We have a long train of experiments, the object of which is to show what degree of mutilation or injury the different parts of the nervous system can sustain, without a complete destruction of their functions, and also what proportion the injury of the part and the loss of its functions bear to each other, and how far they possess the power of spontaneously repairing these injuries. Many of the results are very curious and unexpected, but they are of a kind which it is difficult to particularize in this short abstract; I may, however, remark, that they will tend very materially to illustrate many pathological facts, which have hitherto been altogether inexplicable. We have afterwards a long investigation respecting the involuntary motions; and the author inquires, whether there be any common centre, to which the origin of these actions may be referred, as is the case with the voluntary motions, and he especially examines what part of the nervous system is immediately concerned in the actions of the organs of respiration and circulation.

Flourens' work may be regarded as a very valuable repository of experiments, which appear to have been planned with ingenuity, and have every appearance of having been carefully executed and faithfully detailed. Many of his conclusions are fully warranted by the facts; but in some cases, I should be inclined to draw an inference precisely the reverse of that which has been formed by the author. One of the most important points which he attempts to establish is, that the lobes of the cerebrum are the exclusive seat of sensation and volition; yet it seems quite evident, from the result of the experiments, that, after the removal of these lobes, sensation, although rendered feeble or obtuse, was by no means extinguished, while the functions which depend upon volition, such as the various kinds of locomotion, were still executed by the animal, although it was difficult to excite them into action. The experiments that were performed upon the cerebellum are very interesting, and are perhaps some of the most decisive in their results of any which are contained in the volume. But I do not perceive the propriety of regarding them as produced by the operation of a distinct or specific nervous function. The "coordination" of Flourens may be referred to a species of sympathy or association, and the experiments will prove no more than that the cerebellum is the centre of the sympathetic or associated actions of the nerves that are concerned in voluntary motion. As the author appears, in this case, to have unnecessarily multiplied the number of the nervous functions, so he has united together, under the title of sensibility, two operations of the nervous system, which have a peculiar claim to be considered as distinct from each other, perception and volition. I think we may also very fairly question the propriety of making excitability, or the power of producing muscular contraction, a distinct function of the nerves; upon the same principle, the power of conveying the perceptions of sight or of sound might be regarded as distinct functions. Each of these powers belong to their specific and appropriate nerves, but they are all to be regarded as modifications of the same action, the intimate nature of which is unknown. The volume concludes with an account of a series of experiments by Prof. Rolando[1], which were performed twelve years previously to those of M. Flourens, very similar in the mode of their execution and in the conclusions that are deduced from them. The principal circumstance in which the conclusion of Rolando differs from that of Flourens is, that whereas the latter physiologist considers the cerebellum to be the regulator, as he terms it, of the voluntary motions, Rolando regards it as the origin of them, thus more completely separating the primary seat of perception from that of volition. There is every reason to suppose, that the experiments of Rolando were entirely unknown to Flourens, while he was pursuing his investigations; and it is not a little remarkable, that in this case, as in that of Prof. Bellingeri, researches should have been instituted, and experiments performed, so similar to each other, without the consent or cognizance of the authors: as we have no ground for suspecting any unfair claim to originality on the part of Flourens, this coincidence must tend very materially to strengthen our confidence in the accuracy of the results. Upon the whole the experiments of Flourens are more complete, and appear to be more decisive.

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[1] P. 35.

[2] Par. 2. § 6. p. 42 et seq.

[3] Second Mem. § 9..15. p. 100. 122.

Besides the volume of which an abstract has been given above, we have a second treatise by Flourens, entitled "Experiences sur le Système Nerveux," in which he still further details many of his peculiar opinions, and enforces them by additional experimental proofs. See also Ann. Sc. Nat. t. xiii. p. 86 et seq., and t. xxii. p. 337 et seq., and Mém. Acad. Sc. t. ix. p. 478 et seq.

Abstract of M. Serres' Work on the Comparative Anatomy of the Brain[2].

Serres' treatise on the brain, like that of Flourens, obtained the prize of the Royal Academy, and was published about the same time. Its particular object is to give an account of the brain in the four classes of the vertebrata, and, from the observations made upon them, to ascertain the respective functions of the several parts. In the prosecution of this object, the author has produced a work of considerable size, accompanied with numerous engravings, the whole affording very ample testimony of his skill and industry.

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[1] P. 273 et seq. Rolando's experiments were published in a treatise entitled, "Saggio sopra la vera Struttura del Cervello dell' Uomo e degl' Animali, e sopra le Funzioni del Sistema nervoso. Sassari, 1809." The translation in Flourens' work is taken from Magendie's Journal; it is also inserted in the Journ. de Phys. t. xcvi.

[2] Anatomie comparée du Cerveau dans les quatre Classes des Animaux vertèbrés.

To the body of the work is prefixed a preliminary discourse, which extends to more than 100 pages, in which Serres gives an account of what had been done on the subject by his predecessors, and in which he points out the parts of the subject which seem more particularly to require further investigation. He commences by laying down his general principles of what he terms "Zoognosie," of which I have already given some account, p. 75.

These principles are to be regarded as merely introductory to the proper subject of the work, the comparative anatomy of the nervous system. The details into which the author enters, and which compose the main bulk of his volumes, are very numerous, and have every appearance of having been prosecuted with great industry and accuracy. Into these details I shall not enter; but I shall notice a few of the general principles which he deduces from them, more especially those that are more immediately connected with any of the topics which already have fallen under my consideration. It is stated, that the spinal cord is formed before the brain in all classes of animals, a position in which, as we have seen, Serres is supported by the observations of Prof. Tiedemann. They also agree in rejecting the opinion of Gall, that the spinal cord is to be regarded as a series of ganglia, from which the nerves originate; indeed, upon the principle of the excentric formation of the parts, we should suppose that the spine rather proceeds from the nerves than the nerves from the spine, and this indeed is said to be actually the case, so far as respects the order of their formation[1]. Serres has found, that in vertebrated animals, the bulk of the spinal cord and the brain are, for the most part, in an inverse ratio to each other, and that, in this respect, the human fœtus resembles the inferior classes of animals. The spinal cord and the corpora quadrigemina, on the contrary, always bear the same ratio to each other, both in animals of different classes, and in the human fœtus. These tubercles are the parts of the encephalon which is the first formed, and it appears that, in all cases, their development bears an exact proportion to the bulk of the optic nerves and the eyes. The cerebellum, in every instance, is produced after the tubercules. In the first three classes of the vertebrated animals, the middle lobe of the cerebellum is developed in the direct proportion of the tubercles, while the hemispheres of the cerebellum are in the inverse proportion of these bodies. In conformity with the general principle, man has the middle lobe, and the tubercles the smallest in proportion to the hemispheres of the cerebellum. The spinal cord follows the ratio of the tubercles as to its relation with the cerebellum, while the annular protuberance and the optic thalami follow the direct ratio of the lobes of the cerebellum, and the inverse ratio of the tubercles. In this, as in other analogous cases, the state of the organs in the fœtus approximates to those in the inferior animals.

We have many interesting observations on the presence or absence of the different parts of the brain in the different classes of animals. Fish have no optic thalami; the corpora striata are wanting in fish, reptiles, and birds. These three classes also have no cerebral ventricles, the lobes of their brain constituting a solid mass; the corpus callosum likewise belongs exclusively to man and the mammalia. The pineal gland is found in all the four classes. The cerebral hemispheres are always developed in the direct ratio of the bulk of the cerebellum, and, consequently, in the inverse ratio of that of the spine and the tubercles. The corpus callosum follows the same proportion as the cerebral hemispheres; it progressively increases in size through the different orders of the mammalia, until it arrives at its greatest bulk in man. One of the most remarkable conclusions to which the observations of Serres lead him, is the relation which the nerves bear to the brain; in opposition to the commonly received opinion, he supposes that the nerves do not proceed from the brain to the respective organs, but from the organs to the brain and the spinal cord. But, as I have had occasion to remark in a former part of this work, this statement may rather be considered as a new mode of expression, than as an actual difference in the conception of the object, except so far as it respects the order in which the parts become visible.

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[1] P. xxxviii.

The author has drawn up an interesting comparison between the four classes of the vertebrata, as to the degree in which the different portions of the brain and its appendages are developed. In fish the optic thalami are the predominating part, the cerebral hemispheres scarcely exist, the olfactory nerves, or, as he terms them, the olfactory lobes, are very considerable, and the cerebellum is only partially developed. In reptiles the optic thalami bear a less proportion to the other parts, the cerebellum is nearly annihilated, and the olfactory nerve is much diminished, while the cerebrum is more developed. In birds the cerebellum is the predominating part, and the hemispheres of the cerebrum are increased in size, while the optic thalami are diminished, and the olfactory nerves almost annihilated. In the mammalia the cerebral hemispheres become the predominant organ, the cerebellum is also fully developed, while the tubercles are reduced to their smallest size; the olfactory nerves in this class are subject to great varieties.

We have an important principle deduced from numerous observations, that the development of the particular parts of the brain depends immediately upon the disposition of the arterial system of the animal, the circulation thus being the primary action, upon which the existence of the nervous system would appear to depend; this principle is derived both from the observations of comparative anatomy, and from the state of the organs in monstrous productions. We have a curious remark upon the relation which the faculty of instinct bears to the development of the fifth pair of nerves; in man these nerves are said to be peculiarly small, and in the bee as remarkably enlarged, so as to afford us some ground for the opinion, that instinct differs from the rational faculty as well in its seat, as in the mode of its operation.

These observations are to be regarded as only affording a specimen of the interesting and novel information which is contained in these volumes. It is obvious that they must materially influence many of our physiological speculations, and will tend to illustrate some points which have been hitherto involved in obscurity.

Abstract of M. Desmoulins' Treatise on the Nervous System[1].

This treatise on the nervous system is forced upon our attention, not only by the high character of Desmoulins, but by the still more celebrated name of Magendie, who, as we are informed in the title-page, composed the physiological part in conjunction with the author. It is upon this part that the nature of my work will lead me more particularly to dwell. The whole treatise consists of five books, the subjects of which are as follows:—1. Introduction to the study of the cerebro-spinal system, consisting principally of anatomical details respecting the form and structure of the bones of the spine and the head. 2. Of the cerebro-spinal system in general. 3. Of the lateral nervous system. 4. Physiology of the cerebro-spinal system. 5. Physiology of the lateral nervous systems. Although the two last are those which must principally occupy us on the present occasion, yet I find an observation, in the second part, which appears to me so just and important, which is applicable to the writings of nearly all the continental anatomists, and so fully coincides with the views that I have always entertained upon the subject, that I shall quote the paragraph at length. "Malgré les subtilités et les dénégations de quelques personnes, ces mots, *origine, naissance, productions*, impliquent donc dans le langage des auteurs qui s'en servent, l'idée qu'une partie que l'on dit née d'une autre, produite par un autre, est réellement sortie de cette partie qui l'aurait formée, poussée par une acte de vegetation. Cela est évident dans tout l'ouvrage de Tiedemann. Il a réellement pris à la lettre, et au sens propre et non figuré, les mots origine, naissance, production. Tel est aussi le sens qu'y attachent manifestement Gall et Serres. Il est donc démontré pour la première fois qu'aucune partie du systeme cérébro-spinal n'est produite, n'est végétativement poussée par une autre, mais que chaque partie est formée à sa place par la pie-mère."[1] To the opinion expressed in this last sentence, however, I must venture to withhold my assent, notwithstanding the very high authority with which it is sanctioned. If we limit our speculations to those actions which are cognizable by the senses, we must, I conceive, suppose that the capillary arteries are the real agents in these processes, and that the membranes are only so far effective, as they serve for a mechanical basis to which these arteries may be attached.

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[1] Anatomie des Systêmes nerveux des Animaux à vertèbres.

The following observations appear to me important and interesting. Desmoulins remarks, that the hemispheres of the cerebrum and the cerebellum bear no relation of volume with the nerves that are connected with them. But this is not the case with the optic lobes or with the middle lobe of the cerebellum. This last always exhibits a constant relation in bulk to that of the fifth pair of nerves, and the same is the case with the optic nerves and the thalami[2].

I pass on to the fourth book, the physiology of the cerebro-spinal system. The author observes, that there are three modes of becoming acquainted with the functions of the nervous system, and assigning to each part of it its specific office. The first is that of experiment: by removing successively the several parts of the brain and its appendages, and by observing what effect is produced by these successive removals, we attempt to gain the knowledge of the specific uses, both of the parts that are removed, and of those that are left. The two other modes proceed upon the principles of induction. They consist in duly appreciating the facts which are to be obtained by the study of comparative anatomy and of pathology. There is scarcely any part of the nervous system which is not wanting in some class of animals, so that by sufficiently multiplying our observations, we have the means of discovering the result of every combination of the cerebral organs, with respect to the powers and functions of the system. The symptoms and phenomena of disease afford us the same kind of inductive evidence, for the operation of the several parts of the nervous system, although seldom in that clear and decided manner as in the former case.

Desmoulins remarks, that there are three distinct orders of nervous phenomena; those which produce muscular contraction, that which produces sensation; and those which produce thought. The two first are seated both in the cerebro-spinal system and in the nerves; and in each of these systems every nervous function has its appropriate seat and conductor. The third, which is confined to the cerebro-spinal system, gives rise to a variety of faculties, which "consistent très-probablement dans les localisations." The phenomena of consciousness, being very different from those of feeling and thought, ought probably to be regarded as a fourth power, and it is further suggested, that volition may constitute a fifth distinct nervous function[3].

The first chapter of the fourth book consists of inductions and experiments respecting the spinal cord. By comparing the size and extent of this part in the various classes of animals, we come to the conclusion, that it bears a certain ratio both to the quantity of muscular contractility which they respectively exercise, either as to the velocity or the force of their contractions, and to the sensibility of the body. The author remarks, that the prolongation of the spinal cord is not in proportion to the size, or even the strength of the tail, but depends upon the variety of actions which this part is enabled to execute, as in the prehensile tails of certain monkeys. The general conclusion is, that the length and thickness of the spinal cord in the mammalia are augmented in proportion to the extent and the delicacy of the sense of touch, as residing in the surface of the body, the power of motion remaining the same in the different cases [1]. The spinal cord of a bird is at least one quarter larger than that of the mammalia, in consequence of the greater muscular effort which is required in the act of flying; while, on the contrary, in fish, where the buoyancy of the element in which they are immersed diminishes the necessity of muscular exertion, the spinal cord is proportionally small. The remarks of Desmoulins upon the different powers of the different parts of the cord, as to the transmission of contractile or sensitive impressions respectively, coincide substantially with the statement made in the former part of this work; he, however, is led to conclude that it is principally the external part of the cord which is the great agent in the transmission of both kinds of impressions.

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[1] P. 241. [2] P. 273, 4. [3] P. 536, 7.

The second chapter is on the specific properties of the lobe of the fourth ventricle, an organ which would appear to perform a most important office in the œconomy of the nervous system; for we are informed, that if we successively remove the whole of the cerebrum, then the optic thalami, and lastly the whole of the cerebellum, so as to leave the insertion of the fifth pair of nerves uninjured, the animal retains the consciousness of all the sensations which have their seat in the face, except those of sight; he is said to manifest the perception of sounds, odours, tastes, and mechanical irritation; he cries out when the organs of the external senses are stimulated; the respiration and the circulation proceed; the muscular motions are no more affected than when the cerebellum alone is removed; and even the power of volition would appear to be not altogether destroyed. But by the division of the spine below this lobe, all the functions are suspended, so as to indicate that this is the "lieu de concours et de réunion de toutes les sensations du corps, moins la vue." And it further appears that the different parts of this lobe have their specific functions; one part being more immediately connected with the sensations of the face, another with the respiration, and another with the digestive organs.

With respect to the specific properties of the cerebellum, Desmoulins endeavours to show, that there is no foundation for the opinion that has been embraced by Gall and others, that the development of this part of the nervous system bears a relation to the generative faculty. Nor does he agree with Rolando and Flourens, that the cerebellum is the great agent in producing or regulating muscular motion, an hypothesis which appears to be disproved by the most direct experiments. The opinion which Desmoulins entertains respecting the specific use of this part is, that the mutilation and destruction of the cerebellum "neutralisent une force qui faisait équilibre avec une autre force produisant la tendance à reculer. Ce n'est donc pas le cervelet lui-même qui est le siége de cette dernière force, il parait l'être au contraire d'une force impulsion en avant, comme nous le verrons plus tard." [2] Certain experiments are then referred to, which were performed by Magendie, and which consisted in dividing one of the pedicles of the cerebellum, the effect of which is stated to have been a rapid rotatory motion of the animal on its axis, which continues incessantly for a considerable length of time, and is only prevented by a mechanical obstacle. The conclusion that the author draws from the experiments is "que deux forces antagonistes circulent par les deux demi-cercles latéraux que forment le cervelet et sa commissure." [3] A no less remarkable effect is stated as being the result of an injury of one of the optic thalami; this, it is said, "entraine irresistiblement l'animal dans une course ou dans un vol circulaire ou de manege, sur le côté dont on a blessé le lobe;" and, what appears perhaps still more singular, we are informed, that frogs and serpents "tournent sur le côté opposé au lobe blessé." [4]

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[1] P. 540, 1. [2] P. 586, 7. [3] P. 589. [4] P. 593.

Desmoulins introduces his observations on the use of the cerebral lobes by remarking, that the volume of the brain is no measure of the intellect, and that the internal contour of the cranium is frequently not parallel to the external surface, so that we cannot ascertain the relative size of the different parts of these lobes by an examination of the skull. There is, however, a mechanical structure which appears to bear a regular ratio to the perfection of the intellectual faculties; "Ce mechanisme resulte du plissement de la membrane des hémisphères du cerveau."[1] Magendie is stated to have been the first to suggest the idea, that there is a connexion between the number of these convolutions and the state of the intellectual faculties. This position is supported by various facts in comparative anatomy, by the comparative state of the fœtal and adult brain in the same kind of animal, as well as by the brains of idiots. Hence is deduced the general principle, that the number and perfection of the intellectual faculties, both in a series of species and in the individuals of the same species, are in proportion to the extent of the cerebral surfaces. This position, it will be remarked, is conformable to the opinion stated above, that the specific function of the spinal cord is seated in its surface.

Desmoulins reverts to the hypothesis of Gall and Spurzheim, and asks whether there be any evidence that particular faculties have their seat in particular parts of the brain; he admits that the doctrine is plausible, but he thinks that the arguments brought forward by these anatomists are inconclusive, because they are derived only from the external form of the cranium; he conceives that it is by the examination of the brain, after the partial or total loss of certain faculties, that we are to gain our information on this point.

We have some singular varieties of muscular motions produced by the mutilation or removal of certain parts of the cerebrum. "Se l'on retranche à un mammifère la voute de l'hemisphere cerebral et le corps strié; aussitôt l'animal s'élance droit en avant et court sans se dètourner jusqu'à ce qu'il choque un obstacle."[2] This peculiar motion is said to depend upon the destruction of the medullary matter, while the destruction of the cineritious part has no immediate effect upon the motions of the animal, but appears to destroy its volition and intelligence.

The fifth book, which treats of the nerves, like the former part of the work, abounds in novel and ingenious opinions; but as this is the portion of it which more particularly consists in physiological details, so we may conceive that we are more immediately indebted for them to Magendie, and that they are more especially sanctioned by his authority. We accordingly find many of the doctrines maintained in these chapters which I shall have occasion to notice as occurring in his writings; nor do I perceive any point of considerable importance in which the opinions of Desmoulins differ essentially from those of his colleague. For example, in the account of smell, it is stated that the branches of the fifth pair are the only, or at least, the principal nerves of this sense. In the chapter on vision we are informed that no alteration of the eye takes place when it views near objects, and that the supposed adjustment is altogether unnecessary. Upon this opinion I may remark, that one of the principal arguments upon which it is founded, is derived from the comparative anatomy of the cetacea, who see equally well in air and in water, but whose eyes possess a structure that does not admit of a change of figure[3]. But to this argument it may be replied, that the adjustment of the eye may depend upon an alteration in the structure or position of the crystalline, independent of any change in the external figure of the organ. The opinion of the author respecting vision at different distances is, that the size alone of the object varies, and that the image is equally distinct at all distances within the natural range of vision. The observation which was made by Magendie respecting the insensibility of the optic nerves to mechanical irritation, is extended by Desmoulins to the three pairs

[1] P. 599.

[2] P. 625.

[3] P. 650.

of nerves which are connected with the muscles of the eye; at the same time the filaments of the fifth pair that are sent to the eye are exquisitely sensitive. An analogous observation is made in the sixth chapter with respect to the acoustic nerve. The chapter on the properties of the fifth pair of nerves contains nearly the same opinions respecting them which I have noticed above as being supported by Magendie; that they are the immediate organ of all the senses except the sight, and that they are accessary even to this sense, because vision is instantly destroyed by their division.

As containing a great variety of minute anatomical details, it is impossible to speak too highly of the value of these works. Nor are they without considerable value as physiological treatises. Yet I think it is not unfair to remark, that the inferences which are drawn from the facts are not always the direct deductions from them, and that when we venture to assume indirect conclusions in investigating the laws of the animal œconomy, we are always proceeding on dangerous ground.

Mr. Solly's Observations on the Corpora Restiformia.

While the preceding chapter was in the press, a paper of Mr. Solly's was read at the Royal Society, containing an account of some observations which he had recently made on the connexion between the cerebellum and certain parts of the spinal column. Mr. Solly, at my request, drew up the following brief abstract of his observations, which I have the pleasure of laying before my readers.

The spinal cord consists, as has been long known, of two corresponding halves, separated anteriorly and posteriorly by a deep fissure; these two halves, which are connected together by a medullary and cineritious commissure, may be again divided into two columns on each side; this further division being formed by the posterior peaks of grey matter reaching the surface, while a groove marks the exact point where they do so. From this groove the posterior roots of the spinal nerves emerge; all that portion of the cord which is between this groove and the posterior fissure is usually known by the name of the posterior column of the spinal cord, and by the experiments of Sir C. Bell is proved to be appropriated to sensation. The description which anatomical writers give of the connexion of these posterior columns with the cerebral mass, and the composition of the corpora restiformia, or the processus e cerebello ad medullam oblongatam, would lead us to believe, that the posterior columns alone formed the copora restiformia, and that therefore they were connected with the cerebellum alone, whereas the lateral portions of the cord, viz. the portions which are situated between the grooves from which the anterior and posterior roots of the spinal nerves emerge, as well as a small portion of the cord anterior to the anterior roots, form a part of the corpora restiformia, and are therefore connected with the cerebellum, as perfectly and as intimately as the posterior columns, whose relation to this portion of the encephalon has been long known and described.

These fibres, which I do not find mentioned in any anatomical work, consist of two sets, the one superficial, the other deep-seated. The former proceed from the corpora pyramidalia, at some distance below the corpora olivaria, across the surface of the cord, and may generally be seen without dissection; the latter pass to the inner side of the corpora olivaria, and to the outer side of the fibres which have been represented by Mayo, in his plates of the brain, mounting up from the centre of the side of the cord, being ultimately connected with the tubercula quadrigemina. The deeper set are immediately posterior to those fibres which are connected with the tubercula quadrigemina, and in that portion of the cord which is immediately below the restiform bodies, and are separated from the posterior columns by the groove of the posterior roots before mentioned, but in ascending to form part of the restiform bodies they obliterate this groove, interlacing, in their progress to the cerebellum, with the fibres of the posterior columns, to the outer side of which they are united in the first part of their course.

The corpora restiformia appear then to be composed of three sets of fibres; 1st. By a portion of the posterior columns, as usually described by anatomists, the other portion of these columns passing up to the cerebellum, and in their course to that point running to the inner side of the fibres which will be next mentioned; 2d. By fibres from the middle of the side of the cord, which partially interlace in the corpora restiformia with some of the fibres of the anterior columns; 3d. By superficial fibres which come from the corpora pyramidalia, and which I have no doubt interlace with those of the opposite side.

That these fibres, which I have thus described, and which I cannot discover have ever been previously described, form a portion of the motory tract of the spinal cord, no one, I think, can doubt; and the circumstance of proving their existence will be appreciated by those, who are aware of the relation of the cerebellum to the action of various muscles of the extremities,

CHAPTER V.

OF THE CIRCULATION.

SECT. 1. *Introductory Observations.*

I HAVE now described in succession the principal ingredients of which the body is composed; the membranous matter, the bones, the muscles, and the nervous substance; and I have likewise given an account of the two general properties which distinguish animals from all other beings, contractility and sensibility. I must now proceed to the different functions which are individually exercised by the particular organs of the body, which, all of them, consist either in motions brought about by the contraction of the muscular fibres, in sensations produced by the appropriate actions of the nervous system, or in peculiar affections of certain parts of this system, which are connected with the various intellectual operations. According to the arrangement which I proposed in the last chapter, I shall begin with the contractile functions, those which more immediately depend upon the contractility of the muscular fibre, because they seem to be the most essential to the existence of the animal frame, and because we conceive ourselves to be better acquainted with their nature, and regard them as being more analogous to the other powers of matter, than the functions which depend upon the operations of the nervous system.

I have already made some remarks upon the connexion of the functions with each other, and upon their relative importance to the support of animal existence; and we are led to conclude, that in the higher orders of organized beings, the circulation of the blood seems to be, as it were, the main spring of all the rest, that from which they derive their origin, and which is the most essential to the well-being of the whole. Respiration, in the most perfect animals, is, indeed, as essential to their existence as the circulation, but, if we may be allowed the expression, it is only incidentally necessary, inasmuch as by respiration we produce that change in the blood which gives the heart its power of contraction. It appears to follow as a direct consequence of Sir B. Brodie's interesting experiments, that if the blood had its specific change induced upon it by any other means, or were it exposed to the action of the air in any other manner than by passing through the lungs, all the functions would go on as at present without interruption[1], whereas, if the circulation be impeded or suspended, every part of the system, and every one of the functions, immediately feel the effect. This observation is, however, strictly applicable only to a part, although that a large part, of the animal kingdom. As I have remarked above, there is a numerous class, and that possessed of a considerably complicated organization, which has no circulation of the blood, and yet in these the nutritive fluid is acted upon by the air in a way which may be regarded as analogous to respiration. But the general structure of these animals, and the nature of their functions, bear so little analogy to that of man, as not to allow of their being compared to each other, or considered as merely occupying different gradations in the same scale.

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[1] Phil. Trans. *for 1811, p. 36* et seq.

With respect to the other contractile functions, I may remark that, however necessary a certain temperature may be to the existence of what are called the warm-blooded animals, who, being generally immersed in a medium colder than themselves, require some apparatus for generating or evolving caloric, in order to supply this deficiency, yet this may, in like manner be regarded as rather incidental than essential, and what is more dependent upon the peculiar circumstances in which they are placed, than upon any thing necessarily connected with the support of life. The functions of digestion, absorption, and secretion, are evidently still less subservient to mere existence, their object being either to supply materials for the growth and mechanical support of the body, or to mould and fashion its form, while generation is obviously unconnected with the life of the individual, and is only useful as a means of perpetuating the species.

If we take into consideration the relative importance of the contractile and sensitive functions, or of the heart and the brain, as being the respective centres of each, in respect to mere animal existence, we shall also be led to decide in favour of the former. In the higher orders of animals indeed, where there is the greatest number, and the most perfect development of the organs and functions, the brain and the heart may, at the first view, appear to be equally essential, not only to the continuance of their full powers, but even of life itself. Upon a more accurate examination of the subject, however, we shall find that the heart is the centre, not of the contractile powers alone, but of the whole of the corporeal frame, a conclusion to which we are led, both by anatomical researches, and by the nature of the powers and functions respectively exercised by these parts.

When we attempt to trace the progress of an organized being, from its earliest stage of existence to its full maturity, the first appearance that we observe of any arrangement of parts consists in the rude sketch of what is afterwards to become the organs of circulation. We are informed by Harvey, who accurately observed the gradual development of the different parts of the embryo in the chick during incubation, that the first appearance of distinct organization was a beating point, punctum saliens, as he expresses it[1], which was the rudiment of the future heart, and which preceded the formation of the other parts of the body, being visible for some time before he could discern any trace of the brain. The existence of acephalous fœtuses affords a further confirmation of the same opinion; as these beings are absolutely without brains, so it is certain that they cannot possess any share of those powers which are derived solely from this organ; yet they have grown to their full size in the uterus, and have even lived for some time after they have been expelled from it, and their death has appeared to be owing, not to any physical impossibility to the continuance of life, but to their not being able to effect those changes which are, as it were, incidentally necessary to the continuation of an existence of any considerable duration. For example, a regular supply of nutritious matter is essential to the support of life; this can only be supplied by the introduction of food into the stomach, and food can only be received into the stomach by the act of deglutition; but this act, at least in the higher order of animals, cannot be performed without the intervention of the nervous system.

Then with respect to the dependence of these two parts upon each other, the view which has been taken of the nature of their powers, and of the manner in which they are exercised, leads us to the same opinion. The very existence of the brain, as composing part of the substance of the body, necessarily implies the conveyance of the blood or some analogous fluid for its formation and support, while, on the contrary, it does not appear that the mechanical contraction of the heart, or the means, whatever they may be, by which the fluid is carried to the brain, is necessarily connected with the exercise of any sensitive function. These considerations, and others of a similar kind, both anatomical and physiological, all conduce to the conclusion, that we must regard the heart as the centre of the whole corporeal frame, the fountain of life, which is designed to pour out its vital streams to every part of the system, and to unite the various functions, however different in their nature and operations, into one harmonious whole[1].

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[1] Quarto itaque die si inspexeris........punctum sanguineum saliens emicat; De Gener. Exer. p. 17. Haller observed the pulsation of the heart at a considerably earlier period; see Comment. de Form. Cord. in Op. Min. t. ii. p. 101, and Comment. de Form. Pulli, c. 9. "Deinde hora 42 et cor vidi et aortam, et motum vertiginosum quasi, sagittæque similem, sanguinis rubiginosi ex corde sursum projecti, iterumque relabentis;" Op. Min. t. ii. p. 369. For the fullest information on this subject I must refer to the elaborate essay of Dr. A. Thomson, on the development of the vascular system in the human fœtus and in the different classes of the vertebrated animals. The observations of Dr. Thomson and of the authorities to which he refers, lead us to conclude, that Harvey's position is not altogether correct, as we learn, that the formation of the heart and brain is simultaneous; Jameson's New Phil. Journ. No. 18, 19, and 20. This essay, which was originally an inaugural dissertation, was published in its entire state in the above journal: we have an abstract of it in the Ed. Med. Journ. v. xxxvi. See also note in p. 174.

SECT. 2. *Description of the Heart and its Appendages.*

The organs of circulation may be divided into three parts, as connected with their structure and their uses, the heart, the arteries, and the veins. The heart is a hollow muscle, composed of masses of strong longitudinal fibres, forming an irregular cone, and leaving an internal cavity. The outside of the heart is covered with a firm membrane, and the internal cavity is lined with the same substance, the muscular part is copiously supplied with blood-vessels, but its nerves are considered as few in number in proportion to its bulk[2]. It is suspended from its base by the great blood-vessels, which form the main trunks of the sanguiferous system, and it is enclosed in a membranous bag called the pericardium; it is situated in the left side of the fore part of the thorax, resting upon the diaphragm. The interior of the heart is unequally divided, by a strong muscular septum, into two distinct cavities, called ventricles, which have no direct communication with each other; there are also two membranous bags at the base of the heart, called auricles, forming in all four separate cells, each of the auricles communicating with its corresponding ventricle, but the auricles as as well as the ventricles, having no direct communication with each other. Although the auricles may be considered as membranous bodies compared with ventricles, yet they are furnished with numerous fibres, and possess the power of contraction.

In describing the different parts of the heart[3], it is customary to speak of its right and left sides, and of the right and left auricle and ventricle, but it is well known that the terms are not correctly applicable to the situation of these cavities in the human body, which, as far as its situation is concerned, are more accurately designated by the words anterior and posterior. The terms right and left were originally employed by the ancients in consequence of their dissecting brute animals, in which the heart is placed differently from what it is in the human subject, and corresponds generally with the terms that were employed. This affords one proof, among many others of a similar nature, that when Galen and his successors described the anatomy of man, their descriptions were borrowed, at least in a great measure, from the different species of simiæ, which, in consequence of the superstition and prejudices of the age, they were obliged to substitute for the human body[1]. With respect to the names which we attach to the cavities of the heart, perhaps upon the whole, the most unexceptionable terms, and those which are the least likely to lead to any erroneous conceptions, are pulmonic and aortic, those which are usually called right being immediately subservient to the pulmonic, and those called left to the aortic circulation.

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[1] Sœmmering has adduced various considerations, which appear conclusive as to the point, that the nervous system is not necessary to the mere continuance of life; Corp. Hum. Fab. t. iv. § 87. The same doctrine is the necessary deduction from the decisive experiments of Sir B. Brodie and of Dr. Philip, which have been already referred to, and is confirmed by some experiments that are related in the posthumous work of my much respected preceptor, Dr. Marshall; Anatomy of the Brain, p. 249 et seq.; and likewise by those of Mr. Mayo; Comment. p. 16.

[2] Vide Supra, p. 178.

[3] Perhaps one of the most ample and correct anatomical descriptions of the heart will be found in Bichat's Anat. Des. t. iv. p. 87 et seq.; see also Boyer, Anat. t. iv. p. 277. We have a good view of the heart and its appendages in the 16th plate of Eustachius; among the moderns I may refer to Cloquet, pl. *182 . . 4; the* first taken from Loder.

The use of the heart, as forming a part of the circulating system, is to receive the blood from the veins and to propel it again through the arteries; this is accomplished by the contraction of its fibres, by which the cavities of the heart are diminished in size, and their contents necessarily forced out. The simple diminution of the cavities, and the mere pressing out of the blood, would not, however, be sufficient for the purpose of the circulation; for it is not only necessary that the blood be moved, but that it be moved in the right direction. For this purpose the heart is furnished with an elaborate mechanism of valves, which are attached to the orifices of the ventricles and the mouths of the arteries, and which are so constructed, that when the heart contracts, and the blood is forced out, the current is necessarily propelled in the proper direction.

When the blood leaves the heart it is sent with considerable force into the large trunks of the arteries[2]; these vessels soon begin to ramify in different directions to all parts of the body, until at length they are reduced to vessels too small to be traced by the eye or even by the microscope. The arteries, which perform this office of conveying the blood from the heart, are flexible, elastic tubes[3], principally composed of membranous matter formed into distinct layers, and composing what have been called the coats of the arteries. Of these membranous coats anatomists usually describe two, as possessing a sufficiently determined structure to be easily distinguished from each other; the outer one partaking more of the nature of the cellular texture, and therefore called the cellular coat[4]; and an inner membrane, white, firm, and smooth, possessing more of the physical properties of tendon. In consequence of the erroneous notions which formerly prevailed on the subject of the white parts of the body, to which I have already alluded, this latter was named by the older writers the nervous coat, a name which has still been applied to it by some of the moderns; but it is sufficiently designated by the name of the interior or innermost coat.

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[1] Haller, El. Phys. iv. 2, 3.

[2] For a correct representation of these vessels I may refer to Tiedemann's Tabulæ Arter. Corp. Hum.; they are inserted in Cloquet's Man. pl. 215..241. See also his Anatomie, pl. 189..235, for the plates of Tiedemann, Loder, and others. We have a well digested account of the arteries in the 6th chapter of Quain's Anat. p. 463 et seq.

[3] It may be necessary to observe that, according to the observations of the most accurate anatomists, the arteries are not perfectly cylindrical, but conical, the narrower end of the cone being situated towards the heart; see Hunter on the Blood, p. 168 et seq.

[4] Some anatomists have been disposed to regard this outer coat as merely a continuation of the cellular *substance,* which is continued over all the

Between these membranous coats is situated a stratum of transverse fibres, which have been termed the muscular coat: this has been supposed, like other muscular parts, to possess a contractile power, and to give the artery the capacity of alternately contracting and relaxing, thus assisting the heart in the propulsion of the blood[1]. To this alternate change in the capacity of the arteries the pulse has been commonly ascribed, and the sense of pulsation which the artery gives to the finger, when applied to certain parts of the surface of the body, has been supposed a sufficient proof of the existence of the arterial dilatation; but this point will be considered more fully hereafter. The mouths of the two great arteries which receive the blood as it is projected respectively from the two ventricles of the heart, are each of them furnished with a system of valves, by means of which, when the blood once enters the arteries, it cannot return into the heart, but is necessarily forced towards the extremities.

When the blood has been transmitted by the arteries over all parts of the body, it is returned again to the heart by the veins[2], being first received by their minute extremities, and carried from smaller to larger branches, contrary to what takes place in the arteries, until at length it arrives at the large trunks, and is poured from them into the heart. The veins are membranous tubes like the arteries, but they differ from these in possessing a less firm texture, in being nearly without the transverse fibres, and in having a number of valves in different parts of their course; whereas the arteries have no valves except at their commencement.

After this brief and general sketch of the organs of circulation, the next object will be to trace the blood through its whole progress, beginning at one part of the circuit, and following it until it arrives again at the same point. But before I enter

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body, and connects together its different parts, and have therefore conceived it to be not essential to the existence of the proper arterial structure, an opinion which I am disposed to consider as correct, but I have thought it desirable to employ, for the present, the ordinary phraseology. See some judicious observations in a Review of Béclard's Additions to Bichat, in Ed. Med. Journ. t. xviii. p. 258.

[1] The nature of these transverse fibres, and the question whether they are properly entitled to the appellation of muscular, and possess a proper contractile power, will be considered hereafter.

[2] See Cloquet, pl. 245..264; many of these plates are taken from Loder. *See also. Quain's* Anat. ch. 7. p. 571 et seq.

upon the description, I must observe that the blood in fact makes two circulations before it absolutely completes its course, being, between the two, brought back again to the heart, although not to the same part of this organ. This double circuit depends upon the circumstance, that by the circulation of the blood two distinct objects are obtained; by one of them the blood is sent into the lungs, and is there exposed to the action of the atmospheric air, by which its properties are changed, and it is adapted to the support of life. The blood, having thus acquired its specific vital properties, is returned to the heart, and is again sent out from this organ, along another set of vessels, to all parts of the body, except to the cells of the lungs, through which it had been transmitted in its former circuit. These two circuits have been distinguished by different appellations; from the extent of their course they have sometimes been called respectively the lesser and the greater circulation ; or perhaps more appropriately, from the parts to which they are sent, the first has been called the pulmonic and the latter the aortic or systemic circulation[1]. The organs of the circulation have also been divided into the arterial and the venous parts, as depending upon the structure of the vessels and the mechanical purposes which they respectively serve. They have likewise been divided into the parts containing the red and the black blood, a division which proceeds more upon physiological than upon anatomical principles, and does not entirely coincide with the former. We shall find it convenient to use each of these divisions on certain occasions, employing one or other of them according to the objects in view, or the particular point which we wish to illustrate.

In tracing the progress of the blood through the heart and along the arteries and veins, I shall begin with that part where it is returned by the systemic veins, or those which belong to the greater or general circulation, into the right auricle of the heart. From the right auricle it is poured into the right ventricle; when the ventricle becomes distended to a certain extent, its fibres contract, and its cavity being thus considerably diminished, a proportionate quantity of the fluid which it contains is expelled. There is a valve, or set of valves, which, from its figure, as consisting of three principal divisions, has been called tricuspid, attached to the passage between the auricle and the ventricle, and so constructed, that, by the contraction of the ventricle, it closes up this orifice, and prevents the blood from returning into the auricle, so that it is necessarily sent forwards into the pulmonary artery, which likewise opens into the right ventricle. The pulmonary artery carries the blood through the lungs, in a way which will be more particularly described hereafter, along the lesser or pulmonic circulation; and, after it has undergone its appropriate change from the action of the air, it is returned into the left auricle by the pulmonary veins. The same mechanical process occurs in the left side of the heart as I have just described with respect to the right; the ventricle contracts, a valve at its mouth, which, from its consisting of two principal divisions, is called the mitral valve, prevents the blood from returning into the auricle, and it is accordingly propelled into the aorta, the great systemic artery. When the blood has once entered the artery, it is prevented from flowing back into the heart by a set of valves called sigmoid or semilunar, placed at the mouth of the vessel, so that any motion which is afterwards impressed upon it, after it once enters the aorta, either by the succeeding portions of blood sent from the heart, by the action of the vessels themselves, or by an extraneous cause, must all have the effect of carrying the blood forwards from the heart into the veins, then from the smaller veins into the vena cava, the main trunk of the systemic veins, and finally depositing it in the right auricle.

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[1] These are the terms employed by Dr. Barclay in his " New Anatomical Nomenclature," p. 176 ; a work which is justly entitled to the praise of ingenuity, but I think the proposed alterations are most of them unnecessary, and on that account undesirable. The partial adoption of a new language in any department of science tends to embarrass the memory, and the general adoption of it would have the serious objection of rendering the old standard authors, in a great measure, unintelligible.

SECT. 3. *History of the Discovery of the Circulation.*

A slight and casual observation of the phenomena of the living body was sufficient to prove that the blood is perpetually in motion, but the nature of this motion, or the course which it pursues, was unknown to the ancients[1]. They had many chimerical and unfounded opinions upon the subject, which it is not necessary to detail, although some of them were sanctioned by high authorities. As a specimen of their notions it may be sufficient to state, that they considered the principal office of the arteries to be that of conveying air or some kind of spirits to and from the heart, while the veins carried the blood; that the fluids moved along the vessels in one direction during the day, and in the contrary direction during the hours of sleep; and various doctrines of a similar kind were maintained, either derived from incorrect or imperfect observations, or founded totally upon mere unauthorized hypotheses.

Some approaches to the true theory of the circulation were made by Servetus, or Servede[1], the celebrated victim of Calvinistic intolerance, and afterwards by the Italian anatomists, Colombo and Cesalpini, who flourished in the sixteenth century. It appears that they had each of them a correct idea of the passage of the blood through the lungs, along the lesser circulation, and were even aware of its being acted upon by the air, during this part of its course, but, in other respects, their view of the subject was erroneous[1]. The honour of the grand discovery of the circulation, the greatest that was ever made in anatomy or physiology, is due to our illustrious countryman, Harvey. He completed the discovery about the year 1620, but, with a rare degree of philosophical forbearance, he spent eight years in digesting and maturing his ideas, when they were at length given to the world in a short tract, written with remarkable clearness and perspicuity, which is well characterized by Aikin, "as one of the most admirable examples of a series of arguments, deduced from observation and experiment, that ever appeared on any subject."[2]

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[1] For an account of the opinions of the older anatomists on this subject, see Senac's Treatise on the Heart, Introd. p. 68 et seq. On the subject of the circulation generally, I may refer my readers to the third section of Dr. Alison's Physiology, where we have a succinct but judicious account of the principal facts and opinions that we possess on this subject.

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[1] The extreme rarity of the treatise of Servetus, which contains his opinion respecting the transmission of the blood through the lungs, as well as the interesting nature of its contents, have rendered it an object of great literary curiosity. The passage in question is contained in the fifth book of the first part of Servetus's work, entitled "De Christianismi Restitutione," not, as was stated by Boerhaave and others, in the work "De Trinitatis Erroribus." The author clearly and correctly describes the blood, as passing, "non per parietem cordis mediam, ut vulgo creditur, sed magno artificio, a dextro cordis ventriculo, longe per pulmones tractu, et a vena arteriosa, in arteriam venosam transfunditur." He adds, "In ipsa arteria venosa inspirato aere miscetur, expiratione a fuligine repurgatur." He assigns various anatomical considerations as the reason for his opinion, and he shows that he made a considerable approach to the correct theory of the circulation, as well as to many of the most approved modern doctrines respecting respiration and animal temperature. One of the earliest writers who made us acquainted with this passage in the works of Servetus, is Wotton, in his "Reflections on Antient and Modern Learning," p. 211, 2. He informs us, that he never saw the book himself, but that the passage, which he inserts in the margin, was communicated to him by Dr. C. Bernard, whom he designates as a very learned surgeon of London; Dr. Bernard having received it from a learned friend, who had himself copied it from Servetus's work. We have a more particular account of the book by De Bure, Bibliographie Instructive, t. i. p. 418..2; he asserts, that it is well known that "only one copy of the work actually exists, which passed from the cabinet of the late M. de Boze, into that of the Pres. de Cotte, who is now the possessor of this precious copy. It is probably the same which formerly belonged to the Landgrave of Hesse-Cassel, and which could not be found in his library, where it was sought for in the time of P. Eugene, of Savoy, who, in passing through Cassel, desired to see it; so that, for some time, the book was thought to be totally lost." We have a somewhat different account of the history of this volume given us by De Angelis, in his life of "Servet," in the Biog. Univ. t. xlii. He informs us, that only two copies of the work "De Christianismi Restitutione" are known to exist; one is at Paris, in the Bibliothèque Royale, the other in the Imperial Library of Vienna. The first was purchased at the sale of Gaignet, for the D. De la Valliere, for 3,810 francs, notwithstanding its bad preservation. It is the same which the biographers of Servetus say had belonged to the library of the Landgrave of Hesse-Cassel, whence it was stolen. It is from the other copy that De Murr has given a counterfeit of the work, imitating the original, absolutely line for line. The year of the counterfeit edition is marked at the bottom of the last page; it was printed at Nuremberg, in 1790. It is stated, that a new edition was undertaken by Dr. Mead, but which proceeded no farther than the 252d page, the number of pages in the entire work being 734. We learn from Dr. Sigmond, in his essay on the unnoticed theories of Servetus, that this edition was seized and burnt, at the instance of Dr. Gibson, Bishop of London, with the exception of a very few copies. Dr. Sigmond possesses a copy of Servetus's work, which was supposed by Dr. Sims, its previous possessor, to be the one that was formerly in the library of the Landgrave of Hesse-Cassel, and which was afterwards in the possession of Dr. Mead; Dr. Sigmond, however, informs us that he does not believe his copy to be the original; it does not appear where Dr. Sims procured it. His account of the book, as given us in Dr. Sigmond's note, seems to be incorrect in some minute particulars. See also Douglas, Bibliog. Anat. p. 84..6; Blumenbach, Introd. ad Hist. Med. Lit. § 152; Craigie, in Jameson's Journ. No. 24. p. 50 et seq.; Elliotson's Physiol. note in p. 195.

The manner in which this discovery was received by the public forms a curious and interesting occurrence in the history of philosophy. Harvey, for some time, scarcely made a single convert, and an excessive clamour was excited against him, for having called in question the revered authority of the ancients. He fortunately lived in a country which had been favoured with the light of the reformation, otherwise it is not impossible that he might have shared the fate of Galileo; for some of his antagonists, when they found themselves foiled in argument, did not scruple to raise against him the weapons of superstition and prejudice, insinuating that his new doctrines would tend to subvert the credit of the Scriptures, and thus undermine the foundations of religion and morality. After some time, however, it was found that Harvey's theory was true, and his opponents then commenced a different plan of attack. They asserted that the doctrine of the circulation, which had been brought forwards by him as a new discovery, was well known to the ancients, and passages were quoted and warped in a thousand ways to prove the allegation. It is asserted that, for some years, he even suffered in his professional practice from the prejudice excited against him; but by degrees the merits of his discovery began to be appreciated, and he lived long enough to witness the triumph of truth over the cavils of ignorance.

Harvey's doctrine of the circulation is now so universally admitted, that it might seem unnecessary to adduce any formal train of reasoning in its support. It may, however, be useful to review the nature of the arguments that were employed, as many of them consist of curious matters of fact, and throw considerable light upon the structure and properties of the sanguiferous system. If we open the chest of a cold-blooded animal, and bring the heart into view, we may observe its alternate contraction and dilatation proceed with great regularity[1]. For a short space of time the heart lies at rest, and suffers itself to be distended with blood, then it is suddenly seen to rise up on its basis, to shorten its fibres, and to expel its contents; it is during this process that it strikes the ribs, producing what is termed the beating of the heart.

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[1] Haller, El. Phys, iv. 4, 17; Sabatier, Anat. t. ii. p. 255.

[2] General Biography, v. 5. p. 72. See Harvey de Motu Cordis et Sanguinis Circulo. I may remark, that this celebrated treatise is worthy of our admiration, not merely as demonstrating the correct theory of the circulation, but for the sagacity which the author displays on various points indirectly connected with this function.

The passage of the blood along the arteries and the veins was first demonstrated to the eye by the experiment of Malpighi[2], who, by applying the microscope to the web of a frog's foot, or other transparent membranous part, enabled us to behold the interesting spectacle of the arteries rapidly projecting the blood in successive waves towards their extremities, where it was received by the veins and returned in a uniform stream by their trunks. It must, however, be acknowledged, that this experiment, although a peculiarly beautiful one, can scarcely be regarded as proving more than the mere passage of the blood through the arteries and the veins, and the circumstance of the pulsation being confined to the former of these vessels, for the rapidity of its motion, and the interlacing of the vessels with each other, scarcely permit the eye to detect the exact progress which it follows.

From an inspection of the mechanism of the valves, we perceive that it is impossible for the blood to return from the ventricle into the auricle, because, when this fluid endeavours to escape, the first effect is to raise up the valve which was floating upon its surface, and to apply it closely to the passage which leads from the ventricle to the auricle. There is, however, no obstacle to the entrance of the blood into the arteries, and we accordingly find that they become distended with blood. From various causes, which will be more particularly examined hereafter, the artery then contracts, but the valves which are at its mouth are so constructed as to prevent the blood from getting back into the heart, so that it must be necessarily carried forwards into the minute branches of the arterial system.

The curious operation called transfusion proves the course of the circulation to be from the arteries into the veins. In this operation, which seems to have been invented, or at least perfected by Lower, about the year 1660, the artery of one animal is connected by a tube with the vein of another animal, when we find that the first is gradually emptied of its blood, while the second is brought into a state of plethora. If an opening be made at the same time in the veins of the second animal, the blood originally belonging to it will escape, and thus the fluid in the vessels generally will be changed. At the time when these experiments were made, diseases were commonly supposed to depend upon some morbid qualities residing in the blood, and as the operation of transfusion held out a method of changing this fluid at pleasure, it was hailed as a most important means of restoring the health; and, repugnant as it appears to the feelings, some individuals actually submitted to have the blood of lambs or calves transmitted into their vessels, for the purpose of being cured of certain diseases, or having their vigour renovated when it was exhausted by old age.

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[1] Harvey, Exer. 1. cap. 2.

[2] Malpighi informs us that he saw the circulation of the blood, by means of the microscope, in the membranous part of the lungs and the mesentery; see his Second Epistle "De Pulmonibus," addressed to Borelli; it is dated 1661; see also Boerhaavé, Prælect. ab Haller, notæ ad § 160. Leeuwenhoek first saw the circulation by the microscope, as it seems, in the year 1698; he observed it in a bat's wing, a tadpole, and a fish's tail; Hoole's Leeuwenhoek, p. 90 et seq.; also Epistolæ, p. 49, where, in a letter to Heinsius, Oct. 1698, he describes his observations *on the circulation* in microscopic eels.

It does not belong to my present object to notice the operation, except so far as it may illustrate the theory of the circulation, otherwise it would be amusing to recount the extravagant expectations that were formed respecting its probable advantages. Lower himself, who was a man of science, and possessed of a clear and philosophical turn of mind, seemed to regard the discovery as a new era in the healing art[1], and it was warmly patronized by other learned persons, who might be supposed less apt to be biassed in its favour. But the first experiments of the kind that were performed upon the human subject ended fatally, and although the advocates for the practice endeavoured to shew that these unfortunate events were not necessarily connected with the act of transfusion, yet it excited so much alarm, and appeared altogether so disgusting and shocking an operation, that it was prohibited in France by an act of the legislature, and everywhere soon fell into complete neglect[2].

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[1] De Corde, c. 4; Boerhaave, Præl. ab Haller, not. ad § 160.

[2] The following papers give an account of some of the first experiments that were performed on this subject. Phil. Trans. No. 12, p. 352, (1666.) A general notice of the fact of transfusion having been performed before the Royal Society in London and at Oxford. No. 20, p. 353. A more full detail of the experiment. No. 25, p. 449, (1667.) An account of further experiments. No. 26, p. 479. The operation is performed at Paris. No. 27, p. 490. The operation is performed at Pisa, by Fracassi. No. 28, p. 517. Account of Bond's case at Paris, the first human subject on whom the operation was performed; it ended fatally. No. 30, p. 557. The operation was performed in London on a human subject by Lower and King. No. 32, p. 617, (1668.) Denys performs the operation at Paris on a maniac; the disorder is supposed to be relieved; the operation is repeated and ends fatally. No. 36, p. 710. A particular account of the above case. No. 54, p. 1075, (1669.) Further particulars of the case. See also Senac's Treatise on the Heart, Intr. p. 92.—From this time the operation appears to have been entirely laid aside, until it was again introduced by Dr. Blundell, who has given us a minute detail of his experiments, and of the method of performing the operation. He has established the important point, that the blood of an animal of the same species may be safely and easily transfused, but that if the blood of a different kind of animal be employed, great disorder of the functions is occasioned, and death generally ensues; Med. Chir. Trans. v. ix. p. 56. The experiment was tried upon the human subject, and, so far as the operation was concerned, with success; Ibid. v. x. p. 296.—The curious fact that the transfusion of the blood of an animal of a different species

A fifth argument that was advanced as a proof of the circulation being from the arteries into the veins is derived from the effects of wounds of the vessels. It was observed that when an artery was cut or divided, the part nearest to the heart projected a stream of blood, and that comparatively little fluid was poured out from the other end, while the reverse was observed to take place with respect to the veins when they were wounded; here the flow of blood was from the part more remote from the heart. Although this statement is in the main true, and is naturally explained by the course which the blood is known to follow, yet it must be considered rather as affording an illustration of the subject, than as any very direct proof.

A much more decisive argument is offered by the effect of ligatures placed upon the vessels; here it was observed that if the artery be tied, so that the stream of blood along it be interrupted, the part between the heart and the ligature becomes turgid, while the part beyond the ligature is comparatively emptied of blood. A ligature upon a vein has exactly the contrary effect; here the part between the commencement of the vessel and the ligature is rendered turgid, while the part between the ligature and the heart becomes flaccid.

Two other arguments in proof of the circulation have been adduced, even by writers of the first eminence, the power which we have of filling all the vessels of the body by injecting a fluid into one of them, and the fact well known to physiologists, that when certain medicinal substances are introduced into the veins, they are carried into the general circulation, and are found to exercise their specific action upon certain glands or other organs of the body, in the same manner as if they had been received into the stomach by the mouth[1]. These two circumstances, however, can afford only a general proof of the motion of the blood through the vessels, and of their mere communication with each other, without showing the nature of the blood's motion, or the manner in which this communication is effected.

All the circumstances which have been enumerated, the

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proves fatal, has been since observed by Prevost and Dumas; they once mention Dr. Blundell's name, but no one would suspect, from the perusal of their memoir, that he had anticipated them in the most important of their conclusions; Bibl. Univ. t. xvii. p. 215. The results of the experiments of Dr. Blundell, and of Prevost and Dumas, would appear to be scarcely consistent with the following statement made by Magendie; Physiol. t. ii. p. 342. "J'ai eu occasion d'en (expériences) faire un certain nombre, et je n'ai jamais vu que l'introduction du sang d'un animal dans les veines d'un autre eût des inconvénienccs graves, même quand on augmente beaucoup, par ce moyen, la quantité de sang."—Numerous references to cases or treatises on transfusion may be found in Plouquet, "Chirurgia Infusoria et Transfusoria;" we may remark, however, that here, as well as in other parts of this learned performance, the value of the work is diminished in consequence of subjects being incorporated, which have rather a technical or verbal, than a real connexion.

[1] Sœmmering, Hum. Corp. Fab. t. v. § 40.

inspection of the heart of a cold-blooded animal, the application of a microscope to a transparent membrane, the mechanism of the valves, the operation of transfusion, the effect of wounds of the vessels and the action of ligatures, when taken in connexion with each other, may be considered as proving very decisively that the course of the blood is from the heart along the arteries, and through the veins back to the heart; but it still remains to prove in what manner the systemic and the pulmonic circulations are related to each other. This is very satisfactorily demonstrated by the mechanism of the heart, and especially that of its valves. We find that there is no direct passage between the right and left sides of the heart, that when the blood is in either of the auricles, it must be transmitted into the corresponding ventricle, that the tricuspid and mitral valves will not permit it to return into the auricles, and, therefore, that the pulmonic ventricle, when it contracts, must necessarily force the blood into the pulmonary artery, while the aortic ventricle can propel it only into the aorta.

In speaking of the proofs of the circulation, I have hitherto taken no notice of a train of phenomena which daily offer themseves to our notice, and which are generally regarded as affording very direct and decisive proofs of the course of the blood. I allude to the appearances which are frequently found in the dissection of subjects who have died of diseases of the sanguiferous system. These, however, are rather to be considered as illustrations of the true theory, or as confirmations of it, than as actual proofs, and although they may occasionally assist us in investigating the nature of the uses which the blood serves in the animal œconomy, yet, as they belong more to pathology than to physiology, it would be scarcely consistent with the object of this work to enter into any minute detail of them.

I shall merely state in general terms, that when an obstruction occurs to the passage of the blood along any part of its course, a turgescence is produced behind the obstruction, just in the same manner as from the application of a ligature. We occasionally observe individuals in whom we have reason to suppose that the blood does not experience its specific change by the action of the air in the lungs, and, on examining their bodies after death, we find that from some malconformation of the heart or its appendages, the blood had been transmitted immediately from the right to the left side of this organ, without passing through the pulmonary vessels. It not unfrequently happens that a great arterial or venous trunk becomes obliterated by some accident, or may have been deficient in the original formation of the body, and we then find that the branches are increased to an unusual size to supply the deficiency, and that they are given off in such situations, as to correspond with the theory which has been laid down. These examples may serve as specimens of the nature of the illustrations of the true theory of the circulation, which are afforded by morbid anatomy and pathology.

We may now be considered as having established the general fact of the circulation and the path which the blood pursues, but there are several circumstances connected with this function which require to be more minutely examined, either as having formerly been the subject of controversy, as points about which a difference of opinion still exists, or as tending to illustrate some of the operations of the animal œconomy, and to explain the uses of its different parts. In the first place, I shall mention the circumstances of a more mechanical nature, connected with the structure and organization of the heart and its appendages, directly affecting its motion or its action upon the blood, considered merely as an hydraulic machine. I shall afterwards notice some points that are more immediately connected with its action as a vital organ, particularly those that depend upon its contractility. Lastly, I shall give an account of various circumstances connected with the arteries and the veins, which I have hitherto not noticed, or adverted to only in an indirect or cursory manner. In pursuance of this plan it will be my object to intrude as little as possible upon the province of the anatomist, and to make use of the facts deduced from his science only so far as they immediately lead to any important physiological conclusions.

SECT. 4. *Mechanism of the Heart*[1].

We have seen that the substance of the heart is composed of the parietes of two cavities, called ventricles, to which two others are attached, called auricles, making in all four cavities, through which the blood is progressively carried from one to the other in succession. In describing the heart it has been a point warmly contested by anatomists, what is the relative size of these cavities; whether they have all exactly the same capacity, or whether they differ from each other in this respect. As to the auricles, perhaps the point can scarcely be determined with perfect accuracy, as it is not easy to assign the precise limits where the large veins may be said to terminate and the auricles to commence. It is, however, generally admitted that the right auricle is considerably more capacious than the left, and Haller assigns their proportions as about seven to five[2]. The limits of the ventricles are better defined, both in consequence of their more compact form, and of the valves which are attached to both their orifices; yet it is remarkable that very different opinions have been entertained respecting their size, a point which one should have supposed might have been easily subjected to a direct and precise experiment. Most of the older anatomists have described the right ventricle as considerably larger than the left, and even Haller admits that this difference of size exists[1], but there seems to be good reason for doubting the correctness of the opinion. Sabatier examined this point very accurately, and his conclusions have been since generally acquiesced in. He admits that when we examine the heart after death, the right, or pulmonic cavities, are frequently found much more capacious than the aortic, but he conceives that this difference did not exist during life, and that it is occasioned by the manner in which the circulation terminates in the last moments of existence. From causes which will be more particularly described hereafter, in the act of death the blood necessarily becomes accumulated in the pulmonic cavities of the heart, while it is, from the same cause, almost entirely expelled from the aortic cavities; hence the latter become contracted, while the former are proportionably dilated, and the comparative weakness of the muscles of the pulmonic ventricle still further contributes to produce this effect, and permits the dilatation to take place. It appears, therefore, that we are to regard the two ventricles of the heart, during the life of the animal, as possessing nearly equal capacities[2].

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[1] For the most ample account of every thing that respects the mechanism and structure of the heart, the reader may be referred to the learned work of Senac, liv. 1 and 3; a writer no less to be admired for the extent of his information, than for the candour with which he comments upon the opinions of others.

[2] El. Phys. iv. 2. 17.

But whatever may be the fact respecting the size of the two ventricles, they differ very obviously from each other in their form and in the strength of the masses of muscular fibres which compose their sides. The left ventricle, or that which communicates directly with the systemic circulation, is much stronger than the other: it lies more nearly in the centre of the heart, and gives the general form to the organ, while the right, or pulmonary ventricle, lies, as it were, upon the systemic, like an appendage attached to the heart, and has much thinner and weaker parietes. Accordingly, when we divide the heart by a transverse incision across the two ventricles, we find that the section of the left ventricle is nearly a circle, while that of the right exhibits a semilunar figure. This difference in the strength of the ventricles is necessarily connected with the offices which they respectively perform, or with the degree of force which they exercise in the parts of the circulation to which they are immediately destined. The right or pulmonic ventricle has merely to propel the blood through the lungs, while the left has to transmit it to every other part of the body. The resistance which the blood has to overcome in its passage through the vessels depends upon a combination of several circumstances, which will be considered hereafter, but there can be no doubt of the general fact respecting the difference of resistance opposed to the blood at its entrance into the two circulations; and, by ascertaining the relative quantity of fibres which belong to each of the cavities, we might form an estimate of the amount of the force respectively exercised by them. The same difference of strength exists in the auricles as in the ventricles, the right being considerably weaker than the left; Haller conceives that the former possesses only one-third the strength of the latter[1].

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[1] El. Phys. iv. 3. 3; Lower, p. 36, endeavoured to prove that the capacity of the ventricles is equal, but his opinion was not generally adopted.

[2] Sabatier, Anat. t. ii. p. 241, and t. iii. p. 373; Mém. Acad. pour 1774. Bouillaud, however, in his elaborate treatise on the heart, supports the opinion of an original difference in the size of the cavities; see an account of his work in the Br. and For. Med. Rev. v. i. p. 432. Every point connected with the dimensions and weight of the heart appears to have been carefully examined by this writer.

Another subject, which has given rise to much discussion, is the exact order of succession in which the different parts of the heart contract. It is obvious that each auricle must contract before its corresponding ventricle, but it has been questioned whether any of these events are synchronous, or whether they do not each of them occur in succession. It is now, however, very generally admitted, that the parts of the same description contract precisely at the same point of time, as the two auricles and the two ventricles; that the contraction of the auricles exactly alternates with that of the ventricles, and that the contraction of the arteries is synchronous with that of the auricles. This opinion was warmly contested, about the middle of the last century, by Lancisi and Nicholls, who had each of them some peculiar notions on this subject, which, however, it is not necessary to particularize, as they are now entirely discarded[2].

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[1] El. Phys. iv. 3. 2.

[2] The invention of the stethescope, by which Laennec has conferred so great an obligation, both on the physician and the physiologist, enables us to ascertain the mechanical action of the heart, and the parts connected with it, with much more accuracy than was previously the case. Among the most important works which have lately appeared on this subject, besides the original volume of Laennec, as translated by Dr. Forbes, I may mention Dr. Hope's valuable treatise on the heart and large vessels, Dr. Spittal's work on the heart, Dr. Corrigan's essay in the Dublin Med. Trans. v. i. (new ser.), and Dr. Stokes and Dr. Harty in the Edin. Med. Journ. v. xxxiv. Dr. Corrigan supposes that the impulse of the heart corresponds to the contraction of the auricle, and precedes the beat of the artery; Dr. Hope, however, conceives that Dr. Corrigan's observations on this point are not altogether correct. On all points respecting the action of the heart and the motion of the blood, Dr. Elliotson's 11th chapter may be perused with much advantage, also Mr. Mayo's chapter on the pulmonary circulation. We have some valuable remarks on the action of the different parts of the heart, their connexion with each other, and the sounds which they respectively produce, by the late Prof. Turner, of Edinburgh; Ed. Med. Chir. Tr. v. iii. p. 205 et seq. All these points would appear to be minutely and accurately discussed in the treatise of Bouillaud referred to above; see Brit. and For. Med. Rev. v. i. p. 436 et seq. I may remark, that on all points connected with the mechanism of the heart, the opinion of Haller will be generally found more correct than that of many of his successors, and that not unfrequently,

Every part of the blood passes through all the four cavities of the heart in the course of a complete circulation; it therefore necessarily follows, that if all the cavities contract an equal number of times during the same interval, they must project the same quantity of blood, and consequently, if they differ in size, that a portion of the contents of the larger of them will not be expelled. With respect to the ventricles, it appears that their size is nearly equal, and that, at each contraction, they are nearly emptied; but it is probable that this is not the case with the auricles, and if the right be so much larger than the left, as has been stated above, it is obviously impossible that this can be the case. Indeed, from the form of the auricles it would appear that the contraction of their fibres cannot entirely obliterate their cavities. It is also probable that when the auricles contract, a part of their contents will be forced back into the mouths of the great veins, as there is no valve situated between the vein and the auricle, by which the reflux of the blood can be prevented[1].

Calculations have been formed of the length of time which the heart occupies in performing its motion, and of the quantity of blood which is expelled by each contraction. This quantity of course varies in different individuals, and in the same individual at different times, and there are practical difficulties which prevent us from arriving at complete certainty on these points. Blumenbach's estimate may be taken as a fair average; according to this the heart is supposed to expel two ounces of blood at each contraction; the whole mass of blood is reckoned at 33lbs.[2], and the number of pulsations are taken at seventy-five in a minute. Proceeding upon these data we shall find that the blood will complete its circulation, or that the whole of it will have passed through the heart in about two minutes and a half, and that a mass of fluid equal to the blood would be carried through the heart twenty-four times in an hour. It must, however, be observed that the different portions of blood complete the circulation in very different periods of time, partly depending upon the length of the course which they have to follow, and partly upon the degree of resistance which they meet with. When the blood is sent into the aorta it soon begins to pass into the different arterial branches that are connected with the great trunk; a part circulates only through the muscles of the heart, another portion takes a longer circuit through the chest, and others through such as are more

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where it has been controverted, we have been induced to revert to it, after a more complete examination of the facts; see particularly the fourth section of his fourth book.

[1] Haller, El. Phys. iv. 4. 10. For some interesting observations on the mechanism of the heart, see Home's Lect. on Comp. Anat. p. 47.

[2] Dr. Good has collected the various estimates which have been formed of the amount of the whole mass of blood; he concludes the most probable quantity to be between thirty and forty pounds; Study of Med. v. ii. p. 11.

extended, until a part of the blood is carried to those organs that are most remote from the heart.

It has been stated that the auricles and ventricles are filled and emptied alternately, so that there is no period in which the whole of the heart is either full or empty, but, as the substance of the ventricles is much more considerable than that of the auricles, and as the former belong more particularly to the heart itself, the terms systole and diastole of the heart are applied to the contraction and dilatation of the ventricles respectively, and are of course reversed with respect to the auricles. It is now generally understood that the pulsation of the heart, which we feel when the hand is placed upon the ribs, depends not upon any increase of bulk which the ventricles experience by the injection of the blood into them, but upon the effort which they make to expel their contents, or rather by the injection of the blood into the arteries; the beat of the heart therefore occurs during the systole of the ventricles, and is contemporary with the diastole of the auricles.

In describing the structure of the heart it has been already stated that, in the adult and perfect state of this organ, there is no direct communication between its right and left sides, i. e. between the two auricles and the two ventricles. As a general principle, no point in anatomy is better established than this fact, and it is one which admits of an easy and satisfactory proof. With respect to the auricles, however, this position must be taken with some limitation, as it appears that the passage between them, which exists in the fœtus, called the foramen ovale, which I shall have occasion to describe more fully hereafter, is sometimes not entirely closed even long after the period of infancy[1]. The existence of an indirect communication between the ventricles was the subject of much controversy among the anatomists of the last two centuries. It appears, however, now to be generally admitted, that in the healthy and sound state of the heart, no communication between these cavities, either direct or indirect, can be detected, but that in certain morbid conditions of the organ, injections may be passed from the coronary vessels into the left ventricle[2].

I may here notice a very remarkable example of the bigoted attachment to certain opinions, especially, when they were sanctioned by the authority of the ancients, connected with this part of the subject, which it may not be uninstructive to relate. Vesalius, the great restorer of anatomy after the dark ages, perceived that the description of the vessels of the heart which was left by Galen, did not correspond to what he found in the human subject, but resembled this part in apes and monkeys; from this circumstance he very naturally concluded that Galen used these animals in his dissections. A learned French anatomist and professor, Du Bois, better known under his latinized name of Sylvius, who was a warm advocate for the ancients, and a violent antagonist of Vesalius, in his zeal to repel the accusation, seriously maintained the position, that the human form had undergone a change in its structure since the age of Galen, and that formerly the vessels were distributed as he described them [1].

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[1] Sabatier says, it always remains open; see also Blumenbach's Comparative Anatomy, by Lawrence, § 159, note (C).

[2] Haller, El. Phys. iv. 3. 13. Sœmmering, Corp. Hum. Fab. t. v. § 27; Sabatier, Anat. t. ii. p. 235. The existence of these communications was first announced by Vieussens and Thebesius; they were received by Ruysch, Lancisi, and others of the first eminence, and were afterwards called in question by Duverney, Senac and Sabatier; see Sabatier, Anatomie, t. iii. p. 410. In Thebesius's treatise "De Circulo Sanguinis," there are two figures in which these openings are exhibited; they appear to be considerably distorted or exaggerated. We are indebted to Mr. Abernethy for the correct view of the subject; Phil. Trans. for 1798, p. 103 et seq.

The heart, as I remarked above, is enclosed in a membranous bag or pouch, called, from its situation, the pericardium; it is lined with a serous membrane, which, like other parts of the same structure, has a serous fluid perpetually discharged from its surface. This fluid, the liquor pericardii, is sometimes found to exist in considerable quantity, to the amount of several ounces; so large a quantity, however, is evidently the effect of disease, but it is a question, about which there have been very warm and even very acrimonious disputes, whether any perceptible quantity of the liquor pericardii exists during life and in the state of health [2]. Perhaps the question can scarcely yet be considered as completely decided; if we argue from the analogy of what occurs in other close cavities that are furnished with a serous membrane, we should conclude that, in the natural state of the parts there is no fluid present, but that as fast as it is discharged by one set of vessels it is taken up by another. When, however, these actions do not correspond, when either the discharge is too rapid or the removal too slow, an accumulation must take place.

A circumstance respecting the mechanical structure of the heart, which may be worth noticing, is the difference of its size in different individuals; this difference is indeed much more considerable than might have been suspected, for we are informed by anatomists, that the heart is, in some individuals, double the size it is in others, and this has not been observed to bear any proportion to the general bulk of the body. The heart of the fœtus always bears a greater proportion to the whole body than that of the adult; as the growth advances this disproportion is diminished, but it is not entirely removed until the body attains its full size. It has also been observed, that the proportion between the parts of the heart is different in the fœtus from what it is in the adult; the auricles are larger than the ventricles, and the aortic side of the heart generally is larger than the pulmonic; but these points will be considered more particularly in the account of the fœtal circulation[1].

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[1] Haller, El. Phys. iv. 2. 7. Du Bois extends this argument to the intermaxillary bone; Lawrence's Lectures, p. 174.

[2] Haller, El. Phys. iv. 1. 19, 20. Sœmmering, Corp. Hum. Fab. t. v. § 6. Sabatier, Anat. t. ii. p. 217. Bell's Anat. v. ii. p. 52 et seq.

The heart is essentially a muscular organ, and when it contracts it acts, like other muscular organs, by shortening its fibres, and in this way it diminishes the capacity of its cavities. The general fact is demonstrable to the eye, when we lay open the thorax of a cold-blooded animal, and may be directly inferred both from the structure of the valves and from the actual effects which are produced. There has, however, been much controversy upon this subject, although one that is apparently so clear and perspicuous; the great point in dispute is, whether the ventricles are diminished in both their dimensions, whether, by the act of contraction, their sides are not brought nearer together without their being diminished in length, or even whether the heart is not actually lengthened during its systole. This opinion, which at one time had very powerful advocates, seems to have arisen partly from some hypothetical notions about the pulsation of the heart, or the manner in which it is enabled to strike the ribs, during its systole; but by ocular examination, by direct experiments made upon the heart, and by considering the manner in which the valves perform their office, it is now generally admitted that the cavities of the ventricles are diminished in every direction[2].

With respect to the striking of the heart against the ribs, this, like almost every other circumstance connected with its mechanism, has given rise to considerable discussion. Wm. Hunter endeavoured to prove, that this effect did not depend upon the mere distention of the heart, as had been generally supposed, but upon the sudden injection of the blood into the arch of the aorta, which circumstance, by diminishing the curve of the vessel and reducing it more to a straight line, would tend to raise the apex of the heart, and thus bring it into contact with the ribs[3]. This opinion has been, for the most part, acquiesced in, although some modifications of it have been proposed, both in this country and on the Continent. Sabatier conceived that when the ventricles contract, a portion of the blood that was contained in them is necessarily forced behind the valves into the auricles, and in this way contributes to push the heart forwards against the fore part of the thorax[1]. Dr. Alderson, who has lately turned his attention to this subject[2], brings forward two objections against the hypothesis of Hunter; he states, that in consequence of the direction of the arteries with respect to the ventricle, the effect produced by the sudden injection of the curve of the aorta will be merely to lengthen the ventricle in the direction of its axis; and, secondly, he speaks of it as an obvious fact, " that the impulse of the heart is only felt at the moment of the systole of the ventricles, and hence the heart must have commenced its motion towards the parietes of the chest previously to the blood arriving at the arch of the aorta."

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[1] With respect to the comparative state of the fœtal and the adult heart, I may refer to the remarks of Dr. Paget, contained in his interesting essay on the malformations of the heart, published in the Ed. Med. Journ. v. xxxvi. p. 263 et seq. He adverts to the doctrine, which is maintained by many physiologists, and which was, I believe, originally proposed, in its complete form, by St. Hilaire, that every organ of the body, during its development, passes through a series of changes, becoming more complex as it approaches to its perfect state, and that in each of its successive stages, it resembles the corresponding organ in some of the lower animals. This doctrine he applies to the case of the heart, and his remarks appear to be generally well founded. I may mention that Dr. Paget's essay contains a very ample list of references to the authors who have treated on the diseases of the heart, and its various conditions in different states of the system.

[2] Sabatier, Anat. t. ii. p. 229; Bichat, Anat. Descrip. t. iv. p. 113.

[3] J. Hunter on the Blood, p. 146, 7. Harvey was well aware that the striking of the heart against the ribs did not depend upon its distention, p. 30.

But I think it may be said, in opposition to Dr. Alderson's opinion, that if a curved elastic tube, that is fixed at one end, and hanging loose at the other, be suddenly injected, the injection will tend to elevate the loose end, whatever may be the direction of the curve with respect to its orifice. As to the second ground of objection, I think I may venture to assert, that the beating is not felt at the instant when the ventricle begins to contract, but when the contraction has produced its effect in filling the arch of the aorta. That this is the case, may be distinctly seen by viewing the action of the heart of a frog, where, when the motion is not too rapid, we can watch the whole process, and observe the effect of each part of it. And I may remark, in concluding, that even were we to assent to Dr. Alderson's position, that the motion of the heart originates in the uncounteracted force on the side of the ventricle opposite to the orifice of the artery, still the effect of this force manifests itself in its action on the arch of the aorta, the change of figure of which is the actual cause of the propulsion of the heart against the ribs[3].

While the future animal is still retained in the uterus of its mother, it is obviously placed under very different circumstances, with respect to all surrounding agents, from what it is after that period. It is entirely secluded from the air, which is afterwards so essential to its existence, it is incapable of receiving any nourishment through the usual course of the digestive organs, it is constantly immersed in a high temperature, and has no opportunity of employing either the muscles of locomotion, or any of those that are connected with the exercise of the external senses. In short, it may then be regarded as forming a part of the mother; from her it derives its nourishment, its heat, and all those powers necessary for the support of that kind of life which it possesses. Still, however, to a certain extent, its organs are developed, and acquire the form and properties which they are afterwards to possess, although with certain modifications and adaptations to present circumstances, so as to afford a most remarkable example of what has been called prospective contrivance[1], or that adjustment of means to ends, which respects not merely the present condition of the being, but that which it is afterwards to assume. This species of adjustment is more peculiarly remarkable with respect to the circulation of the blood, for we have it directed in a different course from that which it afterwards pursues; we have even vessels employed which are obliterated after birth, while the whole is so connected with the mother, that her system supplies what is deficient in that of the embryo, and completely ministers to all its wants. Both as constituting in itself a very interesting part of physiology, and as tending to illustrate the functions of the adult, we may consider the fœtal circulation as a subject well deserving of our attention[2].

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[1] Anatomie, t. ii. p. 230. [2] Quart. Journ. v. xviii. p. 223 et seq.

[3] The opinions of Dr. Hope and Dr. Corrigan, in their respective works *referred* to above, do not essentially differ from that of Wm. Hunter.

The most important point in which the state of the fœtus differs from the same animal after birth is its seclusion from the atmospheric air; from which it follows, in the first place, that the lungs are incapable of exercising their appropriate functions, or of inducing the specific change in the state of the blood, and secondly, that this change of the blood, as far as it is required for fœtal existence, must be effected by the mother. In order to accomplish this purpose, a large part of the blood, which in the animal after birth is carried through the lungs, forming the lesser or pulmonic circulation, is in the fœtus diverted from this channel, and passes directly into the left, or aortic side of the heart. This diversion in the course of the blood is a necessary consequence of the state of the fœtus, both with respect to its posture and the relative development of its organs, and is likewise an expedient adapted to the circumstances in which it is situated. In order to supply the place of the lungs, or to execute the office which they are incapable of performing, the blood is carried into the placenta, where it is so acted upon by the blood of the mother, as to experience a change analogous to that which, after birth, it undergoes in the lungs. It is carried to this organ, and brought back from it by a set of vessels which exist only before birth, and which are necessarily destroyed by the act of leaving the uterus, at the very instant when they become no longer of any use.

But in appending the supplementary or extraneous set of vessels to the ordinary circulating system, it happens that the blood, after it has experienced its specific change, is returned to the fœtus in the course of the venous part of the circulation.

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[1] Paley's Natural Theology, ch. 14. p. 252 et seq.

[2] Senac, liv. 3, ch. 9, 10, 11. On this subject I may refer to an essay of Prevost and Dumas, on the development of the heart and the formation of the blood; *Ann. Sc. Nat. t. iii. p. 96 et seq.*

It is, however, obviously unnecessary for it to complete its course in this direction, and probably by so doing it would lose some part of that specific quality which it was the office of the placenta to impart to it. On this account a passage exists between the two auricles, called the foramen ovale, through which a part of the blood that is brought into the pulmonic auricle is directly conveyed into the systemic auricle, without passing into the pulmonic ventricle, as well as two additional vessels or ducts, which belong exclusively to the fœtal circulation. By one of these a part of the blood is immediately brought back to the heart, soon after its return from the placenta, without passing through any part of the venous circulation, which it would otherwise have to perform in order to arrive at that organ; by the other, a part of the blood which has arrived at the right or pulmonic ventricle, and is propelled into the mouth of the pulmonic artery, is carried directly into the aorta, thus escaping the whole of the pulmonic circulation, as well as the passage through the left or aortic cavities of the heart.

The first of these temporary or supplementary vessels as they may be styled, is named the ductus venosus; the second, the ductus arteriosus; names which, it may be remarked, are rather anatomical than physiological, being derived from the parts to which they are connected, not from the offices which they perform. In fact they are both to be regarded as arterial vessels, for their purpose is to convey, by a short track, the blood, after it has been changed in the placenta, to the main trunk of the aortic system. In short, the mechanism of the fœtal heart all tends to one object, to reduce what in the adult is a double organ, or at least an organ consisting of two sets of parts, which, although placed in contact, serve two different purposes, to a single organ, or to an organ, the parts of which may conspire to one object only. The fœtus is in the state of those animals that are without lungs, and therefore before birth only so much blood is sent to them as may be sufficient for their growth, and may prepare them for being employed after the animal acquires the capacity of breathing when it leaves the uterus.

In the course of this work many opportunities will occur of illustrating the nature of the functions by the difference between the adult and the fœtal organs; at present I shall only further remark, that although we are fully acquainted with the nature of the difference in the mechanical structure of the parts, and the change which they experience at birth, we are, perhaps, scarcely able, in every period, to trace out the actual or immediate cause by which the change is effected. We, however, understand it sufficiently to perceive that it is accomplished by a system of contrivances at once simple and effectual. The first act of inspiration, which is immediately connected with the introduction of the infant into its new state of existence, necessarily causes the blood to pass through the pulmonic vessels in *greater quantity* than before birth, and we may infer that this new direction which the blood then assumes produces all the changes in the vessels, expanding those parts which were before small and only imperfectly pervious, while it diverts the blood from those that are no longer useful, and which therefore, by their own elasticity, shrink up and finally become obliterated.

All the peculiarities of the fœtal circulation evidently tend to one object, to bring the blood, after it has been changed in the placenta, more directly into the systemic artery, but it is not so evident what is the use of each particular part of the additional apparatus. It may be asked, why might not the whole change have been effected by means of the foramen ovale or the ductus arteriosus alone, as the object of both of them appears to be precisely the same? Part of the difficulty which has attached to this subject is certainly owing to the incorrect notions which, until lately, were entertained respecting the use of the lungs, and the function of respiration. Although the older writers had some imperfect idea of the connexion between the passage of the blood through the lungs and a chemical change in the composition of the air, yet the chief object of respiration was conceived to be mechanical, and of course there could be no correct conception of the use of the placenta as a substitute for the lungs. Whatever purposes the pulmonic circulation may be supposed to serve, it is obvious that it is unnecessary for the fœtal state, but that it becomes necessary the very moment that the animal, by leaving the uterus, acquires an independent existence, and must therefore perform by its own organs those functions which were before exercised by the organs of the mother. Yet we may readily conceive the difficulty there would be in effecting this change, without, at the same time, producing a great derangement in the mechanical structure of the parts concerned, and that it would be desirable to employ every method for rendering this transformation as easy as possible. Perhaps, then, it may be regarded as a sufficient reason for the present construction of the body, that this change would be more readily effected by having the current of blood divided into two streams, instead of the whole of it going through one channel, so that the use of the ductus arteriosus, in addition to the foramen ovale, is simply to facilitate the transmutation from the fœtal to the respiratory state, by increasing the number of parts concerned, and therefore rendering a less mechanical change necessary in any one direction.

Although I think it quite useless to advert to many of the speculations and discussions that have taken place on the subject of the fœtal circulation, there is one hypothesis that may be mentioned, principally on account of the individual by whom it was advanced. Sabatier, whose name has been so frequently referred to, as one of the most accurate and judicious of the anatomists who have particularly directed their attention to the investigation of the structure and mechanism of the heart, has taken much pains to prove that the object of the foramen ovale is not simply to permit a part of the blood to pass directly from the pulmonic to the aortic auricle, but to keep the two currents of blood that proceed from the two great branches of the vena cava separate from each other, to send that part of it which comes from the vena cava superior into the pulmonic ventricle, while the blood from the vena cava inferior is transmitted through the foramen ovale into the aortic ventricle[1]. But in spite of the authority by which the hypothesis is supported, I must acknowledge that I think it very fanciful and altogether inadmissible[2].

It has been stated by those who have minutely attended to the gradual development of the parts in the fœtal state, that in the first stages of existence the two sides of the heart are nearly of the same strength and form, but by degrees the aortic ventricle, in consequence of the greater force which it has to exert, acquires its superior strength and thickness. This is analogous to other well-known facts in the animal œconomy, where a muscular part acquires additional strength by action, a provision which is attended with the most beneficial results, and is one of the most beautiful examples of the adaptation of means to ends with which we are acquainted: the efficient cause of this change will be considered hereafter.

The great importance of the circulation, considered as the regulator of the whole animal machine, and the connexion which it has with all the other functions, renders it desirable for us to view it in all its relations, and to examine it under every different circumstance in which it presents itself to our inspection. It may, on this account, be useful to make some observations upon the comparative anatomy of the organs of circulation, at least in those animals whose general structure is so similar or analogous to the human, as to enable us to establish a comparison between them[3]. Of the two great divisions into which the whole animal kingdom has been arranged by Cuvier, the animals with and without vertebræ, I shall refer only to the former, as the structure and œconomy of the latter differ so materially from man, as not to throw any light upon the subject now under consideration. We have seen that an essential part of the circulation in the human frame is the circumstance of its being double, or of the blood being carried twice through the heart in each complete circuit, once for the purpose of its being acted upon by the air which is received into the lungs, the other, in order to transmit it, when thus acted upon, to all parts of the body.

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[1] Anat. t. ii. p. 224, and t. iii. p. 387; Mém. Acad. pour 1774, p. 198.

[2] Sabatier's hypothesis has recently found an advocate in Dr. Reid of Edinburgh, who supports his opinion by an examination of the parts after injection; Ed. Med. Journ. v. xliii. p. 308..0.

[3] In the 10th chapter of Dr. Roget's Bridgewater Treatise we have an interesting account of the various kinds of circulation, beginning with the most simple animals, and proceeding to those which are the most complicated in their structure. They are arranged under the three heads of diffused, vascular, and respiratory.

The animals possessed of vertebræ have been divided into the four classes of mammalia, birds, reptiles or amphibia, and fish. The circulatory organs of all the animals that belong to the first two classes are essentially the same with those in man; they consist of a heart divided into two ventricles, to which two auricles are appended; to these the arteries and veins are so attached, that the blood pursues its complete course along the two circulations, as in the human subject, the differences depending only upon some variations in the shape or position of the parts, the order and disposition of the vessels, or the structure of the valves.

The third class, the reptiles or amphibia, although they differ from the other vertebrated animals sufficiently to be placed in a separate division, differ also considerably among themselves, so as to be defined almost as much by negative, as by positive characters. The organs of the circulation are less uniform in this than in the other classes, and it is, perhaps, not a little remarkable, considering the great attention which has been bestowed upon comparative anatomy, and the facility with which animals of this description may be procured, that anatomists do not altogether coincide in the account which they give of the circulatory organs of the same animal. Without descending, however, to minute particulars, which would be foreign to my object, it will be sufficient to state as a general fact, that the heart of the amphibia consists either of only one ventricle, or of two ventricles which freely communicate with each other, so as, physiologically considered, to be equivalent to one. The number of auricles depends upon the construction of the ventricular part of the heart; where there is a single ventricle there is but one auricle, and where there is a double ventricle there are two auricles. In all these animals, however, there is only a single large artery proceeding from the heart, which serves both for the pulmonic and the systemic circulation, and the essential peculiarity of the circulatory organs of these animals consists in the pulmonic circulation being merely an appendage to the systemic circulation. After the great artery, which may be regarded as analogous to the aorta of the mammalia and birds, has left the heart, it divides into two main branches, by one of which a part of the blood is carried to the lungs, while the other part goes through the arteries that are distributed over all the parts of the body. These two portions of blood are united in the heart, and after being mixed together, are again expelled through the great artery. It appears, therefore, that whatever be the office of the lungs, a part only of the blood is transmitted through them during each circuit, and that although *there are two sets of* vessels, yet that each portion of the blood passes through one circulation only each time that it leaves the heart[1].

The circulation of the blood is effected in fishes, the fourth great class of the vertebrated animals, in a different and more simple manner. Here the heart consists of only one auricle and one ventricle; all the blood, as it returns from the veins, is brought back into the auricle, is poured from this into the ventricle, and from the ventricle into the great artery, which carries it to the branchiæ or gills, the organ which in fish corresponds to the lungs of breathing animals. The single heart in fish may therefore be considered as corresponding to the pulmonic heart of the mammalia and birds. After the blood has traversed the gills, and undergone the specific change from the action of the air that is suspended or dissolved in the water in which the animals are immersed, it is again collected into a main arterial trunk; from this it is sent to all parts of the body, and is again brought back into the auricle by the veins. Here every part of the blood is exposed to the action of the air through each circuit, but in consequence of the smaller proportion of blood in fishes, and the more scanty supply of air, the mutual action of the air and the blood, or the degree of effect produced by this action, is much inferior to what it is in animals furnished with lungs[2].

While I am upon this part of the subject, it may be necessary to make an observation upon the terms single and double circulation, which I have frequently had occasion to employ, and which have not always been used by authors in the same sense. According to what I conceive to be the most correct acceptation of the phrase double circulation, it should be applied to that construction of the heart and its appendages, where there are two cavities which do not communicate with each other, except through the medium of one of the circuits, as in the first two classes of animals. By a single circulation, on the contrary, we should understand that which consists of a heart with only two cavities, where the blood, when it has once left this organ, does not return to it until it has completed its passage through all the vessels, as is the case in fishes. The amphibia will hold a middle place between the two, part of the blood being carried through only one circuit, while another part of it goes through both the lungs and the body generally.

This concise sketch of the comparative anatomy of the circulatory organs proves to us both the importance of the function, and its intimate connexion with respiration, so much so as to justify the assertion, that a primary object of the circulation is to propel the blood through the lungs or some equivalent organ, where it may receive the influence of the air, and have a certain specific change wrought upon it, which is a necessary requisite to the performance of its other offices. It will also appear from these remarks, that the use of the heart is entirely mechanical, that it is an apparatus by which the blood is propelled along a series of vessels, by means of the alternate contraction and dilatation of a muscular receptacle. The mechanism of the heart is all directed to this object, that is, to the propulsion of the fluid with the due degree of force and in the proper direction, and upon this principle we may explain the action of all its separate cavities, valves, and other appendages, and show how any variation which occurs with respect to them in the different classes of animals is adapted to the general structure and functions of the individual. Where the quantity of blood is large in proportion to the size of the body, and where it is necessary that it should receive the full influence of the air, there are two complete circulations, and of course two muscular bags to send it along the two sets of vessels, the relative strength of which is proportionate to the resistance which they have to overcome. Where the quantity of blood is smaller, and where a less degree of change is induced upon it by the air, we have, according to circumstances, either only a single circulation, or one of an intermediate kind, and we find that the mechanism of the heart and its absolute power are always exactly adapted to the circumstances of each particular case. We have here only one principal muscular cavity, or if there be a greater number, they have a free communication with each other, while we find that the number of auricles, whether one or more, is in like manner adapted to the number and disposition of the great veins, so as the most readily to receive the blood from them, and convey it into the ventricle in the manner which best admits of their effectually performing the alternate actions of contraction and dilatation.

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[1] Blumenbach's Comp. Anat. by Lawrence, p. 241, with note (E) containing the references to Cuvier. See also the art. "Herpetology," by Dr. Kirby, and "Ophiology," by Dr. Fleming, in Brewster's Encyc.; and the art. "Amphibia," by Mr. Bell, in Cyc. of Anat. v. i, p. 96 . . 8. The heart, in this class, when in its most complete state, may be considered as consisting of two auricles and one ventricle. Dr. Grant makes the same statement in the art. "Animal Kingdom," p. 115. I may further refer to Mr. Owen's account *of the heart of* the Siren Lacertina, in Zool. Trans. v. i. p. 215 et seq. pl. 31.

[2] *Monro* on Fishes, p. 14 et seq.

SECT. 5. *Vital Properties and Actions of the Heart.*

Having now made ourselves acquainted with the mechanism of the heart, we must proceed, in the next place, to consider its vital properties, which in this, as in every other part of the body, are two, contractility and sensibility. I have already had occasion to enter upon the question, how far the heart possesses proper sensibility, and we have seen that on this point a great difference of opinion still exists, among those who might be supposed the best qualified to judge concerning it, both with respect to the actual facts, and to the inferences that may be deduced from them. What I am disposed to regard as the most probable *conclusion, in the present* state of our knowledge, is, that the heart has but a small share of sensibility; that when it acts in its ordinary manner, it produces no sensation; that it is not under the control of the will; that the nerves distributed to it are less numerous than those which are sent to other parts containing the same number of muscular fibres, and that, both from their origin and their texture, they are more analogous to the nerves which supply the muscular coats of the viscera, than to those which are distributed over the proper muscles.

Still, however, the heart is supplied with nerves, although perhaps scantily, and we are naturally led to inquire what is their use. To this question we must seek for a reply rather from a consideration of the uses of the nervous system generally, than from any individual facts which we have it in our power to adduce on this particular topic. The uses of the nervous system have been stated above to be two; to connect us with the external world, and to unite the different parts of the system with each other, so as to form it into one connected whole. The first of these objects is effected through the medium of the organs of the external senses only, and does not apply to the heart; we are therefore to look to the second to explain the point now under consideration. We may suppose that one important use which the nerves of the heart serve, as well as those of all the other internal viscera, is to indicate to us any injury or disease of the organs, of which, as they are removed from our view, we might be unconscious, were not the parts endued with the faculty of feeling pain. To this explanation, as applied to the heart, an objection may indeed be urged, that it not unfrequently becomes the subject of disease, and even in a very considerable degree, without our experiencing any direct sensation of pain, so that we become sensible of the morbid condition of the organ more from some indirect circumstances connected with the state of the circulation, or of some of the other functions, than from any indication of it depending upon the sensation excited in the heart itself. On the other hand, however, we are aware that there are certain affections of the heart, as induced either by injuries or disease, in which it is sensible to pain or uneasiness, and perhaps all that we are able to say, in the present case, is, that the degree of sensation which the heart possesses is in proportion to the quantity of nerves sent to it, and is adequate to the wants of the system.

There is another use which may be assigned to the nerves of the heart, although it may belong more to the moral than to the physical part of our frame. We have already had occasion to remark, that although the heart in its ordinary action is independent of the nervous system, yet that on certain occasions it is liable to be influenced through it. This is especially the case with respect to mental emotions, which frequently produce or are attended by some change in the state of the circulation, either quickening or retarding the action of the heart, or affecting the quantity of blood propelled by each pulsation. Hence, according to the nature of the emotion, a greater or less quantity of blood will be sent to the surface of the body, and in no part will the effect of this change be more apparent than in the face. The countenance, therefore, as indicated by the state of the circulation, becomes the index of the mental emotions, and as it is not under the control of the will, it frequently points out what is actually passing in the mind, in a manner which can neither be falsified nor concealed. There are many important effects which are produced upon the various functions, as well contractile as sensitive, by an occasional increase or diminution of the circulation, as indirectly effected by the nerves of the heart; but these will be considered more fully hereafter[1].

With respect to the other vital function of the heart, its contractility, there are not the same difficulties as with respect to its sensibility, since its most obvious and characteristic property is its constant motion and the readiness with which it obeys the action of various stimulants. The contractility of the muscular fibres of the heart is indeed the main spring of the animal machine, considering it as an apparatus adapted for the purpose of spontaneous motion; for the direct office of the heart, and the immediate effect of its contraction, is to propel the blood through the two circulations, in one of which it undergoes a certain necessary change, while in the other it is sent, after being changed, to all parts of the body, giving to each of them their specific powers and capacities for action. Many causes have been assigned for the power by which the great machine is primarily moved. The ancients, who could conceive of no proper mechanical cause for so great an effect, had recourse to many mysterious agencies and fanciful hypotheses. They imagined that the heart contained a species of innate fire, or a kind of ethereal or subtile spirit, which produced its motion, but they did not condescend to give any minute description of the manner in which the subtile fluid acted. The learned Dutch Professor, Sylvius, who is noted as the founder of the chemical sect in medicine, accounted for the motion of the heart in a way which was perhaps more intelligible, although certainly not more correct. He ascribed it to an effervescence excited by a mixture of the different kinds of blood, one of which possessed an acid and the other an alkaline nature, and many other equally absurd ideas prevailed until the publication of Senac's valuable treatise on the heart[2].

This judicious physiologist properly attributed the power by which the circulation is carried on entirely to muscular contractility, residing principally in the heart, and showed that by the blood being poured into its cavities, and causing a certain degree of distention of its fibres, the heart is stimulated to contraction, and by this act expels its contents[1]. The stimulating cause being thus removed, the heart relaxes, but, in the mean time, a portion of blood has been propelled along the arteries and veins into the auricles, and is ready to be poured into the ventricles immediately upon their relaxation. They therefore again become distended, are again excited to contract, and again propel the fluid along the vessels, and this alternation proceeds as long as life continues. It is frequently observed that, even in human contrivances, the most important effects are produced by the most simple means, and the observation applies to the present case. Yet simple and intelligible as is the general doctrine of the circulation, there are many interesting and difficult points concerning it, which we are not yet able to answer or comprehend to our entire satifaction[2].

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[1] As intimately connected with this subject, I may refer to Flourens' experiments on the action of the spinal cord upon the circulation; Mém. Acad. t. x. p. 625 et seq., and Ann. Sc. Nat. t. xviii. p. 271 et seq.

[2] For a sketch of the opinions of his predecessors, and for a full exposition of his own doctrine, see *Senac, liv. 4. c. 7, 8, 9.*

The most important subject connected with the motion of the heart, and one which has given rise to much controversy, is the inquiry into the cause of its regularity and constancy. What, it has been asked, can enable the heart to proceed for years together, without fatigue, pulsating at equal intervals, and propelling the same quantity of blood? The hypotheses that have been advanced to answer this question, are, as usual, very numerous. Willis, who may be regarded as the first physiologist that was fully sensible of the importance of the nervous system in the animal œconomy, advanced an opinion, which, for some time, was very generally received, that the nerves of the heart, as well as of all the organs of the body, which are in constant motion, are derived from a different part of the brain from the nerves of those organs which are only called into occasional action. It must be admitted that there are certain facts which countenance the idea that the voluntary and involuntary muscles derive their nerves from different sources, but it is doubtful how far this will apply to the heart, and even were it proved, although it might tend to generalize the fact, still it would not solve the difficulty. Willis's doctrine, however, had many adherents; among others, Lancisi, who published an elaborate treatise upon the heart, adopted it, and laboured to remove all the objections that had been urged against it[1], and in short it remained, for a long time, the most approved hypothesis with the mechanical physiologists, and with those who were the most conversant with the anatomical structure of the body. Many other opinions were occasionally started, which it will not be necessary to notice, but there is one which has had so extensive an influence over the science of physiology, that it must not be passed over in silence; I refer to the doctrine of Stahl, who ascribed the regularity of the heart's motien to the *anima*, or soul, which resides in man, and superintends his actions, and which, knowing the fatal effects that would ensue from the interruption of so important a function, is careful always to preserve it in a proper state of action[2].

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[1] Among the physiologists who attributed the contraction of the heart to the stimulus of the blood, some ascribed it to certain specific properties in this fluid, which acted upon the fibres, others to their distention only. Those who adopted the former opinion were much puzzled to account for the different effects of the two kinds of blood in the two ventricles.

[2] Some experiments are stated to have been made by Sir B. Brodie, which appear to oppose the doctrine of Senac. He emptied the heart of its blood and found that it still contracted and relaxed alternately, hence he concludes that the action depends upon some influence transmitted to it, in the same manner as with respect to the diaphragm, and not upon the blood in its cavities; see Cooke on Nervous Diseases, Introd. p. 61. Whatever comes from Sir B. Brodie's pen must be received with attention, but until the experiments are given more in detail, it would be premature to speculate upon the consequences that may be deduced from them. We have likewise a similar statement made by Mr. Mayo; Comment. p. 16; from which he concludes that the alternations of contraction and relaxation in the heart depend upon something in its structure; in Mr. Mayo's experiment the effect was produced after the organ was separated from the brain and spinal cord. Dr. Alison remarks, that there is a tendency in the heart to the regular succession of its movements, independently of the regular application of the *stimulus of the blood*; Physiol. p. 27.

The doctrine of a superintending intelligent principle, residing in the body, seems to have originated from Vanhelmont, who gave it the name of *archeus;* Stahl refined upon Vanhelmont's notion and applied it to many parts of the animal œconomy, under the appellation of *anima*, and it has borne a distinguishing share in the hypotheses of many learned physiologists, down to the present time, under the title of the vital principle, spirit of animation, and various other names. An agent of this kind, although not so distinctly brought into view as by Stahl and some of his immediate followers, forms a leading feature in the writings of John Hunter[3]; and many of Darwin's speculations, when divested of their poetical garb, can only be referred to the same class. But I have no hesitation in saying, that nothing can be more contrary to the spirit of true philosophy, than to assume the existence of an intelligent agent, of which we are entirely unconscious, or to adduce, as the cause of certain effects in the body, a power of which we have no knowledge or intimation, except as a commodious method of solving the present difficulty.

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[1] De Motu Cordis, &c., lib. 1. sect. 3. cap. 3.

[2] The last defender of the pure Stahlian doctrine appears to have been Nicholls; see his treatise "De Anima Medica." But even as modified by Whytt and those physiologists who have been designated by the title of semi-animists, it involves us in the greatest inconsistencies. Whytt maintains the identity of the vital and sentient principle, or of life and intellect, and argues that a considerable part of the actions of the body are effected by mental operations, of which we are essentially unconscious; on Vital and Involuntary Motions, § 11.

[3] Although it is an ungracious task to dwell upon the errors of great men, yet the very circumstance of their celebrity is a reason why we should be more especially put upon our guard against the mistakes into which they may have occasionally fallen. This is perhaps in no one instance more necessary than with respect to John Hunter. After giving his reasons for dissenting from the explanations that had been previously given of the alternate action of the heart, he subjoins his own. "The alternate contraction and relaxation of the heart constitutes a part of the circulation; and the whole takes place in consequence of a necessity, the constitution demanding it, and becoming the stimulus; it is rather therefore the want of repletion which makes a negative impression on the constitution, and becomes the stimulus, than the immediate impression of something applied to the heart;" On the Blood, p. 149.

The Stahlian doctrine, in all its ramifications, and under every form in which it has been exhibited, seems to have originated, partly from the want of an accurate discrimination between the physical and the final causes of the operations of the body, and partly from an indistinct conception of the difference between the agency of the first great cause of all things, and the secondary or physical cause, into which alone it is the province of natural philosophy to inquire. It affords no explanation of the physical cause, or the nature of any phenomenon, to say that the Supreme Being has thought fit to order it so; the object of our researches is to examine, to which of the great laws that are impressed upon matter, the action in question can be referred. But we are always unwilling to confess our ignorance, and are never satisfied without attempting to explain every thing that passes before us. In the present instance the only answer that we can give to the proposed question is one that is in itself, in some measure, an acknowledgment of our imperfect acquaintance with the subject. We know that distention excites a muscle to contract, and that when a muscle has contracted relaxation ensues. In the present case, therefore, we can only say, that in the formation of our body, the degree of contractility bestowed upon the heart, the quantity of distention which it receives from the blood, the size and texture of the arteries which are to transmit the blood, and the quantity of resistance which it has to overcome, are all so nicely balanced, that each particular action is retained in due subjection to the rest, and contributes to form one harmonious whole.

Admitting the justice of this remark, which is indeed merely the expression of an obvious matter of fact, it remains for us to inquire, what is the specific mechanism by which this action is accomplished, or whether we can observe any thing peculiar in the structure of the heart, or the disposition of its parts, which would seem to adapt it to this state of continued action. It must be confessed, that on this point we have but little assistance from the researches of anatomy, at least there are no facts with which we are acquainted, that can afford us a satisfactory solution of the difficulty. I have already adverted to the nerves of the heart, which, both with respect to their quantity, their origin, and perhaps also their mode of distribution, seem to differ from the nerves of the voluntary muscles. And with respect to the muscular fibres of the heart, it has been observed, that both in *their form* and their mode of connexion with each other, they likewise differ from the voluntary muscles. Instead of an assemblage of long and comparatively straight fibres, disposed in the form of separate bundles, each of them enclosed in a sheath of cellular substance, and the whole furnished with a coating of the same, the muscular fibres of the heart are disposed in an irregular manner, they are not divided into distinct parcels, and they have but little cellular substance attached to them [1]. This peculiar form and composition of the muscular part of the heart seems, however, to refer rather to its mechanical action than to any peculiarity in its vital powers. The long and straight lacerti of the muscles of voluntary motion are fitted to produce the contraction of these parts in one direction only, whereas the irregular interlacing of the fibres of the heart obviously serve to promote the contraction of this organ in every direction, so as to diminish its size in all its dimensions. There is no muscular part which is possessed of the same kind of action with the heart, and which is possessed of nearly the same degree of power, so as to require an equal quantity of muscular fibres to be concentrated in one point; but it would appear that a similar structure of the muscular matter exists in the stomach and the bladder, and that some approach to it will be found in all those hollow muscles, where the contractile force is exercised in more than one direction. This structure, however, throws no light upon the nature of the vital powers of the heart, nor does it in any degree tend to explain the difficulty in which this point seems to be involved.

The regularity of the heart's motion, or the perfectly uniform manner in which the different steps of the process follow each other, proceeding in the same order and occupying the same space of time, is a circumstance still more worthy of our admiration than its constancy, and was accordingly regarded by the earlier anatomists and physiologists as something almost beyond the powers of the human mind to comprehend or explain. The mathematicians were not, however, wanting in their attempts to solve the problem, and Bellini, who in the application of his general principles was appalled by no difficulties, endeavoured to account for it by supposing that the auricles and ventricles of the heart were to be regarded as antagonist muscles so arranged, that when one set of them contracted the other necessarily relaxed. Baglivi, whose premature death must excuse many of his errors, formed an ingenious, although fanciful hypothesis, according to which he accounted for the phenomena in question upon the idea that the alternate contraction and relaxation of the ventricles proceeded from a corresponding alternation of pressure on certain parts of the nervous system, by which their energy, and consequently the contractile power of the muscles, was alternately repressed and excited. A mechanical theory something like this was maintained by Boerhaave, but it will not be necessary to examine it in detail, for this and some other refined speculations of the same description, were completely overthrown by Haller. He reduced the regularity of the motion of the heart to the simple effect of stimuli, acting successively upon a series of contractile organs, the mechanism of which is so arranged, that each part, when relaxed, receives the stimulus from the contiguous parts, is then roused to contraction, and by this act transmits the stimulating body to the next cavity, which is then in a state of relaxation and prepared for its reception.

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[1] *Sœmmering, Hum. Corp.* Fab. t. v. § 29.

SECT. 6. *Action and Properties of the Blood-vessels.*

We are now to proceed to the third of the topics which I pointed out as requiring our attention, the structure and actions of the arteries and veins. We have hitherto considered the blood-vessels merely in the light of tubes appended to the heart, receiving the fluid that is propelled into them by this organ, and transmitting it from one set of cavities to the other. We must now regard them in a further point of view, not simply as mechanical agents, acting like the parts of an hydraulic machine, but as vital agents, endued with the properties of living matter, and forming parts of an organized system. Although nothing can appear more reasonable than this doctrine in the abstract, yet there are few topics in physiology which have been the subject of more controversy than the exact nature of the vital powers of the blood-vessels, and the consequent share which they have in the circulation of the blood. The mechanical physiologists, as might be expected from their system, regarded them as mere tubes, subject to no laws but those of mechanical impulse, and even some of the latest and most esteemed of the moderns do not admit them to possess either contractility or sensibility.

As to the latter quality, it is generally agreed that the blood-vessels possess no proper sensibility in their healthy and natural state, being in this respect analogous to many other parts of the body which are principally composed of membranous matter, and are not subject to the control of the will. How far the blood-vessels resemble the other membranous parts in the capacity of feeling pain, while suffering from inflammation or any other morbid state, is a question which appears to have been but little attended to; analogy would induce us to suppose that they possessed sensibility under these circumstances, and I know of no facts which would lead us to a contrary conclusion.

But the great controversy respecting the vital powers of the blood-vessels has been concerning their contractility, or rather that of the arteries, and this question has been resolved into another, which is intimately and necessarily connected with it, whether the arteries possess any muscular fibres, or whether the transverse fibres, which have been usually called the muscular coat, are justly entitled to that denomination. That the larger arteries actually possess a certain assemblage of fibres, placed in a transverse direction, is admitted by the concurrent testimony of all anatomists, but there has been a great difference of opinion respecting their nature and properties, or the effect which they produce in the animal œconomy.

The contractile power of the arteries was very warmly discussed by the anatomists of the last century, and was one of those points on which Haller exercised all his talents, both anatomical and physiological. To a certain extent he was the advocate for the muscularity of the arteries; he regarded the transverse fibres as clearly muscular; he, indeed, admitted that he could not exhibit their contraction by the application of the appropriate stimuli, but he adduced a variety of considerations to prove that the contraction of the arteries was an important agent in the circulation of the blood [1]. The contractility of the arteries was warmly advocated by Whytt [2] and Senac [3], and may be regarded as forming the basis of Cullen's celebrated hypothesis of the action of the vessels. This hypothesis, which had so much influence over the general doctrines of pathology, as to introduce a totally new theory of some of the most important parts of the science, although not invented [4], was so far modified and arranged by Cullen, as to possess a merit far superior to that of mere originality. The Galenic doctrine of the humoral pathology had been already called in question by Baglivi and others, and the hypothesis of the solidists had begun to make some progress in public estimation. But it was rather from the weakness of the old theory, than from the consistency or force of the new one, that this change of opinion is to be ascribed, for it must be acknowledged, that the opposers of the humoralists had been much more successful in controverting their antagonists, than in proving the superiority of their own doctrines. Cullen was not satisfied with completing the destruction of the ancient edifice, but aspired to the honour of erecting a new one in its place, and consequently he did not think it sufficient merely to state in general terms that the vital actions of the body, and the changes which it experiences, depend upon the solids, but he proceeded to point out the agents by which these changes are effected, and these he determines to be the minute ramifications of the arterial system, or, as they have been named from their size, the capillary vessels[1].

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[1] El. Phys. ii. 1. 7; ii. 1. 13; iv. 4. 32; vi. 1. 39; Mémoires sur la Nature Sens. et Irrit. des Part. du Corps Animal, mém. 2. sect. 11; his treatises "De Sanguinis Motu," "De Part. Corp. Hum. Sent. et Irrit.", and "Responsio," in Opera Minora, t. i. On no topic is the candour and caution of this great man more apparent than in what relates to the muscularity of the arteries.

[2] Whytt's works, On the Circulation of the Fluids in the very small Vessels of Animals; and Observations on Sensibility and Irritability, part ii. sect. 1.

[3] Traité du Cœur, liv. 5. ch. 3.

[4] The doctrine of the vital action of the vessels was perhaps first distinctly announced by Stahl, and was very explicitly stated by Gorter, but so combined with erroneous opinions as to lose much of its value; see Med. Compend. t. i. tract. 17. For a more particular account of the doctrine of Cullen, respecting the muscularity of the arteries, I may refer to Dr. Thomson's work, p. 278.

The action of the capillary vessels has, since the time of Cullen, been generally regarded as the great agent in all the vital actions of the body, either physiological or pathological, by which any permanent changes are produced in its form or composition. This point we shall have abundant opportunities of illustrating in the account of the different functions as they will successively fall under our review, and with respect to the circulation in particular, there seems every reason to conclude, that it is very materially influenced by the capillaries, not merely as one of the means employed to propel the blood along the vessels in its ordinary course, but as the power by which all the subordinate changes in the state of the circulation are principally or entirely effected.

With respect to the share which the arteries themselves have in the motion of the blood, this acute physiologist remarks, that if the motion of the blood depended entirely upon the impetus impressed on it by the heart, its force and velocity in the different parts of the body must, at all times, bear the same ratio to each other. If the quantity and velocity of the blood in any two corresponding parts, as in the two arms, for example, be the same, provided the arteries be mere membranous tubes, the momentum of the blood in the two arms must always be precisely similar, and this will continue to be the case, whatever be the strength or velocity of the heart's motion, and however these may vary at different times.

But we do not observe this constant ratio to exist; on the contrary, the relative momentum of the blood in the different parts of the body is always varying, so that the quantity of blood and the velocity with which it moves are perpetually altered, both from the effect of external stimuli, and from a number of internal causes. This variable state of the momentum of the arterial system may be regarded as a decisive proof of the vital action of the vessels, and it may be considered as almost a necessary consequence, that this action must consist in alternate contraction and dilatation, and it is at least highly probable, that the contraction is effected by a muscular structure [1].

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[1] I have described the blood as being conveyed immediately from the termination of the arteries into the commencement of the veins, the minute intervening vessels being supposed to belong, according to their position, to one or the other of these systems. Dr. Hall, however, conceives them to be essentially different both in their structure and in their office; he states the circumstances which led him to form this opinion, and he illustrates it with plates. The chief point on which he insists is, that arteries branch off from large to small vessels, while veins proceed in an inverse order, from smaller to larger; the capillaries, on the contrary, have numerous anastomoses of vessels of the same size; On the Circulation of the Blood, Ch. 1. See some judicious *remarks* on the structure of the capillaries, in Craigie's Elem. p. 134 et seq.

An elaborate set of experiments was performed by Hunter, the object of which was to distinguish between the elastic and the muscular power of the arteries, in which the existence of this latter appeared to be fully established. The experiments proceeded upon the principle, that the arteries possess both a proper contractile and an elastic power, that the former of these ceases with life, but that the latter remains as long as the parts retain their structure and composition. An animal was bled to death, by which the arteries are brought into a state of complete contraction; portions of them were then taken and slit open longitudinally, and were distended by a given weight in the transverse direction. After some time the weight was removed, and they were then left at liberty to resume their natural size, when it was found that they had undergone a certain degree of contraction, but less than that which they possessed before the experiment. They were measured in their three states, of ordinary contraction, of greatest distention, and of medium contraction; it was argued that the difference between their original size and the greatest distention of which they were capable, was the measure of the sum of their muscular and elastic contraction, while the degree in which they recovered themselves after the distention was the measure of their elastic force only, because after death this power alone could be supposed to remain in them, the muscular power necessarily ceasing with the cessation of life. By the weights that were employed, and by the degree of distention that was produced, Hunter endeavoured to estimate the exact relation which these forces bore to each other in the different vessels, or in the different parts of the vessels upon which he performed his experiments [2].

Although I conceive there are various circumstances which will prevent us from arriving at any great degree of accuracy in the estimates that are deduced from experiments of this description, yet the principle upon which they were performed I believe to be correct, and they may be considered as certainly establishing the general fact, that the arteries possess a proper contractile, as well as an elastic power. We may also be allowed to depend upon them so far as to admit that they afford evidence of the general distribution of these respective forces; viz., that the larger arteries possess a greater proportion of the elastic, and the smaller of the muscular power, a conclusion which coincides with Cullen's speculations.

A number of experiments, essentially resembling those of Hunter, and which appear to have been performed with great care, were made by the late Dr. Parry, and have been lately repeated and extended by his son; from these, as well as from various considerations connected with the subject, they arrive at a conclusion which, in its most important features, may be regarded as coinciding with that which we have obtained from other sources[1]. He, indeed, conceives that the fibres which compose the middle coat of the artery are not properly muscular, because he supposes they cannot be made to contract upon the application of those stimulants which produce contraction in proper muscles; but he admits of a certain vital action in these parts, allowing a considerable degree of contraction of the arteries, necessarily connected with the life of the part, and which is a distinct property from what the vessels possess as elastic tubes; to this power he gives the name of tonicity[2].

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[1] Thomson's Lectures on Inflammation, p. 66.

[2] Hunter on the Blood, p. 124 et seq.; see Hewson's Exp. Enq. v. ii. p. 14, for a confirmation of the principles upon which the above experiments were conducted.

But although so many circumstances, both physiological and pathological, tended to favour the doctrine of the contractility of the vessels, still it remained defective in its most important point, that the transverse fibres could not be made to contract upon the application of stimulants. It had, indeed, been generally assumed, from their appearance and texture, that these fibres were muscular, but Haller's experiments were unfavourable to this opinion[3], and it was strenuously opposed by Bichat, both upon anatomical and physical considerations[4]. We are also informed by Berzelius that the chemical analysis of these transverse fibres differs from that of the proper muscles[5], and the same statement is made by Dr. Young[6]. But the objection, although proceeding from such high authority, will probably not be thought absolutely decisive, when we reflect upon the obvious marks of contractility which is manifested by many of the lower classes of animals, as the Actiniæ, the substance of which is at least as unlike that of the muscles of warm-blooded animals, as the transverse fibres of the arteries can be conceived to be [1].

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[1] See Dr. Hastings's remarks on Dr. Parrys's experiments, in his Treatise on the Mucous Membrane, p. 20, 36, &c.

[2] On the Arterial Pulse, p. 52 et seq.

[3] Albinus describing the anatomical appearance of the parts, without any regard to physiological hypotheses says, that the transverse fibres of the large arteries do not resemble either muscle or tendon; Acad. Annot. lib. 4. c. 8. pp. 32, 38.

[4] Anat. Gén. t. 1, p. 272 et seq. Nysten endeavoured to prove the correctness of Bichat's opinion, by showing that the aorta is not sensible to the stimulus of galvanism; Recherches de Physiol. p. 325 et seq. It must, however, be remarked, that the aorta was not the part of the arterial system where contractility is supposed to reside, and that we have experiments which lead to the contrary conclusion, although, perhaps, not so decisive as Nysten's. We must remark also, that Bichat supposes the contraction of the capillaries to be independent of the heart; Sur la Vie et Mort, p. 134. Lerminier, the writer of the article "Circulation," in the Dict. des Scienc. Méd. t. v. p. 233, argues that the circular fibres are not muscular, nor the arteries contractile; but he restricts the term *artery* to the larger vessels, and supposes the capillaries to be distinct from either the arteries or veins; it does not, however, appear to me that any advantage is gained by this innovation, and it is difficult to assign the precise limit where this change takes place.

[5] *View of Animal Chemistry*, p. 25.

[6] Med. Lit. p. 501.

But all the arguments of a general nature that have been adduced in this question are of little weight compared to the direct experiments which have been performed since the time of Haller, in which the arteries exhibit a power in every respect analogous to, or rather identical with, that of the proper muscular fibre. Experiments of this description were performed by Verschuir shortly after those of Haller; they are related with much apparent candour and accuracy, and seem to prove decidedly that the arteries visibly contract upon the application of the ordinary stimulants [2], but from the high and almost overwhelming authority of Haller, they appear to have been but little attended to, and to have made scarcely any impression on the opinions of physiologists.

The results of Verschuir's experiments have been confirmed by the successive investigations of Dr. Philip, Dr. Thomson, and Dr. Hastings. Dr. Philip placed the web of the frog's foot in the microscope, and distinctly saw the capillaries contract upon the application of those stimulants which produce the contraction of the muscular fibre [3]. The same effect is described as having been the result of Dr. Thomson's experiments [4]; and it would appear that, in both these cases, the appearances were so obvious and decisive, as to admit of no question as to their reality, and scarcely of any doubt as to the cause producing them. This conclusion has been recently confirmed by Dr. Hastings, and he has farther extended his experiments to the large arterial trunks, and even to the veins [5], and has observed in them, as well as in the arteries, the most clear evidence of their contraction upon the application of various stimulants, both chemical and mechanical [6].

And besides the direct result of experiment, there are other circumstances stated by Dr. Philip, which are scarcely less decisive in favour of the contractility of the arteries. He informs us that he distinctly observed the circulation in the smaller vessels to continue for some time after the heart was removed from the body [1], and the same observation was made by Dr. Hastings [2], an effect which must necessarily have depended upon the action of the vessels themselves. Dr. Philip further informs us that the motion of the blood in the capillaries is influenced by stimulants applied to the central parts of the nervous system [3], which must depend upon the capillaries possessing a proper contractile power, similar to that of the muscular fibre [4].

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[1] Dr. Young appears almost to afford a reply to his own objection; for at the termination of the paragraph he maintains that a part may be muscular which does not contain fibrin, and he adduces the case of the crystalline lens in support of his opinion. He supposes these fibres to possess considerable elasticity; Med. Lit. p. 502. Dr. Jones, who, it may be presumed, had made himself familiar with the appearance of these parts, very decidedly pronounces them to be muscular; at the same time he says that they possess considerable elasticity; Treatise on Hæm. p. 2. Sir Everard Home observes, that the thin membranous coat of the hydatid possesses contractility, although its structure is so different from that of the muscles; Lect. on Compar. Anat. p. 30.

[2] De Arter. & Ven. Vi irrit. p. 20, 25, and 81.

[3] On Febrile Diseases, (3d ed.) v. ii. p. 17 et seq.; Med. Chir. Trans. v. xii. p. 401 et seq.

[4] Lectures on Inflammation, p. 83.

[5] The contraction of the veins was observed by Verschuir.

[6] Treatise on the *Mucous Membrane, Introd.* p. 24..28, 50..58, 61..64.

It appears, therefore, that we are fully warranted in the conclusion that the arteries possess a proper contractile power, and it is to be presumed that this power resides in their transverse fibres. It appears likewise to be established, that this contractile power is principally seated in the capillary arteries, while the large trunks and the veins, although not destitute of it, possess it in a less degree. Indeed every fact with which we are acquainted respecting the mechanism and functions of the sanguiferous system, lead us to the same conclusion, that the large arteries are to be regarded as canals transmitting the blood from the heart, where it receives its great impulse, into the smaller branches, and that it is principally in these smaller branches that it exercises its various functions. We are therefore to consider the large trunks in the light of a mechanical or hydraulic system, and the capillaries as physiological or vital organs. It may be remarked that this distinction, which is of the greatest importance, has seldom been clearly laid down by physiologists [5], and that to this neglect may be attributed at least a part of that ambiguity which has so long prevailed respecting the properties of the arterial system [6].

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[1] Inquiry, Ex. 24, 62, 63. Quart. Journ. v. xiii. p. 107.

[2] Introd. p. 51.

[3] Inquiry, p. 291, 292.

[4] An indirect argument of considerable importance in favour of the contractile power of the arteries may, I think, be drawn from the occurrence of cases in which the heart has been either altogether wanting or completely defective in its structure, of which a well-marked instance is recorded by Sir B. Brodie, Phil. Trans. for 1809, p. 161 et seq. We have a decisive instance of the same kind mentioned by Hewson, Exp. Enq. v. ii. p. 15. We may conclude, in these cases, that the arteries must have been the *compensating* organs; and it is more agreeable to the general principles of the animal œconomy to suppose that they were endued with an additional portion of their natural power, than that a completely new function was imparted to them. See Arnott's Elements, p. 509.

[5] From this censure I must except the anonymous author in the Edin. Med. Journ. to whose judicious papers I have so frequently had occasion to refer. I have often regretted that the valuable critical articles in this journal are not, in all cases, sanctioned by the names of the writers.

[6] With respect to the state of opinion regarding the muscularity of the arteries, I must observe, that some of the most eminent continental physiologists maintain the negative side of the question. Among others, we have the high authority of Magendie, Journ. t. i. p. 102 et seq. and Mém. Soc. d'Emulation, t. viii. p. 770 et seq.; of Adelon, Physiol. t. iii. p. 382; of Rolando, Anat. Physiol. iv.; of Broussais, who, while he admits the contractility of the arteries, Physiol. t. i. p. 38, 9, conceives that the transverse fibres are not muscular, t. ii. p. 209; and of Alard, who enters fully into the

With respect to the properties of the veins there is much less difference of opinion than with respect to the arteries. It is generally admitted that the veins exhibit the transverse fibres in small quantity only, or are nearly destitute of that part which is analogous to the middle coat of the arteries, and are consequently to be regarded as little more than mere elastic tubes. Their office is to return the blood to the heart, after it has completed all its functions in the different organs, and has either expended that ingredient which gives it the power of acting upon the constituents of the body, or has acquired some addition which renders it inert or noxious. The action of the veins is therefore entirely mechanical, and the blood is transmitted by them to the auricles upon hydraulic principles, in a way which will be considered hereafter. With respect to their structure, as far as their physiological properties are concerned, it is only necessary to remark, that all the large veins which pass along the muscles are furnished with valves so placed as to prevent the regurgitation of the blood towards the commencement of the vessels; the use of these valves, and the reason why they are confined to certain veins alone, will be further considered when we treat of the powers by which the circulation is effected.

Before we quit this part of our subject it will be necessary

question, and concludes that the arteries are not possessed of contractility; On the Seat and Nature of Diseases, p. 50..7. Dr. Arnott, although he brings forwards various considerations in proof of the contractility of the arteries, conceives that they can have no effect in propelling the blood; Elem. of Physics, p. 509 et seq. In relation to this work I may observe, that the part of it which treats of the circulation of the blood contains much valuable information, and well deserves our attentive perusal. The late investigations of Dr. Hodgkin, which have been already referred to, lead him to doubt whether the middle coat of the artery is properly muscular, because, although its fibrous structure is perfectly evident, the fibres do not possess the transverse striæ, which he conceives to be characteristic of the muscular fibre; Phil. Mag. v. ii. p. 137. The contractility of the arteries is not admitted by Dr. Craigie; Anat. p. 141, 2. Dr. Hall, on the other hand, brings forward various powerful arguments, derived from observation and experiment, in favour of their muscularity; I must acknowledge, that I regard his arguments as decisive on the question; On the Circulation of the Blood, p. 76 et seq. Rossi also, in his Elémens de Méd. Oper., speaks of the muscular coat of the arteries as an admitted fact, and remarks that "l'essence de l'artere" resides in this part; t. i. p. 216. Dr. Milligan has arranged the facts and arguments that have been brought forwards in this controversy in a tabular form, which may be advantageously consulted; Trans. of Magendie, p. 584..6. See also some judicious remarks on the controversy by Dr. Quain, Anat. p. 71..4. In speaking of the origin of the forces by which the blood is circulated, I must not omit to notice the peculiar opinion of Raspail. He attributes a considerable share of the action of the vessels, and even of the heart itself, to the double function of aspiration and inspiration, which is exercised by their internal coats; New System, § 897..9. Dr. Roget observes that the "capillary arteries, besides being elastic, are likewise endowed with muscular power, which contributes its share in forwarding the motion of the blood, and completing its circulation;" Bridgewater Treat. v. ii. p. 355.

to make some observations upon the peculiar affection of the arteries which is denominated the pulse. It is well known that if the finger be applied with a moderate degree of force to certain of the arteries, a degree of pressure or increased resistance is experienced by the finger during the instant of the systole of the heart, or that period when the heart propels its contents into the arterial trunks. This constitutes the pulse; and until lately it was always conceived to depend upon the dilatation of the vessels produced by the increased quantity of blood that was projected into them during the contraction of the ventricle. The acuteness of Bichat led him to doubt whether the dilatation of the artery actually existed, or even, if this were the case, whether it was sufficient to account for the phenomena, and he was disposed to refer it rather to a change in the position of the artery, produced by the sudden impetus of the jet of blood into it, analogous to the effect on the arch of the aorta, by which the apex of the heart is raised up and made to strike on the thorax. Still, however, the bulk of physiologists continued to attribute the pulse to the dilatation of the artery, until Dr. Parry instituted an experimental inquiry into the subject, the result of which was to show, that not the smallest dilatation can be perceived in the larger arteries, when they are laid bare during life, nor does he believe that there is any degree of displacement or unbending of the artery, which can account for the effect that is produced upon the finger[1]. He ascribes the pulse to "impulse of distention from the systole of the left ventricle given by the blood, as it passes through any part of an artery contracted within its natural diameter." This view of the subject appears to me to be correct; at the same time we may remark that it does not fundamentally alter our idea of the nature of the pulse, or the relation which its phenomena bear to the other parts of the system, or to the use that is made of it in pathology. According to this doctrine we must regard the artery as an elastic and distensible tube, which is at all times filled, although with the contained fluid not in an equally condensed state, and that the effect produced on the finger depends upon the amount of this condensation, or upon the pressure which it exercises on the vessel as determined by the degree in which it is capable of being compressed. Upon this principle we can easily account for the different conditions of the pulse, and the relation which each of them bears to the action of the heart, whether it expels a larger or a smaller quantity of blood, whether it is sent out of the ventricle with a greater or less force, whether the contraction be performed in a longer or shorter space of time, and other points of this nature, the consideration of which belongs more especially to the science of pathology [1].

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[1] We are, however, informed by Dr. Hastings, that the alternate dilatation and contraction of the larger arteries was sufficiently obvious to himself and his friends; it is implied, although not so stated, that this alternate action corresponds to, or was connected with the pulse; Treatise on the Mucous Membrane, p. 31, note. We are indebted to Poiseuille for the invention of an instrument, by which the degree of the dilatation may be accurately measured; *Blandin's* notes to Bichat, t. ii. p. 83. pl. c.

SECT. 7. *Efficient Causes of the Circulation.*

In considering this subject, we shall inquire first, into the nature and operation of these causes; and, secondly, into their amount or the degree of their effect. The causes themselves may be arranged under the heads of vital and mechanical, those which belong to the organs, as parts of the living body, and those which they possess when regarded merely as a system of elastic tubes or membranous cavities. The vital powers of the constitution may be all reduced to the single effect of muscular contractility, by which, as has been so often remarked, the size of the cavity is diminished, and the contents necessarily pressed out. This is a real source of power in which motion is generated which did not previously exist, and the ventricles of the heart are the main spring from which this force proceeds. A similar source of motion appears likewise to originate in the arteries, especially in their capillary extremities, by which they produce an actual addition to the motion of the sanguiferous system, subsidiary to that of the heart. The power of the arteries is, however, entirely subservient to that of the heart, and much inferior to it in quantity, for we find that the pulsations of every part of the arterial system, although situated at such various distances from the centre, are all nearly synchronous, and contemporary with the contraction of the ventricles. Hence it appears that the impulse which is given to the blood in the trunks of the great arteries is propagated to the remotest parts of the system, and that they are immediately brought into their state of distention.

It was a point which was much contested by the earlier physiologists, especially by those of the mechanical sect, whether the elasticity of the vessels promoted the flow of the blood through them. As a question concerning the mere quantity of power, we apprehend there can be no doubt of its insufficiency, because it is a principle which is now generally admitted, that simple elasticity is not a source of power, but only a means of distributing the power generated by other causes in a new direction. Although, therefore, the present constitution of the blood-vessels is much more convenient and more conducive to the well-being of the animal œconomy than a set of rigid tubes would have been, their membranous coats are not to be considered as, in any degree, contributing to the actual power by which the blood is propelled along them.

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[1] For some judicious observations upon this subject see an article in the Journal of Medical Science, No. 38. I shall beg also to refer to the art. "Pulse," in the Cyc. of Med.

Among the mechanical causes which promote the circulation of the blood there is one which is generally supposed to be considerably efficacious, especially in the venous part of the circulation, the pressure which the muscles, during their contraction, exercise upon the veins. Whenever a muscle contracts, so as to have its ends brought nearer to each other, its belly is proportionably increased in thickness, and it is evident that in this change of form all the tubes that pass through the muscles, or in contact with them, will be compressed. When this compression is exercised upon an artery, its operation will be rather to retard, than to promote the flow of the blood, in consequence of its diminishing the capacity of the vessels, and rendering them less able to yield to the distending force of the heart; but in the veins any effect of this kind, will be much more than compensated, in consequence of the numerous valves which these vessels contain. As the veins are less firm or dense than the arteries, and are generally so situated as to be more subject to be acted upon by the muscles, it follows that the effect of external pressure will be experienced in a greater degree than by the arteries, and therefore that the balance will be considerably in favour of the transmission of the blood towards the heart. The use of these valves is sufficiently obvious from their form, and it is still further proved by the circumstance, that those veins alone are furnished with them which accompany the muscles, especially those of the extremities, while the veins belonging to the internal viscera, and those connected with the system of the hepatic circulation, are not provided with them.

There is a mechanical cause which has been pointed out, as contributing to the circulation of the blood, to which the name of derivation is applied. When the ventricles have contracted, and have expelled their contents into the arteries, they relax, and by this means become again increased, and consequently a vacuum would be produced in them, were it not that they are immediately filled by the flowing in of the blood from the contiguous auricles. The fluid in this case is supposed to leave the auricle, not from the muscular contraction of this organ, but from its necessarily flowing into that part where there is the least pressure. The effect of derivation, as influencing the circulation of the blood, is a circumstance which seems to have escaped the older physiologists; it is mentioned, although obscurely, by Haller, but was first brought forwards in a clear and definite manner by Wilson, in a short essay, which contains some ingenious speculations, connected with much of what is incorrect, both as to matter of fact and the inferences that are deduced from them. After remarking upon the insufficiency of the force of the heart to propel the blood through the whole course of the circulation, he goes so far as to state that the heart is not the origin of the motion of the blood, and that it *even acquires* no actual addition of motion in passing through the heart. The sole, or at least by far the most important power of the heart, according to this author, is its faculty of absorbing the blood from the veins, which it does upon the principle of an exhausting machine; by throwing out the blood from its cavities a space is left into which the contents of the veins are immediately discharged, because there is the least resistance in this direction[1].

The views which had been thus imperfectly opened by Wilson have since been more fully disclosed by Dr. Carson. In an elaborate dissertation, in which he very fully discusses the nature and extent of the powers of the circulating system, he so far coincides with the generally received opinion, as to conceive, that the contractile force of the heart transmits the blood through the arteries, and even into the extremities of the veins, but that this *vis à tergo*, as it is termed, would not be competent to complete its return into the right auricle. To accomplish this object Dr. Carson has recourse to what he terms the power of suction, which, on the principle suggested by Wilson, impels the venous blood into the cavities of the heart, to fill up the vacuum which would otherwise be formed there. This diminution of resistance to the entrance of the venous blood into the heart is brought about in two ways: it is supposed that the construction or disposition of the muscular fibres of the ventricles is of that kind, that when they relax, the organ is necessarily dilated, a circumstance which depends upon the fibres being twisted, so as to give the parietes of both the ventricles a shape somewhat resembling that of the figure 8[1]. A still more powerful agency, however, he conceives to be derived from the action of the lungs. These, it is supposed, are always in a state of forced distention, and would consequently collapse by the pressure of the atmosphere, were they not contained in a rigid case which secludes them from its operation. In one part, however, where the membranes of the heart are connected with the pleura, the walls of the thorax are merely membranous, and are therefore subject to the influence of this external pressure, which, acting through the intervention of the pericardium, keeps its cavities in a state of dilatation, and as the external surface of the heart is always in contact with the internal surface of the pericardium, the auricles must also of course be kept distended, except when they contract by their vital energy. Hence, what may be termed the natural condition of both the auricles and ventricles is a state of dilatation, so that immediately after each of them have completed their systole, and relaxation ensues, they become again dilated, and necessarily receive the blood which is contiguous to them in the veins, as the valves will prevent the return of that which has once passed through them[2]. I conceive the general conclusions of Dr. Carson to be fully established, as far as respects the inadequacy of the contraction of the heart and arteries to return the blood to the right auricle, and also the actual existence of the principle of derivation, by which the venous blood is poured into the auricle and ventricle, because it meets with less resistance in this quarter than in any other direction. I am, however, disposed to doubt the validity of some parts of his reasoning, while there are certain positions which appear to be decidedly incorrect. In the latter predicament I consider the alleged condition of the lungs, as being always retained in a state of forced distention, a point which I shall discuss more at large when I treat upon the function of respiration, while among the doubtful positions may be placed the supposed dilatation of the heart by the mere relaxation of its fibres. If we conceive of the relaxation of a set of fibres of this description, as forming the parietes of a double cavity, it will have the effect of leaving this cavity in what may be termed a passive state, so as readily to yield to any distending force from without; but this is merely a negative effect, and will not account for the entrance of the fluid into it without some other more active principle. This I conceive to be a mechanical cause depending upon the structure of the heart, as consisting of a substance which possesses a great degree of elasticity, so that when its cavity is diminished by the contraction of the muscular fibres, as soon as the contractile power ceases to operate, the elastic force comes into play, and tends to bring it back to its former shape. We may form a clear idea of the operation of this supposed principle by regarding the heart in two ways, first, as consisting of a flexible and inelastic bag similar to a moistened bladder; and, afterwards, as composed of a bag of similar dimensions formed of caoutchouc[1]. We may imagine that each of these has the same apparatus of muscular fibres; when the first of them is filled with blood, the muscular fibres contract, reduce its size, thereby expelling its contents, and leave the bag in a collapsed state; whereas, in the second case, after the fibres have contracted and expelled the blood, the elastic nature of the organ causes it to resume its rounded form, and to leave a cavity nearly as considerable as before the operation. This cavity will of course be immediately filled by any fluid which is in contact with it; and in the case of the heart, the blood that is in the auricles, without any action on their part, will immediately flow into the ventricles, and will itself be succeeded in the auricles, by an equal quantity of blood from the veins. We have, perhaps, no very decisive experiments or observations to prove that this is actually the state of things with respect to the heart, but it appears very probable that it is so, at least to a certain extent, and therefore I do not hesitate to enumerate derivation, or, as it might be more correctly and less hypothetically termed, the elasticity of the heart itself, as among those causes which assist in the circulation of the blood[2].

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[1] Wilson's Enquiry, p. 9, 11, 16, 35 et alibi. I received the following communication from the late Dr. Goodwyn a short time only previous to his death, and I am confident that I cannot render my readers a more acceptable service, than by laying it before them in the words of the writer.—" With respect to the older physiologists, I think it may be allowed, that Bartholine had a notion of derivation in the diastole, although he did not employ the precise word. ' Diastole,' he says,' ' motus accidentarius, est dilatatio cordis; *ut hauriatur* sanguis, per venam cavam, in dextrum ventriculum, et per arteriam venosam in sinistrum;' and although he afterwards says of the diastole, ' passio potius est quam actio,' yet that does not destroy the effect of his first assertion. See Anatomia Bartholiniana, 8vo edit. p. 371. And, with respect to the moderns, I think there is one author anterior to Dr. Wilson, who brought forward in a clear and definite manner an assertion, supported by facts, that the diastole of the heart contributes mainly to carry on the circulation of the blood, by exerting the power of suction, and thus drawing into its cavity the blood from the trunks of the veins. I have said, this doctrine was brought forward by an author, which you may probably think not correct, as it was only in an inaugural dissertation; the writer was J. T. Vanderkemp; the title was ' De Vita et Vivificatione Materiæ Corpus humanum constituentis;' printed at Edinburgh, anno 1782. In page 52, his words are, ' Docet autem peropportune, physiologica ratio, sanguinem per arterias, alterna cordis arteriarumque contractione et *dilatatione* permoveri.' Upon this he has the following note: ' Mirum videri poterit, me hoc loco, cordis et vasorum sanguinem vehentium diastolem, inter causas sanguinem moventes retulisse, quum ex vulgari doctrina, cor suæ dilatationi reluctans, aut ad summum passive se habens; Sic fere Hallerus, El. Phys. t. i. p. 398; ab impulso sanguine explicari, et distendi credatur. Sed certe ubi cor ranæ vel anguillæ ex corpore exscissum, post singulas contractiones, nullo sanguine distendente, plena se diastole restituens video, ubi mecum repeto, omnem fibram muscularem, remoto post contractionem stimulo, propria virtute in eundem statum se recipere, in quo ante contractionem versabatur, ubi porro in cadavere etiam flaccidum cor auriculasque vitali turgescentia destitutas, pendentesque, vel sic tamen a perfecta diastole propius abesse animadverto; ubi tandem ad explicandum sanguinis venosi motum progressivum vix sufficere sentio, anteriosi sanguinis a tergo urgentis vim insitam; diffiteri non possum, in hanc me fere adduci sententiam, dilatatum vi suæ fabricæ, cor attrahere sanguinem æquabili vi illi, qua eundem expellit in systole, et diastoles causam non esse repletionem cordis, sed contra diastolem repletionis, quemadmodum in respiratione, *thoracis expansio, irruentis* aeris causa est, non effectus.' "

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[1] See Roget's Bridgewater Treatise, p. 138, note, where this decussation, or crossing of the fibres, is pointed out, as a contrivance, by which the blood is expelled with the smallest amount of contraction.

[2] *Carson's* Inq. pp. 97, 108, 117 et alibi.

Contrary, however, to what appears to be the opinion of those who have treated on this subject, I cannot admit that the principle of derivation is an actual source of power, or that there is any generation of motion produced by it. Like other forces which depend upon elasticity, it is to be regarded solely as giving a new direction to a power previously existing, the origin of which, in this case, is the contractility of the ventricles. It may indeed be regarded as a very beautiful contrivance for equalizing the force employed in the circulation, and for transferring, as it were, part of it from the commencement to the termination of the passage of the blood along the vessels; but I apprehend that precisely as much power as is gained by the influx of the blood from the auricles into the ventricles, must have been previously exercised by the muscles of the ventricle in overcoming the increased resistance caused by the increased elasticity of its substance. We may conclude, therefore, that the efficient causes of the circulation of the blood are the contractility of the muscular fibres of the heart, that of the capillary arteries, and external pressure upon the veins, principally produced by the contraction of the muscles[1].

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[1] See Prof. Turner, in Ed. Med. Chir. Tr. v. iii. p. 225. This appears also to be the opinion of Bouillaud.

[2] It is to this elasticity of the heart that we may refer a certain degree of re-action which it appears to exert during its diastole, and which Magendie observes is something more than a mere passive operation; Physiol. t. ii. p. 329. The same opinion, as has been stated above, p. 97, was maintained by Bichat. On the subject of Dr. Carson's hypothesis, see the observations of Dr. Hastings, in his treatise on the mucous membrane, p. 8 et seq.; also Dr. Philip, in Med. Chir. Tr. v. xii. p. 397 et seq.; and in Phil. Trans. for 1831, p. 489 *et seq.*

After considering the causes or powers which promote the flow of the blood along the vessels, it will be necessary to take a cursory view of those which tend to retard it. These are very numerous, and some of them very considerable; they may be all referred to the head of mechanical causes, because any circumstance which tends to diminish the vital energy of the heart or arteries acts merely in a negative manner, diminishing the effect of their contractility. The most important of the mechanical causes which retard the motion of the blood are the following; the physical composition and structure of the vessels, as being imperfectly elastic, flexible, pursuing a winding course, ramifying into branches which go off at considerable angles from the trunks, the branches occasionally uniting, by which the streams of blood meet each other in opposite directions, and the circumstance of the sum of the areas of all the branches being greater than that of the main trunk. The resistance which the blood already in the vessels opposes to the entrance of any fresh quantity is the cause of a prodigious expenditure of power, while the nature of the blood itself, as being a thick and tenacious fluid, its adhesion to the sides of the vessels, and the friction which it must experience in passing along so extensive a system of tubes, afford other causes of the loss of power, the amount of which must be very great. Those who are versed in the laws of hydraulics will perceive the reality of all these causes of retardation, and will be aware that they operate independently of the vital power of the organs, acting upon them as they would on a system of tubes possessed of the same physical properties, but without any of those functions which are peculiar to the living animal.

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[1] The nature of the primary moving force which produces the circulation has been made the subject of a valuable treatise by Dr. Wedemeyer, of Hanover, of which we have an analysis in the thirty-second volume of the Edin. Med. Journ. The most important of his conclusions are, that the heart is almost the sole moving power, and that the capillaries do not contribute to the propulsion of the blood by any alternation of contraction and dilatation. In Magendie's Journ. t. x. p. 277 et seq. we have an account of some experiments performed by Poiseuille on the cause of the motion of the blood in the veins, especially with a view to the hypothesis of derivation; his conclusion is, that the dilatation of the chest and the right side of the heart may act as an accessory cause of this motion; p. 292. Dr. Alison, after enumerating the various facts and opinions that have been brought forwards on this subject, concludes, that the arteries possess "a truly vital power of contraction;" Physiol. p. 36. The principle of derivation has been applied by Sir D. Barry to explain the beating of the heart. He opened the thorax of an animal during life, and, by introducing his hand into the cavity, he endeavoured to ascertain the actual condition of the heart and great vessels, as to their state of distention, and their relative position. He performed seven experiments of this kind, and concluded from them that the vena cava is considerably increased in size during inspiration, which he ascribes to the partial vacuum which is then formed in the chest. The force which the venous blood exerts in entering the heart, in consequence of the expansion of the chest and the great vessels behind the heart, is supposed to push this organ forwards, and thus to cause it to strike against the ribs. The expansion of the chest thus attracts the blood, and causes it to fill the great veins, in order to occupy the partial vacuum which would otherwise be produced. I shall take occasion, in a subsequent part of the work, to enter more fully into the merits of Sir D. Barry's hypothesis, as affecting the functions both of respiration and of absorption.

When we consider how much the powers of hydraulics are concerned in the circulation, we cannot be surprised that the mechanical physiologists, who applied their science to many points of the animal œconomy to which it had so little relation, should have bestowed unusual attention upon every circumstance connected with the action of the heart, and the motion of the blood along the vessels. We shall accordingly find that some of their most elaborate calculations were directed to this subject, and that they bestowed upon it no less attention than upon the cause and nature of muscular contraction. Upon the whole, the result of their labours has been almost as unfortunate in this as in the former case, for although the data are less obscure, as far as concerns the nature of the powers employed, yet we are totally unable to ascertain the amount both of the powers which promote and those which retard the circulation of the blood.

With respect to mathematical reasoning in general, it must be admitted that, when it is cautiously applied, it has enabled us to arrive at physiological truths, which we perhaps could not have attained by any other method, and which are beyond the reach of actual observation. But when we call in the aid of mathematics to assist us in our researches, it is of the utmost importance to ascertain that our data be well founded, and that we are not misled by false analogies, or by the mis-application of principles which may be in themselves correct. But the mechanical physiologists fell into the fatal errors of assuming principles which were incorrect, of adopting data which were of doubtful authority, and of applying them in an incorrect manner.

To relate all the theories, hypotheses, and calculations that have been formed upon the subject of the circulation, would be an idle expense of time and labour, but it may be proper to give an account of some of the attempts that have been made to estimate the force of the heart, because they were conceived by men eminent for their learning and ability, and afford an excellent specimen of the method of reasoning that was fashionable at the time when they flourished.

Borelli, proceeding upon his hypothesis, that the power of muscles is in proportion to their weight, estimated that the force of the heart is equal to the enormous sum of 180,000 lbs. Keill perceived the extravagance of Borelli's calculation, and attempted to arrive at the truth by a more complicated process. He set out by the position that the force of the heart produces two effects; it expels a quantity of blood from its cavities, and communicates motion to the contents of the arteries. He first attempted to estimate the quantity of blood thrown out of the heart by each of its contractions; and by taking the diameter of the aorta, he could then calculate the velocity with which it passes along this vessel. He found the quantity of blood to be about two ounces, the area of the great vessel to be about three-fourths of a square inch, and he conceived that the actual contraction of the ventricle would occupy about the two-hundredth part of a minute. Hence he found that the blood sent into the aorta would compose a cylinder of about eight inches in length, and be driven along with a velocity of 156 feet in a minute. In producing this velocity the heart has not only to expel the blood, but to overcome all the resistances in the vessels, and the next step was to ascertain their amount. For this purpose he opened a living animal, and laid bare the iliac artery and vein. Now he argued that all the blood which passes through any artery must be returned by the corresponding vein in the same time, but with a different degree of force; the arterial blood has to overcome all the resistances which occur in the course of the circulation, the venous blood possesses only the force which remains after the resistances have been overcome. He opened the iliac vein and received all the blood that flowed out in ten minutes, and afterwards he opened the artery and received the blood that flowed out during the same length of time, when he found that the quantity of blood which he obtained from the artery was to that from the vein as seven and a half to three, or as two and a half to one. He therefore concluded that the first of these numbers may be regarded as the measure of the velocity which the blood receives from the full force of the heart; and the second, the velocity with which it moves after it has overcome all the obstacles which it meets with in its passage through the vessels. Proceeding upon this principle, the velocity in the aorta, without the resistances, is estimated at 390 feet in a minute, or nearly six feet and a half in a second. From this datum, by means of the Newtonian theorem, he estimates the force which is necessary to move a *given* column of blood, with a known velocity in a given time, and this he determines to be five ounces and a half, or about half a million of times less than the calculation of Borelli.

We cannot but give the merit of ingenuity to Keill's reasoning, but it is obviously incorrect in many particulars. In the first place, a great mass of resistance is opposed to the blood at its entrance into the aorta, which must have been overcome before it arrives at the iliac artery, on which Keill made the experiment. In the second place, the quantity of blood which flows from a divided vessel is no measure of what passes through it at other times, as on the principle of derivation, blood will be sent from all the neighbouring vessels to a part where the resistance is removed. In the third place, we are not certain that the same quantity of blood is returned by what are called the corresponding veins; and it is also probable that, in Keill's experiments, a greater quantity than ordinary would pass off by the veins; in consequence of the anastomoses that veins have with each other. But it is unnecessary to dwell any longer upon these objections, enough having been said to show that the estimate does not afford even an approximation to truth.

The only other calculation of this kind which I shall notice, is that of Hales. Hales was remarkable for the purity and integrity of his character, and his ardour in philosophical researches, and he bestowed a large part of his time on experimental inquiries into the nature of vegetable and animal bodies. He had too much candour to be blindly devoted to any sect, but the genius of the age in which he lived was so decidedly in favour of the employment of mathematical reasoning in every department of philosophy, that he entered very fully into all the views of his predecessors respecting the mechanical powers of the sanguiferous system. He attempted to estimate the relative force of the arteries and veins by inserting tubes into the great vessels near the heart, and observing the comparative height to which the blood was impelled into them. This he found to vary in different experiments, but it was always considerably greater in the arteries than in the veins, upon an average, as about ten to one. In order to ascertain the absolute force of the heart, Hales inserted tubes into the aorta, soon after it leaves the left ventricle, when he found the column of blood raised in the tube to be of such a height, that by comparing it with the cavity from which it was projected, and taking into account the time and the area of the vessel, the force of the heart would be about 50 lbs.[1] Perhaps Hales's estimate may not be very remote from the truth, yet there are many points in which it is defective, even regarding the heart as an hydraulic machine; and it is obvious that when we consider contractility as a variable power, depending upon a number of causes connected with life, which it is impossible to appreciate, we shall be convinced of the futility of such calculations. Hales's works, however, contain much important information, the direct result of experiment, which will always render them a valuable magazine of facts for the philosophical inquirer into the actions of the sanguiferous system[1].

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[1] Statical Essays, v. ii. p. 38, 40.

Sect. 8. *Of Inflammation.*

Before I quit the subject of the circulation it may be proper to offer a few remarks on the nature of inflammation, a point indeed more immediately connected with pathology, but yet of considerable importance considered in its physiological relations. The phenomena of local inflammation, to which I mean principally to confine my observations, vary according to the structure and functions of the part affected, yet there are some circumstances which may be considered as common to all cases, of which the four following are considered the most essential, redness, heat, pain, and swelling[2]. It is generally agreed that the capillaries are the immediate seat of the inflammatory action, and that when any change occurs in the large vessels, it is to be attributed to a secondary or consequential operation, originating from the affection of the minute arteries. All the four symptoms mentioned above, redness, heat, pain, and swelling, may be attributed to one primary cause, an increase in the size of the minute vessels, by which they are enabled to admit of more than the ordinary quantity of blood to be received into them, but a great diversity of opinion has prevailed concerning the mode in which the blood is conveyed or retained there, or concerning what has been termed the proximate cause of inflammation.

After the vital power of the vessels had been established by Stahl and his disciples, the phenomena of local inflammation were referred to this action, and as all the natural operations of the arterial system seemed to be augmented during this state, so it followed, almost as an obvious consequence, that inflammation essentially consists in increased action of the capillaries[1]. A strong objection to this supposition was, however, quickly perceived, that the effect of the vital action of the vessels is contraction, while, as we observed above, the very essence of inflammation consists in an increase of the capacity of the vessels. But the ingenuity of the physiologists was not baffled by this difficulty; various means were invented to accommodate the theory to the facts, depending upon the presence of some obstacle which the blood was conceived to meet with in its passage through the vessels, which, at the same time, was not incompatible with their increased diameter. Boerhaave attributed the obstruction to a change in the texture of the blood itself, by which it became more thick and viscid, acquiring, what he called, a state of *lentor*, and to this *lentor* he added the further hypothesis, that the increased action of the arteries forces the larger particles of the blood into vessels, which, in their natural condition, are too small to receive them. This constitutes the *error loci* of the mathematical physiologists, and by the *lentor* and *error loci* were the phenomena of local inflammation explained by Boerhaave and his numerous disciples[2], until this, like most of their other speculations, was assailed by the powerful genius of Cullen. In place of the mechanical doctrine of Boerhaave, Cullen substituted his favourite hypothesis of spasm[3]; for he admitted the increased action of the vessels in local inflammation, while he was aware, that without some counteracting circumstance, this increased action would produce effects totally inconsistent with the actual phenomena[4]. We shall, however, find, upon due reflection, that the spasm of Cullen is equally unfounded, and perhaps even less intelligible, than the *lentor* and *error loci* of the Boerhaavians; and accordingly his explanation has been generally regarded as incomplete, yet since his time no regular attempt has been made to reconcile the increased action of the vessels with their enlarged diameters.

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[1] The latest estimate of the force of the heart, with which I am acquainted, is that of Dr. Arnott, who supposes it to be equivalent to 60 lbs.; but this confessedly applies to the left ventricle only, the inner surface of which is stated to be equal to 10 square inches, and the pressure on each inch to be equal to 6 lbs.; Elem. of Phys. p. 537, 8. We have an account of an essay on the subject by Poiseuille, in the Edin. Med. Journ. v. xxxii. p. 28 et seq., in which the author makes some judicious observations on the experiments of previous physiologists, and gives us the result of his own estimate, which appears to be very different from that of any of his predecessors; he supposes that the force with which the heart propels the blood in the human aorta is not much more than 4 lbs.; see also Magendie's Journ. t. x. p. 241 et seq.

[2] Cullen's First Lines, § 235; Thomson's Lectures on Inflammation, p. 42; see also, Hunter's Treatise on the Blood, c. 3, a portion of this celebrated work, which cannot be too carefully studied, both for its valuable observations and for its profound insight into the operations of the animal œconomy; yet even here we have to lament the metaphysical subtilties in which the author is occasionally involved.

In this dilemma a totally different view of the question has been taken, according to which local inflammation is to be attributed to a diminished action of the capillaries. This hypothesis, which appears to have been originally proposed by Vacca, an Italian physiologist, about the middle of the last century, was first brought forwards in a clear and consistent form by Mr. Allen, who, for some years, lectured in Edinburgh on the animal œconomy; it was adopted by Dr. Wilson Philip[1], who performed a series of experiments in support of it, and has been since embraced by Dr. Parr[2], Dr. Thomson[3], and Dr. Hastings[4].

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[1] For a clear and candid statement of the opinions which have been entertained upon the state of the blood-vessels in inflammation, I may refer the reader to Dr. Thomson's Lectures on Inflammation, p. 61..75; and Hastings's Treatise on the Mucous Membrane of the Lungs, p. 67 et seq.

[2] Aphor. 110, 122, 370 et seq. cum Sweiten. Comment.

[3] First Lines, § 244 et seq.

[4] Haller was quite aware that an increased action of the arteries must have a tendency to diminish their capacity, and employed this consideration as an argument against the muscularity of the capillaries.

According to Mr. Allen's hypothesis, the redness, heat, pain, and tumour, are to be ascribed to the increased quantity of blood which the vessels contain in consequence of their relaxed state; the symptoms, therefore, which had been usually attributed to excessive action, he ascribes to this partial stagnation of the fluids, together with a kind of struggle between the loss of power in the part, and the unusual stimulus to which it is thus subjected. Although this doctrine of the proximate cause of inflammation may, at first view, seem to counteract our established notions upon the subject, yet it will, I think, be found upon examination to be more consistent in all its parts, and to accord better with the various phenomena both of pathology and of physiology, than any of the speculations which had preceded it, derived from the principle of increased action combined with obstruction. And indeed if we take into account that the exact seat of the inflammatory action is not visible to the eye, and that, according to the hypothesis of Dr. Hastings, the state of active inflammation consists in an increased action of the larger arteries, while the capillaries are in their natural state[5], it will approximate this hypothesis, at least as to all pathological and practical consequences, pretty nearly to the old doctrine. But I must add, that I think we are not arrived at that degree of knowledge on the subject which will enable us to form a decisive opinion respecting it. Waiving, therefore, the regular discussion of the hypothesis, I shall conclude by a few observations, which may be of some use to those who are inclined to investigate the proximate cause of inflammation with more minuteness[1].

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[1] Wilson on Febrile Diseases, v. iii. p. 15..73.

[2] Dictionary, Article "Inflammation," v. ii. p. 13 et seq.

[3] Lectures on Inflammation, p. 70.

[4] On the Mucous Membrane, p. 71 et seq. Dr. Thomson and Dr. Hastings likewise supported the hypothesis by numerous experiments. Although Philip, Thomson, and Hastings, agree in the main point, that inflammation essentially consists in diminished action of the capillary arteries, they differ respecting the actual state of the vessels. Dr. Philip supposed that the constant effect of inflammation is to dilate the vessels, and to diminish the velocity of their contents; Treatise on Febrile Diseases, v. iii. p. 15 et seq.; also Preface to 4th ed. p. vii.; and Med. Chir. Trans. v. xii. p. 407. Dr. Thomson concludes that the velocity is sometimes increased and sometimes diminished; Lect. on Inflammation, p. 89; while Dr. Hastings adopts the opinion of Dr. Philip, that in the proper inflammatory state, the velocity of the fluids is always retarded; On the Mucous Membrane, p. 91 et alibi. Mr. Mayo also conceives that local action depends upon, or is accompanied by relaxation of the vessels; Physiol. p. 67..71. It may be worth noticing, that the article "Inflammation," in the Dict. des Scien. Méd. t. xxiv. p. 525 et seq., written by Boyer in 1818, contains no account of either the hypothesis or experiments of the English physiologists: inflammation is referred, according to the old doctrine, to the increase of vital action.

[5] On the Mucous Membrane, p. 106.

Although we may conceive that the phenomena of inflammation are as readily explicable upon the hypothesis of diminished as of increased contractility of the capillaries, yet it appears to me that both the exciting causes of this affection and the treatment coincide more with the idea of excessive than of defective action. All those circumstances which we are usually in the habit of considering as stimulants excite inflammation; and where the same effect is brought about by sedatives, or by agents of a more doubtful operation, still we can generally perceive the existence of what has been termed re-action, which is the immediate precursor of the change in the state of the circulation. In the same way the remedies for inflammation appear to me more adapted to remove or relieve an excess than a defect of vital energy, as for this purpose, except under peculiar circumstances, we always apply either direct or indirect sedatives, and find stimulants to be as injurious as the others are beneficial. From these considerations I am induced to recur to the former idea of increased action being the proximate cause of inflammation, or at least as being essential to it, and to inquire whether there be no correct method of combining a state of increased action with distention of the vessels. This, it is obvious, must be accomplished by obstruction in some form or other, either arising from the nature of the fluid, or from the difficulty with which it leaves the vessels after it has entered into them. Now, although we may agree with Cullen that Boerhaave adduced no sufficient proof of the existence of his *lentor* and *error loci*, yet it does not follow that no alteration in the condition of the blood exists. May we not conceive, that by the inflammatory action the proportion of fibrin is increased, or that the fibrin already present acquires a greater tendency to coagulate? That the solid contents generally of the blood are augmented, either by an increased quantity being thrown into the vessels, or a portion of the more fluid part being removed? May not some new arrangement take place with respect to the globules, so that they may coalesce or be more strongly attracted together? Or, without having recourse to any speculations of this kind, may we not conceive it possible, that if the minute arteries are contracting more vigorously than ordinary, their relaxation will be proportionably great, so as to allow of the different parts of the blood, as the fibrin and the globules, to be admitted into vessels which are generally impervious to them, and which, when once entered, from the *vis à tergo* on the one hand, and from the decreasing diameter of the vessels as they divide into small branches on the other, are forcibly detained, and produce all those symptoms which seem to originate from mechanical obstruction? These considerations are thrown out rather to show what may be conceived as a possible occurrence, than from the idea of our possessing any evidence of their actual existence. They may, however, show that we have not a sufficiently intimate acquaintance with the subject to enable us to decide peremptorily respecting the proximate cause of inflammation.

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[1] The theory of inflammation has been lately discussed in an ample and luminous manner by Mr. James Earle, in a series of essays, which appeared during the last spring, in the successive numbers of the Medical Gazette. He conceives that the essence of inflammation consists in obstruction of the minute vessels, and that its greater or less degree of activity depends principally upon the relation between the degree of obstruction and consequent dilatation, and the impulsive force of the blood. He does not admit of the proper contractility of the capillaries, and therefore refers all the actual force of the circulation to the action of the heart. On this, as well as on some minor points, I must dissent from the doctrine of Mr. Earle, but I must recommend the attentive perusal of his essays to those who are interested in the progress of physiological science, as containing much valuable information on the various topics on which he treats. Dr. Alison takes a totally different view of the subject; he ascribes the effect to a vital action in the fluids, not in the vessels or in any of the solids; Ed. Med. Journ. v. xlv. p. 98 et seq.

CHAPTER VI.

OF THE BLOOD.

SECT. 1. *Remarks on the Progress of Animal Chemistry.*

AFTER the description of the course which the blood follows in the circulation, and of the mechanism by which its motion is produced and regulated, I must now proceed to consider those functions by which its physical and chemical properties are changed, so as to become subservient to the growth and nutrition of the body. The blood is, however, a fluid of a very compound nature, the different constituents of which are possessed of peculiar properties, and are retained in a state of combination, in many respects, unlike that of any other substance with which we are acquainted. It will therefore be desirable, before we proceed to the other functions, to give some account of the nature and properties of the blood; to this I shall prefix a few remarks upon the history and present state of animal chemistry.

During the earlier part of the eighteenth century, when chemistry first began to assume a scientific form, and when the experimentalist proposed to himself some definite and intelligible object of inquiry, the analysis of animal and vegetable substances naturally attracted a share of his attention. The mode of examination which was then adopted was indeed little calculated to throw any light upon the subject, for it consisted almost entirely in submitting the substances to the process of distillation at a high temperature, by which their primary compounds or proximate principles were entirely destroyed, and either converted into new compounds, which did not previously exist, or resolved into their ultimate elements. Many of these latter were of a gaseous or volatile nature, and from the operator being, at that period, ignorant of the properties, or even of the existence of such bodies, they were suffered to escape, and were totally disregarded, while the solid or fluid products that were obtained were, in all cases, nearly similar to each other, and conveyed no idea of the nature of the substance from which they were procured. They principally consisted of a carbonaceous residuum, with a quantity of empyreumatic oil and ammonia, which two latter substances were generated during the process.

The first radical improvement in animal analysis consisted in substituting the action of various re-agents for this method of destructive distillation, an improvement for which we are principally indebted to the French, and especially to some members of the Royal Academy of Sciences, who, about the middle of the last century, were led to investigate the composition of organized bodies, and soon perceived the little benefit that had been derived from the former method of proceeding. One of the most active of those who were engaged in this new pursuit was Rouelle; he subjected the substances which he wished to analyze to the action of alcohol, acids, alkalies, and other powerful reagents; he noticed the effect of simple exposure to the atmosphere, he examined the changes produced by temperature and moisture, and, at the same time, carefully watched the progress of spontaneous decomposition. Neumann of Berlin, and some other of the German chemists, proceeded upon the same plan with Rouelle and his associates, and, at a somewhat later period, Macquer and Baumé materially contributed to our knowledge of this department of chemistry by their publications. Hitherto it had been little attended to in this country; Hales indeed made the important discovery, that a permanent gas was obtained from various animal substances by distillation[1], but it was an insulated fact, the nature of which was not understood, so that it led to no further improvement; and it was not until the establishment of the pneumatic chemistry, as it has been called, for which we are so much indebted to Black, Cavendish, and Priestley[2], that the nature of the results could be understood, or the true method of analysis be clearly comprehended. The experiments of Priestley, in which he obtained azotic gas by treating animal substances with nitric acid[1], constituted a very important advance in our knowledge; but we must admit that it was chiefly by the labours of the French, and especially by those of Fourcroy, Vauquelin, and Berthollet[2], that the foundation was laid of the correct information which we at present possess on this subject. Latterly, indeed, the English have had at least an equal share in promoting the science of animal chemistry; I have already had occasion repeatedly to refer to the labours of Mr. Hatchett, and, in addition to his name, we may select those of Wollaston, Pearson, Marcet, Henry, Prout, Brande, J. Davy, Thomson, and Ure. On the continent, those who may be selected as among the most distinguished in this department, are Gay-Lussac, Thenard, Chevreul, Proust, Bouillon la Grange, Braconnot, Tiedemann, Gmelin, Dumas, Pelletier, Caventou, Rose, Brugnatelli, Wöhler, and Lecanu; and we must not omit to acknowledge the great obligation under which we lie to Sweden, formerly in the person of Scheele, and now in that of Berzelius, whose genius and assiduity have rendered him almost equally illustrious in every branch of chemistry[3].

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[1] He informs us, that he obtained "a considerable quantity of permanent air" from blood, fat, and various other animal substances, and more especially from urinary calculi; Stat. Ess. v. i. p. 173, 193, (1769.) Scheele's Experiments on Calculus were published in 1776; see Marcet on Calculous Disorders, p. 63.

[2] It was from the latter of these philosophers that I received my first instruction in chemistry, and I cannot mention his name without offering to his memory my grateful tribute of respect and admiration. His merit as a chemical discoverer of the first order is so generally acknowledged, that it may appear almost unnecessary to enlarge upon it, yet I believe that few persons who have not particularly attended to the subject are aware of the full extent of our obligation. Those who are disposed to investigate this point should peruse his original six volumes of experiments, and compare the information which they contain with the chemical publications which immediately preceded them. In originality, in quickness of apprehension, and in diligence, he has probably never been surpassed; but I conceive that his judgment was by no means equal to his genius, for, although we are astonished with the variety and extent of his discoveries, we are not unfrequently surprised at the futility of his hypotheses and the weakness of the arguments by which he defends them. As far as I am capable of forming an opinion, the same character will apply to his other publications. Of his strictly theological works I do not profess to judge; but two of his most elaborate performances, which were intended for general use, the "Disquisitions," in which he attempts to identify the phenomena of mind with those of matter, and the "Letters to a Philosophical Unbeliever," the object of which is to prove that the credibility of supernatural events rests upon the same grounds with that of the ordinary operations of nature, while they abound with ingenious remarks and original conceptions, appear to me to be both of them founded upon fallacious principles.

The method at present employed in the examination of animal substances may be considered as combining three distinct sets of operations. The first consists in noticing the effect of external agents upon the substance in question, and observing its spontaneous changes; the second depends upon the application of re-agents, which are used either in the way of tests, to indicate the existence of particular elements or primary compounds, or as menstrua, which, by their specific affinities, may separate the elements or primary compounds from each other; while the third set of operations are to be regarded as, in some measure, a return to the original plan of destructive distillation, but with this very essential difference, that in the modern analysis, we carefully collect the gaseous and volatile matter, and by ascertaining its nature and the amount of its elements, we estimate the nature and amount of the compounds into which they previously entered as constituents. We procure, for example, a certain quantity of carbonic acid gas and of water, and we know the proportion of carbon, oxygen, and hydrogen, which they respectively contain; the azote remains uncombined and is collected in the gaseous state, so that we are able to ascertain the amount and relative proportion of the elements which entered into the constitution of the substance analyzed. This method of discovering the nature of a body, by resolving it into other bodies of known composition, was first practised, although in rather a rude manner, by Fourcroy and Vauquelin; it was afterwards very much improved by Gay-Lussac and Thenard, and still more so by Prof. Berzelius and Dr. Prout, from whose science and skill it has arrived at a very great degree of perfection [1].

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[1] Priestley's experiments, in which he procured "phlogisticated air" by the action of nitric acid on animal substances, were published in 1775; Experiments on Air, v. ii. p. 145 et seq. (Original series of six volumes.) Fourcroy says that Berthollet *discovered* azote in animal substances in 1784; System, v. ix. p. 42.

[2] See Berthollet's Memoirs on the Analysis of Animal Substances; Mém. Acad. for 1785; also Journ. de Phys. t. xxviii. p. 272, et t. xxix. p. 389.

[3] For a sketch of the progress of animal chemistry, see Fourcroy's System, v. ix. p. 33..56; and Berzelius's essay expressly on this subject. It is much to be regretted that these writers should have entirely omitted to give any particular references to the experiments or opinions which they detail.

SECT. 2. *Nature and Properties of the Blood.*

I shall now revert to our more immediate object, the nature and properties of the blood. After describing this fluid in its entire state, I shall give an account of the primary compounds, and proximate principles, into which it may be separated, either by its spontaneous changes, or by the application of re-agents. In the next place I shall notice some of the alterations that are brought about in the blood by the natural functions of the system or by the effects of disease, and I shall conclude by some observations upon the opinions that have been successively entertained respecting its various constituents [2].

Blood, when first drawn from the vessels, is an adhesive fluid of an homogeneous consistence, of a specific gravity of about 1·050 [3], in man and the more perfect animals, of a red colour, of a slightly saline taste, and, in the human subject, of the temperature of about 98°. Soon after it leaves the vessels, if it be suffered to remain at rest, it begins to coagulate; and as this process advances it will be found to separate into two distinct parts, so that we at length obtain a red mass floating in a yellowish fluid: the red part is called the clot or crassamentum, and the fluid part the serum[1]. In venous blood, which is generally employed in these experiments, the average period of coagulation is said to be about seven minutes. The proportion of the two constituents has been variously estimated; it is not easy to obtain any accurate result, because the separation is by no means complete, a portion of the serum always remaining attached to the clot; and by attending to the state of the blood, we find that the proportion varies considerably in different individuals, and even in the same individual at different times. It has been stated, as a general average, that the crassamentum amounts to about one-third of the weight of the serum, and perhaps this may not be far from the truth.

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[1] For an account of the modern analysis of animal substances, see Thenard's "Traité," t. iv.; Children's Translation of the same, containing much valuable additional matter; an Essay by Berzelius, in the 4th and 5th vols. of Annals of Philosophy; Dr. Ure's paper in Phil. Trans. for 1822; and the various papers of Dr. Prout, in the Med. Chir. Trans. and the Annals of Philosophy, and most especially his paper in the Phil. Trans. for 1827.

[2] Before I enter upon the subject of the blood I may refer my readers to the "Experimental Researches" of Denis; Magendie's Journ. t. ix. p. 176 et seq., of which an abstract is given by Lecanu in Journ. Pharm. t. xvii. p. 522 et seq., and to a series of original researches by Lecanu, in the same work, t. xvii. p. 485 et seq., and 545 et seq.; of this we have an extract in Ann. Chim. t. xlviii. p. 308 et seq. Also to the following systematic works, Berzelius's Chem., by Jourdan and Esslinger, t. vii; Henry's Chem. ch. 13. sect. 1; Turner's Chem. p. 958 et seq.; Alison's Physiol. sect. 4; Mayo's Physiol. ch. 2, and Elliotson's Physiol. ch. 10. Raspail displays the same originality in this as in most of the other parts of his work; § 887 et seq. We have some original observations on the constitution of the blood by Prof. Muller, in Ann. Sc. Nat. t. i. 2d ser. p. 342 et seq. I may also refer my readers, for an account of the chemical constitution of the blood, to the valuable art. "Sang," by Orfila, in Dict. Méd. t. xix. p. 56 et seq.

[3] Dr. Davy states the sp. gr. of arterial blood to be 1·049, that of venous blood to be 1·051. Dr. Milne Edwards makes it rather greater; he says that *it varies from* 1·052 to 1·057: Cyc. of Anat. v. i. p. 404.

As the fluid of warm-blooded animals, when first drawn from the vessels, possesses a temperature considerably greater than that of the atmosphere, it has been made the subject of experimental inquiry, what is the rate of the cooling of the blood, compared to that of water raised to the same temperature. The greater viscidity of the blood must necessarily tend to retard the escape of heat from it, but, besides this, it has been conceived that the coagulation of the fibrin, like other processes in which a fluid is converted into a solid, should cause the absolute extrication of caloric. Fourcroy relates an experiment, in which, during the formation of the clot, the thermometer rose no less than 11°[2], but as the particulars were not mentioned, and as the result appeared to be in contradiction to some facts adduced by Hunter[3], and others, the conclusion was not generally admitted. Fourcroy's experiment has, however, been confirmed by some that have been lately performed by Gordon, in which the effect of coagulation in evolving caloric was rendered most evident, by moving the thermometer, during the formation of the clot, first into the coagulated, and afterwards into the fluid part of the blood, when he found that by this means he could detect a difference of 6°, and that the difference remained perceptible for 20 minutes after the process had commenced. In repeating the experiment upon blood, drawn from a person labouring under inflammatory fever, the rise of the thermometer was no less than 12°[4]. I conceive, therefore, that this point may be considered as established, that during the coagulation of fibrin a quantity of caloric is extricated, thus proving that fibrin has a less capacity for heat in its coagulated than in its uncoagulated state.

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[1] Dr. B. Babington has lately given us a different view of the constitution of the Blood, and of the relation of its component parts to each other; he employs the term liquor sanguinis to designate the liquid part of the blood generally, as consisting of fibrine, albumen, and various other substances, held in solution by water. His paper contains some interesting experiments and valuable observations, but I do not perceive that any advantage is gained by the adoption of his peculiar nomenclature; Med. Chir. Trans. v. xvi. p. 293 et seq. See also the observations of Prof. Muller on this subject; Ann. Sc. Nat. t. xxvii. p. 208 et seq.; he conceives that the fibrin is dissolved in the serum.

[2] From 20° to 25° R.; Ann. de Chim. t. vii. p. 147.

[3] Hunter on the Blood, p. 27.

[4] Annals of Philosophy, v. iv. p. 139; a similar result was obtained by Sir C. Scudamore; Essay on the Blood, p. 68. I must, however, observe that Dr. Davy has *not found this elevation* of temperature to take place;

SECT. 3. *Fibrin.*

The crassamentum, when removed from the serum, generally appears under the form of a soft solid, of such consistence as to bear cutting with a knife: it frequently assumes a fibrous appearance, and when it has been coagulated under particular circumstances, it may be converted into an irregular net-work, consisting altogether of fibres, of a greater or less degree of fineness, according to the manner in which the process has been conducted. The best method of exhibiting this fibrous appearance is to stir the blood, as it flows from the vessel, with a bunch of twigs, or to receive it into a bottle, and shake it during its coagulation, but it must be observed, that if the motion be too considerable, the clot is altogether prevented from forming. The coagulum which has been produced in the usual manner, while the blood is at rest, may also be deprived of its red colour by repeated ablution in water, thus showing that the colouring matter is only mechanically mixed with the fibrin, and not retained there by any chemical affinity. When the fibrin is thus procured in a pure state, it is found to be a solid of considerable consistence, elastic and tenacious, and in its general aspect, as well as in its chemical relations, very similar to the pure muscular fibre, although we have reason to suppose that it differs from it in its minute organization. It has been designated by several names, as coagulable lymph, gluten, fibre of the blood, and fibrin; this last appellation was given to it by the French chemists, and will be adopted in this work, as being the most characteristic and appropriate.

It is obviously upon the fibrin that the formation and separation of the clot depends, thus producing what has been termed the spontaneous coagulation of the blood, in opposition to the other kinds of coagulation, which are effected by the application of heat, or of some chemical re-agent. A change so singular in its nature could not but excite great attention among physiologists, and numerous observations and experiments have been made to account for its occurrence, or to discover what circumstances tended to promote or retard it. The two most obvious circumstances which might be supposed to operate, as constituting the chief difference between the condition of the blood while in the vessels, and after it is discharged from them, are rest and exposure to air. I have already alluded to the effect of agitating the blood; it is well known that if the fluid,

Journ. of Science, v. ii. p. 246; and that Raspail states that the temperature actually falls during coagulation. Dr. M. Edwards is disposed to coincide in opinion with Dr. Davy; Cyc. of Anat. p. 413.

as it is discharged from the vessels, be briskly stirred about for some time, the process of coagulation is entirely prevented from taking place, either in consequence of a more complete union of its parts with each other, which prevents their future separation, or from the fibrin, after it has been for some time discharged from the blood, losing this peculiar property, by which its particles are attracted together. It is probable that both these causes may operate, but I am not acquainted with any facts which can enable us to determine which of them has the most powerful effect.

With respect to the influence of air upon the spontaneous coagulation of the blood, we have not yet arrived at any very positive conclusion. Many experiments were performed on this subject by Hewson, and although they are not sufficiently decisive, nor always uniform in their results, yet they lead to the opinion, that the presence of air promotes coagulation[1]. Hunter opposed the doctrine of Hewson[2], but his experiments and arguments go no further than to show, that air is not essential to the process; a point which was fully admitted by Hewson, and which follows immediately from his experiments. This circumstance is proved by the fact, that coagulation not unfrequently takes place in the vessels or cavities of the body, where the blood must be completely excluded from the air; and indeed this change has been found to exist, to a certain degree, during life, as the polypous concretions that are occasionally found in different cavities of the body, and which, from the previous symptoms, as well as from their appearance and texture, must, at least in some cases, have existed before death, are chiefly composed of fibrin. Many of the wonderful stories that are recorded, and sometimes on very good authority, of worms being found in the chambers of the heart, the arch of the aorta, the sinuses of the brain, or the large veins, must be explained upon the supposition, that the spectators have been deceived by portions of coagulated fibrin, in the form of long strings, possessing somewhat of the shape and form of worms, to which a lively imagination and fondness for the marvellous have added the other properties of these animals.

Besides the action of the atmosphere generally on the coagulation of the fibrin, experiments have been made upon the effect of its constituent parts taken separately, and also of other gases; but although the examination has been pursued with considerable diligence, the results are not so decisive as might perhaps have been expected. It must, however, be confessed, that in performing experiments of this kind, much manual dexterity is requisite, and that there are a variety of circumstances connected with the state of the atmosphere itself, the manner in which the blood flows from the body, the kind of vessel in which it is received, the temperature and other conditions of the gas in question, and the manner in which it is brought into contact with the blood, all of which may produce a notable difference in the result. We find, therefore, that although some respectable authors would lead us to suppose that oxygen retards the progress of coagulation, and that it is promoted by carbonic acid and some other of the unrespirable gases, yet we are informed by Sir H. Davy, that he could not perceive any difference in the period of the coagulation of venous blood when it was exposed to azote, to nitrous gas, to oxygen, to nitrous oxide, to carbonic acid, to hydrocarbon, or to atmospheric air[1].

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[1] Experimental Enquiries, p. 20.

[2] On the Blood, p. 22.

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[1] Researches on Nitrous Oxide, p. 380. We are indebted to Sir C. Scudamore for "An Essay on the Blood," which is principally occupied with an account of various observations and experiments which he performed, for the purpose of illustrating the circumstances which influence its coagulation. The most important points to which his attention was directed respect the effects of temperature, exposure to the air, the question whether carbonic acid is evolved from blood during its coagulation, the connexion between the rapidity of its coagulation and its specific gravity, the manner in which it flows from the vessels, the form of the cup into which it is received, the effect of vitality, of electricity, and of various chemical agents, he inquires whether heat be evolved during coagulation, and he offers some remarks on the formation of the buffy coat. The most important of the results appear to be the following: his experiments favour the opinion that carbonic acid is disengaged during the coagulation of the blood; blood which has the highest specific gravity coagulates the most rapidly; coagulation is promoted by the blood being drawn slowly from the vessel, and by being received into small shallow cups, probably in consequence of its heat being in this case abstracted more rapidly. When blood exhibited the buffy coat it coagulated more slowly; when it is extravasated, or remains in the vessels after they have lost their vitality, it coagulates very slowly; electricity appears to promote coagulation; heat is disengaged during coagulation, although in small quantity only; it was found that the quantity of fibrin was considerably increased in blood that exhibited the buffy coat, and that the proportion of fibrin was much greater near the surface of the clot than at its lower part.

I must also refer my readers to some original observations on the subject by Dr. Thackray, in his Inquiry into the Nature of the Blood, a treatise which contains a full account of the opinions entertained by contemporary physiologists on the nature and properties of the blood, together with a number of original experiments on this substance, both in its healthy and its morbid state. The most important of the original observations are those on the coagulation of the fibrin: from these he draws the conclusion, that "blood coagulates slowly, in regular proportion to the tonic state, or that condition of the system in which the vital powers are strongest;" p. 47. The fifth chapter is "on the cause of the blood's coagulation," and contains an account of a variety of experiments which were instituted for the purpose of examining the effect of those agents which are commonly supposed to influence this process.

Dr. Davy has lately performed a series of experiments on various points connected with the chemical and physical properties of the blood, and especially on what respects the formation of the buffy coat. He conceives that pure fibrin is heavier than serum, but that the mixture of fibrin and serum, which constitutes the buffy coat, is lighter than the mixture of fibrin, serum, and red particles, and therefore floats on the surface. He conceives that in inflamed blood, the serum and coagulable lymph are less viscid than ordinary. The specific gravity of the red particles he found to be 1.087.

I have mentioned above that the spontaneous coagulation of the fibrin is prevented by sufficient agitation, and the same effect is produced by the addition of certain neutral salts. Hewson, to whom we are principally indebted for these facts, found that the sulphate and muriate of soda and the nitrate of potash were among the most powerful salts in this respect, so much so, that if we add to a portion of blood rather less than one-twentieth of its weight of any one of them, the coagulation does not take place. On what this depends we are entirely ignorant; it would not appear to be upon any tendency in the salt employed to dissolve the fibrin, because the neutral salts do not possess this property, at the same time that potash, which is the proper solvent of fibrin, has less power in retarding its coagulation[1]. Besides the neutral salts, the mere dilution of blood with a sufficient quantity of water will effectually prevent its spontaneous coagulation. We are informed by Crawford, that when blood is mixed with twelve times its bulk of water, no coagulation was observed to take place for several hours[2], an effect which may perhaps be explained merely by the particles being removed to so great a distance from each other, as to be placed beyond the reach of their mutual attraction.

Among the changes which attend the coagulation of the fibrin, I may remark that its specific gravity is said to be increased by this process; but this is a point which it must be difficult to ascertain, in consequence of the serum and the red globules which are always mixed with the clot, besides that the firmness of the clot, and consequently its specific gravity, differ very much in different cases. Haller, as it would appear upon the authority of Jurin, states that the specific gravity of the crassamentum is 1.126[3], the serum being only 1.030[4]; but the fact which was long since observed by Boyle, that the crassamentum floats in the serum, so as to preserve the surface of the two nearly on a level, would seem to show that they cannot differ much in their specific gravity.

Much has been written about what is termed the halitus of

The formation of the buffy coat does not appear to bear any exact relation to the specific gravity of the blood; in acute diseases, the blood, whether buffed or not, is generally of greater specific gravity than ordinary; in diseases of debility, the reverse. The formation of the buffy coat is supposed to depend on the viscidity of the blood, as connected with the proportion of water, or the complete mixture of its ingredients. He controverts some of the positions of Sir C. Scudamore detailed above, respecting the circumstances under which coagulation takes place, and the effect of various re-agents on the operation; Ed. Med. Journ. v. xxix. p. 244 et seq.; and v. xxx. p. 248 et seq.

[1] Dr. Turner informs us, that the coagulation of the blood is prevented "by a saturated solution of chloride of sodium, hydrochlorate of ammonia, nitre, and a solution of potassa. The coagulation, on the contrary, is promoted by alum, and the sulphates of the oxides of zinc and copper; Chem. p. 967.

[2] On Animal Heat, p. 278.

[3] *El. Phys.* v. 2. 5.

[4] v. 3. 1.

the blood, or the vapour which arises from it when it is first drawn from the body. Plenk, who has paid the most attention to it, calls it gas animale sanguinis; he conceives it to be composed of hydrogen and carbon, and that it produces many important effects in the animal œconomy[1]. But I believe that this opinion is altogether unfounded, as the halitus is nothing more than the aqueous vapour, which necessarily arises from a fluid considerably warmer than the air in contact with it, and which, during its evaporation, carries up a very minute quantity of animal, and perhaps even of saline matter[2].

The cause of the coagulation of the fibrin has never been satisfactorily explained: it is a phenomenon which does not exactly resemble any other with which we are acquainted, and the operation of external agents upon it is not so well marked, as to enable us to refer it to any general operation of the physical properties of matter. What renders the subject more difficult is, that there are some circumstances which affect the coagulation of blood in a manner that we are quite unable to explain. Many causes of sudden death have this effect; lightning and electricity[3]; a blow upon the stomach, or injury to the brain; the bites of venomous animals, such as the viper and the rattlesnake; some acrid vegetable poisons, as laurel-water; also excessive exercise, and even violent mental emotions, when they produce the sudden extinction of life, prevent the usual coagulation of the blood from taking place[4].

A remarkable coincidence has been observed in these cases between the want of coagulability in the fibrin of the blood, and the diminution of contractility in the muscles after death. They are all found in a state of relaxation, incapable of being excited by the accustomed stimuli; and it has been further observed that the body is disposed to run rapidly into the state of decomposition. These facts appear to identify, at least to a certain degree, the property of muscular contraction with that of the coagulation of the fibrin, and this identity is further supported by considering that the chemical composition of fibrin is similar to that of muscle. For the knowledge of this relation between the coagulation of the blood and the contraction of the muscles we are principally indebted to Hunter, who noticed it with much attention, and built upon it some of his favourite physiological speculations. It is indeed probable that we may trace to this source his celebrated hypothesis of the life of the blood, a doctrine which is founded upon the principle that a fluid is capable of organization, and that it may possess functions either identical with, or very similar to, those which are the most characteristic of the living animal solid. According to this hypothesis, the blood is supposed not merely to be the substance which gives life to the animal, by carrying to all parts what is necessary for their support and preservation, but that it is properly itself an organized living body, and even the peculiar seat in which the vitality of the whole system resides[1]. The question of the life of the blood cannot be fully examined, until we are further advanced in our view of the animal œconomy, and especially, until I have endeavoured to define the manner in which the term life ought to be employed. But I may remark, that even were the Hunterian doctrine of the life of the blood to be fully established, it would not offer any explanation of the cause of its coagulation; for the same difficulty still remains, in what manner the presence of life operates so as to produce either the coagulation of the blood or the contraction of the muscles.

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[1] Hydrologia, p. 42.

[2] Fourcroy, Syst. v. ix. p. 185. Barruel has, however, lately endeavoured to prove, that this halitus possesses something of a more definite character, and that it contains a specific volatile principle peculiar to each animal, and which has a considerable resemblance to the fluid of perspiration; Journ. of Science, v. vi. p. 187.

[3] This is, however, denied by Sir C. Scudamore.

[4] *Hunter* on the Blood, p. 26.

Perhaps the most obvious and consistent view of the subject is that the fibrin has a natural disposition to assume the solid form, when no circumstance prevents it from exercising this inherent tendency. As it is gradually added to the blood, particle by particle, while this fluid is in a state of agitation in the vessels, it has no opportunity of concreting; but when it is suffered to lie at rest, either within or without the vessels, it is then able to exercise its natural tendency. In this respect the coagulation of the fibrin of the blood is very analogous to the formation of organized solids in general, which only exercise their property of concreting or coalescing under certain circumstances, and when those causes, either chemical or mechanical, which would tend to prevent the operation, are not in action. Upon this principle, we shall be induced to regard the coagulation of the blood as analogous rather to the operation by which the muscular fibre is originally formed, than to that by which its contractile power is afterwards occasionally called into action; for, notwithstanding the relations pointed out by Hunter, we shall find that the operations are esentially different in two very important particulars, in the causes which produce them, and in the subsequent state of the parts. The causes of muscular contraction, as we have already had occasion to observe, are exclusively stimulants of various kinds, but it does not appear that any one of these, numerous and various as they are, has the smallest effect in promoting the coagulation of the blood. With respect to the subsequent state of the parts, in the muscle contraction is always succeeded by relaxation, whereas nothing at all resembling this ever occurs in the blood; the fibrin, when once formed, remains unchanged as long as it retains its chemical composition. Upon the whole, therefore, although we must acknowledge the validity of the facts pointed out by Hunter, we are at present scarcely prepared to form them into a consistent theory; and we must content ourselves with the simple statement, that the fibrin of the blood and the muscular fibre possess, the former the property of coagulation, and the latter that of contraction, which are acted upon in the same manner by various circumstances, although we are not able, in these cases, to perceive the relation of cause and effect[1].

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[1] *Hunter on the Blood*, p. 76.

Before I dismiss the subject of the coagulation of the fibrin, I must advert to a circumstance which is of great importance in the practice of medicine, that the nature and appearance of the coagulum vary very much according to the state of the body at the time when the blood is drawn. The most important of these variations consists in what is called the size, or buffy coat of the blood, a term employed to denote that state of the crassamentum, when the upper part of it contains no red particles, but exhibits a layer of a buff-coloured substance, lying on the top of the red clot. This buffy coat is generally formed when the system is labouring under inflammatory fever, and when, according to the modern doctrines of pathology, there is supposed to be an increased action of the arteries. The immediate cause of this appearance in the crassamentum is obvious; the globules, or other matter which gives it the red colour, begin to subside before the coagulation is completed, so that the upper part of the clot is left without them. The remote cause of the buffy coat is not yet ascertained, although many experiments have been made to discover it. Hewson thought that the fibrin became specifically lighter, and, of course, the red particles comparatively heavier, whence they would be disposed to sink to the lower part of the clot; he also thought that the blood coagulated more slowly[2]. Hunter was inclined to account for the appearance by the firmer coagulation of the fibrin, as it were, squeezing out the red particles: but this would scarcely explain why the upper part of the clot alone is left without them. Hey's opinion is perhaps better founded, that by the increased action of the vessels the different constituents of the blood are more intimately mixed together[3], while Dr. Davy opposes the opinion of Hewson as to the fact of the slower coagulation of inflamed blood[4]. From some experiments that were performed on the composition of the buffy coat by Dr. Dowler, it appears that it contains a considerable proportion of serum[1], and this, by diminishing its viscidity, will more readily allow of the subsidence of the red particles. It is, however, not improbable that Hunter's opinion is in part correct, for we find that the clot of inflamed blood obviously possesses a firmer texture than in its ordinary state, so that sometimes, in consequence of the contraction of the clot, after it has begun to form, the surface has a depression in the centre, forming what is called the cupped state of the coagulum. And here we have another analogy between the blood and the muscles; for there are several circumstances which lead us to conclude, that the force of muscular contraction through the system generally is increased in inflammatory fever[2].

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[1] Upon the same principle which induced me to notice John Hunter's hypothesis of the action of the heart, I shall quote his opinion respecting the coagulation of the blood. "My opinion is, that it coagulates from an impression: that is, its fluidity under such circumstances being improper, or no longer necessary, it coagulates to answer now the necessary purpose of solidity;" On the Blood, p. 25.

[2] Experimental Enquiries, p. 39, 59 et alibi.

[3] Observations on the Blood, p. 10, 19 et alibi.

[4] Phil. Trans. for 1822, p. 271.

It is probably upon the fibrin that the property which the blood possesses of repairing the injuries of the solids principally depends, a property which affords us one of the most interesting examples of the resources of the animal œconomy, while, at the same time, it very forcibly illustrates the slow progress of medical information, when the mind has once received an impulse in a wrong direction. Every one who is acquainted with the history of surgery must have heard of the sympathetic powder, which, about the middle of the seventeenth century, engaged the notice and received the sanction of the most learned men of the age. This celebrated remedy derived its virtues not from its composition, but from the mode of its application, for it was not to be applied to the wound, but to the weapon by which the wound was inflicted; the wound was ordered to be merely closed up, and was taken no further care of[3]. Most men of sense, indeed, ridiculed the proposal, but after being fully tried, it was found that the sympathetic mode of treating wounds was more successful than those plans which proceeded upon what were considered scientific principles; and it continued to gain ground in the public estimation, until at length some innovator ventured to try the experiment of closing up the wound without applying the sympathetic powder to the sword. Wiseman, who wrote about fifty or sixty years after the introduction of this mysterious operation by Sir Kenelm Digby, in describing the importance of keeping the divided parts in close union, says, "for here nature will truly act her part, by the application of blood and nourishment to both sides indifferently, and finish the *coalitus* without your further assistance. And this is that which gives such credit to the sympathetic powder."[1]

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[1] Med. Chir. Trans. v. xii. p. 91. Hewson conceived that the buffy coat is composed of fibrin, but that in inflammation the fibrin acquires a thinner consistence, p. 34, 45 et alibi. We are indebted to Dr. Stoker for a series of observations on the cause of the buffy coat of the blood, and on the state of the fluid in which this particular appearance is disposed to manifest itself. The conclusion which he draws from the examination of twenty-seven specimens of blood, in various inflammatory affections is, that the formation of the buffy coat does not depend upon any purely mechanical cause, but upon a diseased state of the blood, which is referred more especially to a changed or imperfect chylification; Pathol. Obs. p. 37..42 et alibi.

[2] It may not be uninteresting to peruse the account which Sydenham gives of the buffy coat of the crassamentum of the blood in pleurisy; his observations will be commonly found to be correct, although his hypotheses are too often fallacious; Observ. circa Morb. Acut. Hist. § 6. c. 3.

[3] See "A late Discourse, &c." by Sir K. Digby, a treatise which admirably exemplifies the mode of philosophizing that was fashionable in the earlier part of the *seventeenth century*.

Although we are now well acquainted with the general facts respecting the re-union of divided parts, yet there is much obscurity concerning the exact mode of the operation. We find that when two newly cut surfaces, which were not previously connected, are laid in close apposition, and the air carefully excluded, they will unite, and when the operation is performed under favourable circumstances, the trace of the wound is scarcely perceptible, either in the structure or functions of the part. What will appear more wonderful is, that parts belonging to different organs are capable of contracting this close union, the arteries, veins, and even the nerves, becoming connected, each to each; and to add still further to the marvellous aspect of these operations, we cannot avoid giving our assent to the fact, that portions of the body, which had been entirely cut off, or even of a different body, if speedily applied to a recently divided surface, will unite and retain their functions[2]. Although, in a great majority of instances, we shall find ourselves in the right, if we disbelieve the wonderful tales that we find in the works of the older writers, yet our scepticism may be carried too far; and accordingly it is now admitted, that the operations of Taliacotius, or Tagliacozzi, a name which could not until lately be admitted into a serious discussion, were founded upon correct principles. It appears, indeed, that a process of an analogous nature had been long practised in India, and has been introduced into this country, with complete success, by Mr. Carpue.

As I have already stated, we have reason to believe that the fibrin is the intermedium by which the process is effected, yet

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[1] Chir. Treat. b. 5. c. 1. p. 342.

[2] Hunter caused the spurs of a young cock to adhere to the comb of another cock, and the testicles were found to become united to the internal cavities of other animals. He remarks, "The most extraordinary of all the circumstances respecting union, is by removing a part of one body and afterwards uniting it to some other part of another, where, on one side, there can be no assistance given to the union, as the divided or separated part is hardly able to do more than preserve its own living principle, and accept of the union;" Treatise on the Blood, p. 208. I may notice in this connexion the observations of Is. St. Hilaire, on what he styles, "anomalies par jonction et par fusion," contained in the work to which I have already had occasion to refer; p. 535 et seq. He remarks, that the preternatural union of parts is regulated, not so much by their contiguity as by the similarity of their nature, according to the general principle announced by G. St. Hilaire, which he somewhat quaintly names, "l'affinité de soi faire soi." See the article "Monstre," in the *Dict. Class.* d'Hist. Nat. t. xi.

I confess that I know of no rational method in which we can explain the successive steps of the operation. We may, indeed, conceive of the divided end of an artery, which belongs to the cut surface nearest the heart, discharging a portion of its fibrin, which may coagulate and form a basis, or nidus, as it has been termed, through which the current of blood may afterwards form a new channel; but in what way this stream is to discover the ends of the arteries of the other surface, by what power it is to enter them, how these insulated parts are to propel their blood into the veins, and lastly, how the veins of the divided part are to transmit their contents into the veins of the body, are questions that at present, I apprehend, we are not able to answer[1].

A curious series of facts relative to the coagulation of the blood, and to the mode in which portions of coagulum acquire an organized structure, has been lately brought forward by Sir Everard Home, principally derived from the microscopical observations of Mr. Bauer. It is stated that a quantity of carbonic acid is always present in the blood, that, during its coagulation, this acid is extricated, and that by its extrication it forms linear passages or tubes in the substance of the blood, into which the vessels of contiguous parts are elongated, and which become the rudiments of the future arteries. It would appear that the serum is the nidus in which these tubes are formed, as they are said to be altogether independent of the globules, which are supposed to be the more immediate constituents of the fibrin. It is not expressly stated whether the tubes are themselves converted into blood-vessels, or whether they only afford spaces in which vessels are formed, nor does it seem quite clear what connexion the spontaneous coagulation of the fibrin has with the production of the tubes. Without calling in question the accuracy of Mr. Bauer's observations, it may be remarked, concerning the hypothesis that is deduced from them, that the formation of regular tubes could not be the result of the extrication of gas in a viscid fluid, the formation of the tubes must therefore be the result of a tendency in the fluid in question to assume an organic arrangement. The observations of Mr. Bauer are valuable, as showing the mechanical structure which the blood exhibits, during the process by which it becomes organized, but, I apprehend, they throw no further light upon the operation[2]. We cannot be surprised that so subtile a property as that by which the blood is enabled to acquire an organic arrangement, should be destroyed by its being subjected to the action of the air-pump[1].

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[1] It may be amusing to observe the uses which are ascribed to the crassamentum of the blood by Plenk, a writer of extensive information and general accuracy. The uses which he points out are three; 1. (to employ his own words) "sanguini ruborem conciliat;" 2. by the gravity of its metallic ingredients, it irritates the heart and arteries more effectually than the lighter particles of serum; 3. at the same time it imparts motion to the lighter particles of the serum.

[2] Phil. Trans. for 1818, p. 181 et seq.; for 1820, p. 2.

With respect to the chemical properties of fibrin it is sufficient to remark, that they appear exactly to resemble those of the muscular fibre; it is acted upon in the same manner by nitric acid and the other re-agents, so as to be fully entitled to the appellation of liquid flesh, which was bestowed upon it by the older physiologists. For the first correct account of the chemical relations of fibrin we are indebted to Fourcroy, Vauquelin, and Berthollet: it has been lately examined by Berzelius, whose experiments may be regarded as the most correct that we possess upon the subject[2].

Sect. 4. *Red Particles.*

After having described the fibrin, I proceed to the other constituent of the crassamentum, the red particles or globules[3]. These, from the singularity of their appearance and organization, have attracted an unusual share of attention, and have been the subject of almost innumerable observations and experiments. Soon after the microscope was introduced into anatomical researches, these peculiar bodies were observed by Malpighi, and were afterwards more minutely examined by Leeuwenhoek. They were at first described simply as globules floating in the serum, and giving the blood its red colour, but as observations were multiplied, errors and absurdities were advanced in an almost equal proportion. Leeuwenhoek himself invented a fanciful hypothesis, which had a long and powerful influence over the most enlightened physiologists, that the red particles of the blood were composed of a series of globular bodies descending in regular gradations; each of the red particles was supposed to be made up of six particles of serum, a particle of serum of six particles of lymph, &c.[1] This hypothesis, for which there really does not appear to be the slightest foundation, was so suited to the mechanical genius of the age, that it was generally adopted without any examination of its truth, received as the basis of many learned speculations in that and the succeeding age, and even formed a leading feature in the pathological speculations of Boerhaave. The futility of the doctrine was exposed by Lancisi and Senac, but it still maintained its ground until the time of Haller.

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[1] Dr. Davy has published the result of some experiments on the subject, from which, as well as from other considerations, he concludes that Sir Everard Home's hypothesis is so far unfounded as respects the existence of carbonic acid in the blood. Dr. Davy remarks that the gas observed by Mr. Bauer, is probably azote, implying his belief that gas of some kind or other is contained in the blood. His experiments would, indeed, lead to the opinion, that there is no gas in recent blood; Phil. Trans. for 1823, p. 506. But, on the contrary, the existence of carbonic acid in blood, or at least the fact that carbonic acid may be disengaged from blood, by merely removing the atmospheric pressure, seems to be established by Vogel; Ann. Chim. t. xciii. p. 71 et seq., and Ann. Phil. v. vii. p. 57; by Mr. Brande, Phil. Trans. for 1818, p. 181; and by Sir C. Scudamore, p. 27; see also Dr. Elliotson's Physiol. p. 145, 6. This subject has been recently investigated by the Professors Tiedemann, Gmelin, and Mitscherlich; the result of their experiments is adverse to the conclusion of Vogel, Brande, and Scudamore; Brit. and For. Med. Rev. v. i. p. 590 et seq.; while the contrary doctrine is maintained by Dr. M. Edwards, in his art. "Blood," in the Cyc. of Anat. v. i. p. 415. Dr. Stevens has likewise endeavoured to prove the existence of carbonic acid in venous blood, by showing that hydrogen and oxygen gases are capable of attracting it from the blood, when it could not be removed by the air pump; Proceedings of the R. S. for 1834, 5, p. 334. It is difficult to decide between these conflicting opinions.

[2] Med. Chir. Trans. v. iii. p. 200.

[3] I have retained the ordinary description of the clot, as composed of fibrin and the red globules, although some late experiments, of which an account is given below, may throw some doubt upon its accuracy. I may remark that all the vertebrated animals have red blood, while in the invertebrated animals, with a few exceptions, the blood is nearly colourless.

Next to the observations of Leeuwenhoek, those of Hewson were the most elaborate, and have the appearance of great accuracy. He describes the red particles as consisting of a solid centre, surrounded by a vesicle filled with a fluid. He informs us, that by adding water to them they swell out, the surrounding vesicle becomes thinner, and at length bursts, and leaves no trace behind; the whole substance of the particles is soluble in water, and imparts to it their red colour. It appears also that those animals which have white, or rather colourless blood, possess particles which are supposed to be similar in their general form and organization to those of the red-blooded animals; we are likewise informed that they are of different sizes in different animals, and that their size bears no ratio to that of the animal from which they are taken, as, for example, they are said to be of the same size in the ox and the mouse, larger in birds, and to be the largest in the skate; in birds, amphibia, and in insects, they are elliptical, but, excepting in their form, they resemble those of the human subject. We have an account of some singular changes that are produced upon their colour and form by various chemical re-agents, especially by some saline bodies. Alkalies and acids, when moderately strong, first corrugate the vesicle, and afterwards seem to dissolve or decompose the whole of the globules; nitre has the property of considerably heightening the colour, so as to convert it to a bright red, while, on the contrary, there are some other substances by which their colour is destroyed without changing their form[2].

Hunter, who made many observations on these bodies, differs essentially in his account of them from Hewson. He is silent respecting the central nucleus and the investing vesicle; there are some animals in which he could not discover any globules, as the silkworm and the lobster, and he never observed them, in any case, to assume the elliptical form, described by Hewson. He does not regard them as properly solid bodies, but as liquids possessing a central attraction, which determines their figure; a circumstance that might identify them with substances of an oily nature, were it not stated, on the other hand, that the globules are miscible with water, and that they always preserve the same size, and are never disposed to run together or coalesce[1].

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[1] For an account of Leeuwenhoek's supposed discoveries respecting the constitution of the globules, see Martine, in Ed. Med. Ess. v. ii. p. 74 et seq.; the whole of this Essay is well worth perusing, as a curious specimen of the mode of prosecuting physiological inquiries, that was pursued by the learned mathematicians about a century ago. The earliest of Leeuwenhoeck's papers appears to be in the Phil. Trans. for 1674, No. 23.

[2] Phil. Trans. for 1773, p. 303 et seq. Tab. 12. Also Exper. Enq. v. iii. ch. 1.

The globules of the blood have been also examined by the Abbé Torré, by Monro, and more lately by Dr. Young, by MM. Prevost and Dumas, Dr. Hodgkin, and Dr. M. Edwards. Torré supposed them to be flattened annular bodies, or like rings composed of a number of separate parts cemented together[2]; to Monro they exhibited the appearance of circular flattened bodies like coins with a dark spot in the centre, which he conceived was not owing to a perforation, as Torré had imagined, but only to a depression[3]. Some remarks were published on the subject by Cavallo, who conceived that all these appearances were deceptive, depending upon the peculiar modification of the rays of light, as affected by the form of the particle, and he concludes that they are in fact simple spheres[4]. But the account which Dr. Young has given of these bodies, to a certain extent, coincides with the description of Hewson. He remarks that if the globules be viewed by a strong light, they will appear like simple transparent spheres, but that if we examine them by a confined and diversified light, we shall be better able to ascertain their real figure and structure. The particles of the blood of the skate, from their size and distinctness, are the most proper for this kind of examination, and he found their form to be like that of an almond, but less pointed and a little flattened; they consist of an external envelope containing a central nucleus. This nucleus is independent of the substance with which it is surrounded, for when this latter has been removed or destroyed, the nucleus still appears to retain its original form. The nucleus is much smaller than the part surrounding it, being only about one-third the length and one-half the breadth of the whole particle. We are informed that the entire particle of the human blood is not larger than the central portion of the particle of the skate. Dr. Young found that the particle in the human subject is flattened, and has a depression in the centre, somewhat as was described by Monro, but much less in degree[1].

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[1] On the Blood, p. 40 et seq. Mr. Bauer, however, found that when the investing vesicle is removed, the nuclei are disposed to unite together, and upon this property the formation of the muscular fibre is supposed to depend; Phil. Trans. for 1818, p. 176. MM. Prevost and Dumas, in like manner, suppose that the crassamentum of the blood, in the act of its spontaneous coagulation, is formed by the central globules adhering together in the form of fibres; Ann. Chim. et Phys. t. xxiii. p. 51.

[2] Phil. Trans. for 1765, p. 252 et seq.

[3] We must not omit noticing the result of Prof. Amici's examination of the globules, as given us in the Edin. Med. and Surg. Journ. vol. xv. p. 120. It may afford curious matter for speculation to those who place much confidence in microscopical observations; it is, however, proper to observe that the statement does not come directly from the author himself.

[4] *Medical Properties of Factitious Airs*, p. 237 et seq.

The observations and experiments which have been lately made upon the blood by Prevost and Dumas, present us with a view of its nature and constitution that is, in some respects, different from the one generally adopted. They regard it as essentially composed of serum, holding in suspension a quantity of red particles, which consist of central colourless globules enclosed in a coloured vesicle. When the fluid is drawn from the vessels, the central globules, in consequence, as it may be inferred, of the loss of their envelope, are attracted together, and disposed to arrange themselves in lines or fibres, thus forming the basis of the clot or crassamentum. These fibres, by the network which they form, mechanically entangle a quantity of the serum and of the colouring matter, which by simple draining, or by sufficient ablution in water, may be removed from them. What is then procured is the pure fibrin, which is thus identified with the central globule, and the clot generally with the entire particle[2]. The colouring matter is supposed to be a compound of a peculiar animal substance and the peroxide of iron; water is said to possess the property of breaking down these vesicles, and to detach them from their nuclei, but does not dissolve them. The authors do not appear to have examined the nature of this animal matter, nor to have made any particular observations upon the state or quantity of the iron, nor have they entered into any detail of the chemical relations of the albumen;

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[1] Med. Liter. p. 545. Mr. Bauer has observed that the form of the globules of the skate is oval during the life of the animal, but becomes flattened after its death; this circumstance may perhaps, in some measure, tend to reconcile the discordant statements that we meet with on this subject; Phil. Trans. for 1818, p. 174. We have some curious observations of Prevost and Dumas on the form and size of the globules in different classes of animals, from which it appears, that they differ considerably in these respects both from each other and from human blood; Ann. Chim. and Phys. t. xviii. p. 280 et seq.; Quart. Journ. v. xiii. p. 154..6; Edin. Phil. Journ. v. vii. p. 190, 1. See also Dr. M. Edwards, in Cyc. of Anat., art. "Blood;" where we have an account of the opinion of this acute observer on the various controverted points respecting the red globules, together with some original observations. Among these the most important are that the globules occasionally are found to differ in size in the same individual, p. 405; that the form and size of the globules in the invertebrate animals are irregular and variable, p. 408. His general conclusions with respect to the form and constitution of the globules are contained in p. 409; it will be seen that they do not agree, in all respects, with those of Dr. Hodgkin.

[2] I may observe that Berzelius always considers the fibrin and the red globes as distinct proximate principles; Prog. of Anim. Chem. p. 16, 23, 46.

they only state that various re-agents act upon it in the same manner as upon fibrin. The existence of the uncoagulable matter is recognized, and is characterized as a substance soluble both in water and in alcohol, and precipitable by the salts of lead; they suppose, with Berzelius, that it is combined with lactate of soda. Perhaps the most important part of their researches consists in the experiments on the proportional quantity of globules contained in the blood of different kinds of animals, and of different parts of the same animal. They appear to bear a ratio to its temperature, for we are informed, that the higher is the natural temperature of the animal, the greater is the proportion of red particles in the blood, and that arterial contains a greater proportion of them than venous blood[1].

Dr. Hodgkin's examination of the blood formed a part of the miscroscopical observations which he made, in conjunction with Mr. Lister, upon various animal substances: they inform us that they employed for this purpose an instrument, which was found on comparison to be equal to the celebrated one lately brought into this country by Prof. Amici. The first object to which they directed their attention was the globules of the blood, which, notwithstanding the older observations of Leeuwenhoek, Haller, and Fontana, and the more recent ones of Mr. Bauer and the French physiologists, are said to be not spherical, and not to consist of a central nucleus, enclosed in a vesicle. The description given of these bodies is that "the particles of the human blood appear to consist of circular flattened transparent cakes, which, when seen singly, appear to be nearly or quite colourless. Their edges are rounded, and being the thickest part, occasion a depression in the middle, which exists on both surfaces." We are told, however, that this convexity is not considerable, and in some of the particles is not to be observed. The estimate of the diameter of the particles is $\frac{1}{3000}$ of an inch; the thickness of the particles is about $\frac{1}{45}$ of their diameter. With respect to the particles of other animals, the observations of Dr. Hodgkin and Mr. Lister agree with those of MM. Prevost and Dumas, as to their "having a circular form in the mammalia, and an elliptical one in the other three classes." The diameter and the thickness do not bear the same ratio to each other in the different species. The elliptical particles are invariably larger than the circular, but are proportionally thinner; the particles are more numerous in birds, but smaller than in either reptiles or fishes. The central globules, which are described by Mr. Bauer and others as so obvious, and which are supposed by Dr.

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[1] Ann. Chim. and Phys. t. xxiii. p. 50 et seq. and 90 et seq.; Quart. Journ. v. xvi. p. 115 et seq. MM. Prevost and Dumas do not refer to any of Sir Ev. Home's papers on the blood, a circumstance which is to be regretted, as it would have been interesting to have learned how far their microscopical observations agreed with those of Mr. Bauer; we may, however, remark upon the coincidence between their account of the fibrin and Mr. Bauer's description of the origin of the muscular fibre.

M. Edwards to perform so important a part in the construction of the body, are not recognized by Dr. Hodgkin, and it would appear that the whole system of central nuclei and vesicular envelopes is equally unfounded. Sir Ev. Home's hypothesis, that the particles are not disposed to coalesce in their entire state, appears also to be incorrect; Dr. Hodgkin found them disposed to combine in this state only. This is best seen when the blood is viewed between two slips of glass; and under these circumstances the following appearances may be observed: "When the blood of man, or of any other animal having circular particles, is examined in this manner, considerable agitation is at first seen to take place among the particles; but as this subsides they apply themselves to each other by their broad surfaces, and form piles or rouleaux, which are sometimes of considerable length. These rouleaux often again combine amongst themselves, the end of one being attached to the side of another, producing at times very curious ramifications."[1]

I have had occasion, in various parts of this work, to give an account of the microscopical observations that have been made upon different substances; and, in consequence of their disagreement with each other, I have been induced to remark upon the little confidence which, in most cases, can be placed in them. My animadversions, however, have always proceeded upon general grounds; at the same time that I may appear to underrate the value of the observations, I acknowledge the perseverance and ingenuity of those physiologists who have devoted their time and attention to this object, and I ascribe their failure to the inherent deficiences of the instrument. And in no instance have I had more reason to adhere to my former opinion than in the present, where we find various physiologists of the highest respectability for moral and scientific character, giving a completely different account of a simple matter of fact. From the statements that are given above, especially from the abstract of Dr. Hodgkin's paper, the question resolves itself very much into one of personal authority. It does not depend upon the respective goodness of the instruments employed, because the different observers describe what they saw as being perfectly distinct and obvious, and not presenting that confused or uncertain aspect, which arises from a deficiency in the power of the lens. We are compelled to suppose that all the observers, excepting one, have fallen into some error, either depending upon an optical deception, or resulting from some unconscious and involuntary bias of the mind towards a previous hypothesis. Indeed, the more beautiful is the speculation, and the more completely do the observations chime in with all its parts, the more are they to be suspected. In conclusion, I may remark, that much as the naturalist has been indebted to the microscope, by bringing into view many beings, of which he could not otherwise have ascertained the existence, the physiologist has not yet derived any great benefit from the instrument. Except the simple fact of the existence of the globules of the blood and of some of the animal fluids which are derived from it, and of the spermatic animalcules, I am not aware that we owe it any further obligations, and notwithstanding all the boasted improvements of modern times, I do not very clearly perceive that we have yet advanced much beyond Leeuwenhoek and Hooke, in our power of discriminating minute objects.

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[1] Phil. Mag. and Ann. Phil. v. ii. p. 130 et seq.

There has been as much difference of opinion respecting the size, as the form of these particles. Without recurring to the older and less correct observations, it may be sufficient to give those of Dr. Young, Captain Kater, Mr. Bauer, and Dr. Hodgkin; Dr. Young[1] and Captain Kater[2] both agree that the particles of the human blood are between $\frac{1}{4000}$ and $\frac{1}{6000}$ of an inch in diameter, or taking the medium, $\frac{1}{5000}$ of an inch, while Dr. Hodgkin estimates them at $\frac{1}{3000}$; Mr. Bauer supposes them to be considerably larger; in their entire state he estimates them at $\frac{1}{1700}$ of an inch, and even when they have lost their external part, the nucleus is said to be $\frac{1}{2000}$ of an inch in diameter[3]; it is not stated whether the observations of Dr. Young and Captain Kater were made upon the entire globule, or upon the central part only. These estimates unfortunately differ so much from each other as to throw a considerable doubt upon their correctness; but upon the whole, I feel disposed to assent to the statements of Dr. Young and Captain Kater, from their coincidence with each other, although made with a different kind of apparatus[4].

The composition and chemical properties of these bodies still remain the subject of controversy, for although they have engaged the attention of some of the most acute of the modern chemists, the results which they have obtained are so discordant, that we cannot deduce any consistent or decided conclusion from them. This, in some measure, depends upon the difficulty which there is in procuring them in a separate state; there appears to be no method of detaching them from the serum in which they are enveloped, without at the same time affecting the constitution and properties of the globules themselves. The experiments of Berzelius are the most elaborate, and probably those on which we ought to place the most confidence; but I think it may be objected to them, that according to the method which be adopted, the globules would be necessarily mixed with some of the other ingredients of the blood. His result is, that these bodies do not materially differ from the other parts of the blood, except in their colour, and in the circumstance of a quantity of the red oxide of iron being found among their ashes after combustion[1].

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[1] Med. Liter. p. 555.

[2] Phil. Trans. for 1818, p. 187.

[3] Phil. Trans. for 1818, p. 173. It appears, therefore, that of our two most correct observers, we have the one estimating them at above three times the size of the other. See Phil. Mag. and Ann. Phil. v. ii. p. 133.

[4] Cavallo states that the particles varied in size, the larger being .0003 and the smaller .0004 of an inch in diameter; p. 249. The estimate of Dr. M. Edwards agrees with that of Dr. Hodgkin, and this is the case also with that of Prevost and Dumas. On this, as on so many other subjects that have passed under our review, the opinion of Raspail differs from that of his predecessors and contemporaries; § 904 et seq.

The discovery of iron in the blood appears to have been originally made by Menghini[2], and has been fully confirmed by later observers, although they have differed very much both concerning the amount of the iron and the state in which it exists. Menghini himself, and some of the earlier chemists, seem to have very much over-rated it; Fourcroy's account does not enable us to ascertain the quantity of it in the blood with any degree of accuracy[3], but we are informed by Berzelius, that the colouring matter of the blood, separated from the other part, leaves oneeightieth of an incombustible residuum, of which rather more than one-half is an oxide of iron [1].

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[1] Med. Chir. Trans. v. iii. p. 212 et seq. Raspail seems to think that the globules of the blood are essentially albuminous; they owe their colour to a thin film which is also albuminous; he admits the presence of iron and its effect in producing colour, but there is some doubt about the state in which it exists: § 913 et seq. and 930 et seq.

[2] Menghini's Memoir; Bonon. Comment. t. ii. pars 2, p. 244, et seq.; gives an account of a prodigious number of experiments which he performed on the blood of various animals, as well as other animal substances. Most of the experiments are related only in a summary manner, but some of them are detailed with more minuteness. In the first series he used five ounces (2400 grains) of the blood of a dog, and by calcination he obtained 24 grains of residuum, to which he applied a magnet, and found very nearly the whole to be attracted by it. The calx consisted of two kinds of particles; the one, "splendidiores," which were the most easily attracted; the other, "colore ad crocum martis vergentes," also magnetic, but less so than the former; p. 245, 246. As the Bolonese Commentaries may not be accessible to every one, I shall transcribe an experiment that was made upon human blood. "Humani enim sanguinis globulos unius libræ vehementissimo igne dimidiam horam vexavimus; vexatos ebullire primum aliquantisper, tum ex improviso flammulam emittere conspeximus. Erat hæc cærulea ad instar earum rerum sulphurearum, quæ solitæ sunt adhiberi ad transmutandum ferrum in chalybem. Metus fuit ne emissa flammula, cum materiam omnem absumeret, curiositatem quoque nostram eluderet. Quapropter subsidentem in vase materiem illico supra porphyritem Lellius effudit. Hæc granorum 28 ponderis inventa est. Hanc postea cum supra eburneum planum extendissem, in grandiuscula quædam corpora, inter quæ unum eminuit figura subrotundum, magnitudine ceteris præcellens, granum parvuli ciceris adæquans, offendi. Periclitari hinc, volui an ad cultrum magneticum omnia accederent; accesserunt autem super ea velocitate, qua solet ferrum purissimum. Porro corpusculum illud, quo ceteris magnitudine præstabat, discissum et fractum intus cavum, nitentibus lineis distinctum, figura, duritie, et colore ubicunque simillimum fusco ferro, mediocris lentis auxilio cognovimus." p 260, 261. On this subject it will only be necessary to remark that Menghini's paper affords one instance out of many others, written at that period, where we may be assured that the facts as stated cannot be true, yet where it is not easy to assign the source of fallacy, or to determine in what degree the experimentalist was himself deceived or wished to deceive others. Plenk; Hydrol. p. 38; says that Rhades discovered the iron in the blood. In 25 pounds of blood, the average quantity in a man, he found two drachms of oxide of iron. Vauquelin says that Lemery discovered it; Ann. Chim. et. Phys. t. i. p. 9.

[3] System, t. ix. p. 207.

We have hitherto been unable to ascertain in what state this iron exists; it would appear not to be in the form of any of the known salts of this metal, because, before the blood has been calcined, we cannot detect the iron by the tests which usually indicate its presence, yet its solubility in the serum, on the other hand, would seem to favour the opinion of its possessing something analogous to the saline state. Berzelius has performed many experiments on this point, but his conclusion is merely a negative one, that he has been unable to combine the serum with any salt of iron, so as to produce a compound similar to the colouring matter of the blood [2].

The existence of iron in the globules of the blood is, however, clearly proved; whatever difficulty there may be in determining the state in which it exists, and from the property which iron possesses of colouring the substances with which it is united, it has generally been supposed that the iron gives the blood its red colour. We are not, I apprehend, in possession of any facts by which this opinion can be either decisively proved or disproved, but I think it may be admitted as a probable presumption. It has indeed been opposed by some writers of high authority; of these one of the first, both in point of time and of respectability, is Wells [3]. But his experiments seem to me only to prove that the colour of the blood is not occasioned by any salt of iron, or of iron in such a state as to be affected by the ordinary tests, which is admitted to be the case. Mr. Brande has also attempted to prove that the colour of the blood does not depend upon iron [4], because he found the indications of the presence of iron to be as considerable in the parts of the blood that are without colour as in the globules themselves; and indeed his results would rather tend to prove that the quantity of iron in the blood is too minute to produce any effect in it, than to explain its action on the different component parts of this fluid. But with respect to the quantity of iron, I may remark that they are at variance with the later and apparently more elaborate experiments of Berzelius.

A series of experiments have been more lately performed on the colouring matter of the blood or the hematosine, as it has been termed, by Vauquelin, which he regards as confirming the conclusion of Mr. Brande. They consisted in digesting the crassamentum in diluted sulphuric acid, which dissolves a portion of the albumen and fibrin, together with the colouring matter, which last is thrown down separately from the solution by ammonia. The colouring matter subsides from the fluid, and, by sufficient washing, may be obtained, as it is supposed, in a pure state; if it be then diffused through water it produces a fluid of a purplish colour, in which the presence of iron is not indicated by the usual tests, while they readily detect it in the fluid from which the colouring matter has subsided[1]. But I apprehend that this cannot be considered as deciding the point; for we may observe that after the crassamentum has been subjected to the action of these re-agents, the constitution and chemical properties of its component parts will be considerably altered, so as not to afford us any certain indication of their previous state. Besides, although the precipitate that is formed upon the addition of the ammonia be a substance of a purple colour, we do not seem to have any evidence that it is composed of the red particles as they naturally exist in the blood; and we may further remark that the experiments of Vauquelin differ essentially from those of Mr. Brande, inasmuch as Mr. Brande's results tend to prove the almost total absence of iron in the blood.

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[1] Med. Chir. Trans. v. iii. p. 215.

[2] Med. Chir, Trans. v. iii. p. 221. Fourcroy and Vauquelin announced as the result of direct experiment, that the iron of the blood was in the state of sub-phosphate, but Vauquelin has since retracted this opinion; Fourcroy's System, v. ix. p. 207, 208.

[3] Phil. Trans. for 1797, p. 416 et seq.

[4] *Phil. Trans. for* 1812, p. 90 et seq.

One of the most remarkable properties of the red globules is the change which is effected in their colour by the action of the different gases. Lower noticed the greater brightness of the upper and external part of the clot, when exposed to the air, and attributed it to its proper cause[2], but the mathematical doctrines, which were so prevalent about this period, led to a different explanation of the fact. Cigna of Turin, about 60 years ago, revived the doctrine of Lower, and confirmed it by some ingenious experiments, but it is remarkable that he afterwards almost abandoned his own hypothesis[3]. The subject was then taken up by Priestley, and he not only fully established the point, that the bright red colour of the external part of the crassamentum depends upon the action of the atmosphere, but he proved that it is owing to the oxygenous part of the air alone, and that carbonic acid and azote have precisely the contrary effect, reducing bright scarlet blood to the purple colour[4]. There is reason to suppose that it is on the red particles of the crassamentum that the air more especially acts[5], and it has been conjectured, that the iron which they contain is the agent on this occasion. But although there can be no doubt of the influence of the atmosphere, in producing the change from the venous to the arterial state of the blood, the late experiments of Dr. Stevens prove, that the saline matter contained in the serum is also essential to the process, or at least to the change of colour by which it is indicated. He distinctly shows that strong solutions of nitre and of other salts produce in venous blood a colour even more florid than that produced by exposure to oxygen, while arterial blood, when deprived of its saline matter, becomes even darker than venous blood in its ordinary state. The conclusion which, I conceive, we must draw from these experiments is, that the absorption of oxygen and the change of colour, although generally co-existent, do not stand to each other in the relation of cause and effect, but that they depend upon different principles, and are, as it would seem, not necessarily connected together[1]. A series of experiments has lately been performed by Engelhart, on the colouring matter of the blood, which seem to throw some light on its nature, and the relation which it bears to the other parts of the fluid. In order to obtain it in a state of purity, he diluted the mixture of red particles and serum with fifty parts of water, and exposed it to a temperature of 150°. In this case the serum does not coagulate while the red globules do so; they separate in the form of greyish black flocculi, and may be removed by filtration. The author found that they contained iron, while the fibre and the serum, treated in the same manner, did not indicate its presence; the quantity of iron nearly agrees with Berzelius's estimate[2].

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[1] Ann. Chim. et Phys. t. i. p. 9.

[2] De Corde, c. iii. p. 178.

[3] Priestley on Air, v. iii. p. 357 et seq.

[4] Ibid. p. 363 et seq.

[5] We learn from Berzelius, that "blood in which the colouring matter is still contained, absorbs oxygen gas very quickly, when out of the body and shaken in atmospheric air;....on the other hand, serum, when destitute of colouring matter, does not change the atmospheric air before it begins to putrify;" View of Animal Chemistry, p. 36. Dr. Davy has lately performed a series of experiments, which led him to conclude, that the blood does not possess the power of absorbing air, and that the difference of colour in the upper and lower part of the clot depends on the former containing fewer of the red particles; this circumstance he supposes to constitute the essential

Besides the ordinary globules of the blood, Mr. Bauer has given us an account of another species of globules, which would appear to be essentially different from the former, both in their nature and properties, and in the relation which they bear to the other constituents of the blood. He first detected

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difference between arterial and venous blood; Ed. Med. Journ. v. xxxiv. p. 243 et seq. In the following volume of the same journal, p. 94 et seq., we have an interesting paper by Dr. Christison on the same subject; he gives an account of the various opinions that have been entertained respecting the action of the air on the blood, and the result of his own investigations; his conclusions, which appear to be fairly deduced from the facts, are different from those of Dr. Davy.

[1] We have some experiments by Dr. Turner, which, to a certain degree, confirm the doctrine of Dr. Stevens, in relation to the connexion which subsists between the change of colour in the blood and the presence of its saline contents; Chem. p. 969, and Ed. Med. Journ. v. xxxix. p. 249, 0.

[2] Jameson's Journ. Oct. 1826, p. 314 . . 7. We have a paper by Rose; Ann. Chim. et Phys. t. xxxiv. p. 268 et seq.; confirming the statements of Engelhart, and mentioning some curious circumstances respecting the influence which certain organic matters possess, of preventing the precipitation of the iron by the usual re-agents. For a further account of the colouring matter, see Turner's Chem. p. 963 . . 5. See also Ed. Med. Journ. v. xxvii. p. 96 et seq.

them in the serum, where he observed them to be actually generated while the fluid was under examination. Minute spots made their appearance, which gradually increased in bulk, until some of them attained the size of the globules of the blood when deprived of their colouring matter. We are informed that these globules are generated in serum after it has been removed from the vessels for some days; it would appear, therefore, to be independent of any property in the fluid that is connected with vitality, or its tendency to organization. Similar appearances were detected in pus, which is stated to be, in the first instance, an homogeneous fluid, and that the globules gradually make their appearance in it. It does not appear that the serum is actually composed of these globules, but that they are formed from its constituents. Sir Everard Home gave them the name of lymph globules[1].

Mr. Bauer afterwards observed these lymph globules in the coagula of an aneurysm, where they exist along with the ordinary blood globules, the lymph globules being in greater proportion in the older coagula, until it appeared that the oldest were almost entirely composed of them. The globules in these coagula were $\frac{1}{2800}$ of an inch in diameter, and were supposed to be the same that had been previously observed in the serum. The buffy coat of inflamed blood is said to consist almost entirely of these lymph globules, a circumstance which appears somewhat inconsistent with the fact previously noticed, of their being found in greater abundance in old coagula, and with a remark which is subsequently made, that in tumours, the firmer and older parts are principally composed of the lymph globules, and the more recent principally of the ordinary blood globules[2].

It is not impossible that these lymph globules may be the origin of Leeuwenhoek's descending series, and it affords us a striking illustration of the mode in which microscopical observations may be warped and accommodated to a preconceived theory, even by a person of skill, science, and integrity.

SECT. 5.—*Serum.*

After this account of the crassamentum of the blood, we now proceed to the serum, or the fluid part which is left after the separation of the clot, in consequence of the spontaneous coagulation of the fibrin. Serum is a transparent, homogeneous liquid, of a light straw colour, a saline taste, and an adhesive consistence. Its specific gravity varies in different subjects, but it is always greater than that of water; the average is probably about 1.025[3]. It converts blue vegetable colours to green, thus proving that it contains a quantity of uncombined alkali, and besides this it is found to hold in solution various earthy and neutral salts. Its most remarkable and characteristic property is its coagulation by heat: we find that when the serum of the blood is exposed to a temperature of 160°[1], it becomes white and opake, and acquires a firm consistence. In this state it exactly resembles the white of the egg, when hardened by boiling, and is found to be essentially the same with this substance, whence it has obtained the name of albumen. Although the whole of the serum appears to be converted into a solid mass by the process of coagulation, yet, if the albumen be cut into small pieces, and placed in the mouth of a funnel, a fluid drains from it, which is called the serosity of the blood. The separation of the serosity may be further promoted by washing the coagulated albumen in hot water, and after this process has been continued for a sufficient length of time, the albumen is left pure, united only with a quantity of water; this, by the application of a moderate heat, may be expelled, when the substance assumes the appearance of a semitransparent solid, both in its physical and chemical properties similar to the harder varieties of membrane, except that it does not exhibit any appearance of an organized structure.

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[1] Phil. Trans. for 1819, p. 2 et seq.

[2] Phil. Trans. for 1820, p. 2 et seq.

[3] Marcet, in Med. Chir. Trans. v. iii. p. 363.

The coagulation of albumen is an operation which must be regarded as essentially different from the spontaneous coagulation of the fibrin, although they are both designated by the same appellation, inasmuch as they are produced by totally different means; the one by mere rest, which has no effect upon the other, while, on the contrary, heat, which does not produce the concretion of the fibrin, immediately converts albumen into the solid form. The texture also of the coagulum is different in the two cases; the fibrin has a tendency to arrange its particles in a specific manner, so as to present the appearance of an organized body, but nothing of this kind takes place with respect to the albumen; it simply concretes into a mass of an uniform consistence, in which the albumen remains united to the water that previously held it in solution.

Besides heat, there are other agents, which are said to coagulate albumen; alcohol, acids, metallic salts, and the tannic acid, and, according to the curious discovery of Mr. Brande, it may be also effected by the negative wire in the interrupted galvanic circle[2]. But I apprehend, that in these cases, although the whole or a part of the albumen is converted into the solid form, the operation is very different from that of heat. Some of these agents, as alcohol, and perhaps the stronger mineral acids, produce their effect by abstracting a portion of the water which held the albumen in solution, while the tannic acid and the metallic salts unite with the albumen and form a compound which is insoluble in water, and consequently separates from the fluid, thus producing an effect, which should rather be styled precipitation than coagulation.

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[1] Dr. Thomson quotes Cullen as stating that the coagulation of albumen takes place at 165°, but without any particular reference; and this is the degree assumed by Marcet; Med. Chir. Tr. v. ii. p. 378. Cullen, in his "Institutions," p. 206, fixes the degree at 156. Fourcroy, in his "System, v. ix. p. 190, fixes it at 75 C., i. e. 167 F.; while in Ann. Chim. t. vii. p. 156, he names 55 R., i. e. 155·75 F.; 160 is the degree assigned by Henry, Children, Brande, Ure, and Turner. Prevost and Dumas, in some recent experiments, state the degree to be "autour de 70 Cent." i. e. 158 F.; Ann. Chim. et Phys. t. xxiii. p. 53.

[2] *Phil. Trans.* for 1809, p. 373 et seq.

Albumen which has been coagulated by heat, to which I shall at present confine my remarks, differs in many respects from albumen before it has undergone this change. Besides its conversion from the fluid to the solid form, many of its physical properties are altered, and it is differently acted upon by chemical re-agents, especially by water, in which it is no longer soluble. The efficient cause of the coagulation of albumen is a question that has been frequently discussed, but hitherto, I conceive, without much success. We are informed that the specific gravity of albumen is not affected by the process, so that we may conclude it does not depend upon condensation; nothing is added to it, and nothing is abstracted from it; for although Scheele endeavoured to prove that there was a fixation of the matter of heat, and Fourcroy, in his vague way, attributes it to the absorption of oxygen, we do not find that there is any foundation for this opinion. We learn, indeed, from the experiments of Carradori, that the coagulation takes place as readily when the contact of air is prevented, as when the albumen is exposed to the atmosphere; and he likewise informs us that the substance experiences no change of bulk during the operation[1]. We can only, therefore, account for it upon the supposition of some change which the figure or nature of its particles have experienced, by which their relation to each other is altered, without their being brought nearer together, or, as far as we can perceive, being arranged in any specific manner analogous to organization. But we are unable to explain what is the nature of this new relation, or what are the means by which it is effected.

Dr. Thomson supposes that the process of coagulation is analogous to the change which takes place in a solution of silicated potash, when it is saturated with muriatic acid, where the acid gradually detaches the potash from the silex, which being thus left at liberty, unites with a portion of the water and forms a gelatinous mass[2]. But it may be objected to Dr. Thomson's hypothesis, that the case he adduces is one of an interchange of chemical elements, which in fact produces a precipitation, but which is brought about so slowly, that the precipitated body becomes united to a portion of water, instead of being thrown down separately, as it would be under ordinary circumstances. With respect to albumen, however, we have no evidence that any chemical change takes place among its constituent parts, or that any thing analogous to precipitation exists.

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[1] Ann. Chim. t. xxix. p. 98, 9.

[2] System. v. iv. p. 409.

Mr. Brande employs a method of explaining the phenomenon which is somewhat different. He regards liquid albumen as a solution of solid albumen in alkali, and upon this principle he endeavours to show how the action of the galvanic apparatus, of alcohol, and of acids, coagulate albumen by abstracting the alkali[1]. But I think I have proved by direct experiment, that the quantity of alkali in albumen is much too minute to retain it in solution, and that the alkali may be neutralized and the albumen still retain its fluid form[2]; besides I do not perceive how this explanation applies to the action of caloric. Upon the whole I am disposed to regard the coagulation of albumen by heat as an effect entirely sui generis, as one which at present we are not able to refer to any general principle.

The most important chemical properties of albumen, while in its liquid form, are its solubility in water, and the precipitates which it forms with the mineral acids, the tannic acid, and a variety of metallic salts. Of the mineral acids, the muriatic is supposed to combine with it the most readily, and is therefore employed as one of the most delicate tests of its presence in a substance where we suspect it to exist. Tannic acid forms with albumen a dense precipitate, of a tough consistence, and insoluble in water. A variety of the metallic salts precipitate albumen, and, like the acids, serve as very delicate tests of its presence; of these probably the corrosive sublimate, or the bichloride of mercury, is the most delicate, and at the same time the most discriminate, as it appears to have no action upon the other animal substances which enter into the composition of the albuminous fluids.

The chemical relations of coagulated albumen, as I have already remarked, differ materially from those of this substance before coagulation. It then becomes completely insoluble in water, unless by heating the water under strong compression it be raised to a temperature considerably above the boiling point, and then it would appear that the albumen is rather decomposed than dissolved. Coagulated albumen is partially soluble in sulphuric acid, but one of its most interesting chemical relations is that which was described by Mr. Hatchett, where he found that by digesting it for some time in diluted nitric acid, it is converted into a substance which possesses the physical and chemical properties of jelly. If the nitric acid be applied in a concentrated state, and the action be assisted by heat, the albumen is dissolved, and the fluid assumes a yellow colour, hich is changed into a deep orange by the addition of ammonia, change which may be employed as a test of the presence of lbumen in the solution[1]. The caustic mineral alkalies act eadily upon solid albumen, and form with it a saponaceous uid, which may be substituted for some of the coarser kinds of oap in various manufacturing processes[2].

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[1] Phil. Trans. for 1809, p. 373 et seq.

[2] Med. Chir. Trans. v. ii. p. 173, 174.

SECT. 6. *Serosity.*

The portion of the serum which remains fluid after the albumen as been coagulated by heat, and which may be obtained either y washing the coagulum with water, or by simply suffering e fluid part to drain from it, is called the serosity. Compared ith the other constituents of the blood, it exists only in small uantity, and is generally so connected with the albumen, that was some time before it attracted any attention; and partly n this account, and partly from its being a compound subtance, consisting of several ingredients, it was not until very tely that any correct notions were entertained respecting it. ts existence as a substance distinct from the albumen appears o have been first announced by Butt, in a thesis published at dinburgh in 1760[3]; its properties were still further developed y Cullen in his "Institutions,"[4] and it afterwards became the ubject of more minute chemical examination in France, by ourcroy and Vauquelin[5], and by Parmentier and Deyeux[6]. he most important point which the French chemists stated, as e result of their experiments, is the discovery of a quantity of elatine, or animal jelly, in the serosity, which was said to contitute the bulk of the animal matter contained in it, and to hich it owed its specific properties. The account which was iven of this substance, especially by Parmentier and Deyeux, as so much in detail, and altogether bore so much the appearnce of accuracy, that every one acquiesced in their statement, nd not only was jelly always considered by systematic chemists o be one of the constituents of the blood, but means were ointed out for ascertaining its proportion, and many important circumstances. With respect to albumen, however, we have no evidence that any chemical change takes place among its constituent parts, or that any thing analogous to precipitation exists.

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[3] De Spontanea Separatione Sanguinis, p. 53 et seq.

[4] § 247 . . 253.

[5] Ann. Chim. t. vi. p. 181; and t. vii. p. 157.

[6] Journ. de Phys. t. xliv. p. 438, 9.

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[5] Ann. Chim. t. vi. p. 181; and t. vii. p. 157.

[6] Journ. de Phys. t. xliv. p. 438, 9.

During a course of experiments on the nature of the animal fluids, in which I was engaged in the years 1805 and 1806, I examined the serosity of the blood, but was unable to detect the smallest quantity of jelly in it, or in any other of the albuminous fluids, so that I was led to conclude that jelly never exists as a constituent of the blood[2]. This conclusion has been since amply confirmed by Prof. Berzelius[3], Dr. Marcet, and Mr. Brande, so that notwithstanding the high authority and the weight of experiment by which the contrary opinion appeared to be supported, we may certainly decide that the blood contains no gelatine. At the same time, however, that I made the above experiments, I found that the serosity contains a quantity of animal matter, and that this matter is not albumen, but it was difficult to ascertain any other than negative characters for it, as it is always united with a quantity of soda, and with a variety of other salts, from which I know of no method of separating it, without its being, at the same time, decomposed. To this substance Dr. Marcet has applied the name of muco-extractive matter[4]; I have preferred styling it the uncoagulable matter of the blood, as a term expressive of its most characteristic and distinctive property. As far as I have been able to ascertain its nature, it is not coagulable by heat, or by any other means; nor is it affected by those re-agents which are the appropriate tests of albumen and of jelly.

Although, I think, there is sufficient proof of the existence of this peculiar substance in the serosity, and of its being a proximate animal principle different from any other of the constituents of the blood, yet a contrary opinion has been maintained by Mr. Brande. Mr. Brande principally resting upon the fact, that when the serosity is exposed to the action of the galvanic apparatus, a quantity of coagulum, apparently similar to albumen, is collected round the negative wire, supposes the animal matter in this part of the blood to be merely albumen, held in solution by an alkali. But I think, from the manner in which he performed his experiment, it will be found, that the serosity upon which he operated was in an impure state, and that it still contained a quantity of albumen, which was separated by the action of the apparatus. Besides, I found by direct experiment, that if the alkali in the serosity be neutralized, and even if there be an excess of acid, still there is no tendency to coagulation manifested by the animal matter which it contains, under circumstances which would immediately have produced this effect in an equally concentrated solution of albumen.

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[1] Sprengel, a writer of extensive information, and generally of great accuracy, describes the jelly of the blood in the following terms; "namque gelatina, quam continet (serum) in frigore duntaxat coit, nequaquam vero in æstu;" Inst. Med. t. i. p. 381.

[2] Med. Chir. Trans. v. i. p. 71 et seq.

[3] *Chimie*, par Jourdan et Esslinger, t. vii. p. 76.

[4] *Med. Chir.* Trans. v. ii. p. 364.

Prof. Berzelius entertains a different opinion respecting the nature of the animal matter in serosity. He says, "it is clear that Dr. Marcet's extractive matter is the impure lactate of soda."[1] It would seem that he conceives, that a part of the soda of the blood, which had been supposed by other physiologists to be in a caustic state, is in combination with the lactic acid, and that this salt is united to a portion of animal matter. I do not, however, find that he has distinctly stated what are the properties of this animal matter, which is combined with the lactate of soda, whether he considers it to be a portion of the albumen which remains attached to it, or something of a specific nature. Although, therefore, we may admit, upon his authority, the existence of the lactic acid, or of the lactate of soda in the blood, I do not think that this affords any objection to the proofs that have been adduced by Dr. Marcet and myself of the uncoagulable matter as forming one of its essential constituents.

The only remaining ingredients of the blood are the various salts that are found in it, which, although they might in one sense be regarded as extraneous substances, yet they are so constantly present, and so nearly in the same proportion under all circumstances, that we must regard them as an essential part of it. They appear to have been first distinctly noticed by Guglielmini[2]: they were examined with considerable skill and accuracy by Rouelle[3], and since his time have been frequently made the subject of examination, but for the latest and most correct account of them we are indebted to Dr. Marcet and Prof. Berzelius.

Dr. Marcet, after coagulating the albumen and washing out the serosity from it, evaporated the solution thus obtained, and incinerated the residuum, by which means he obtained the saline matter in a separate state; it was found to amount to rather more than 9 grains in 1000 grains of serum. Of these 9 grains about 6½ were muriate of soda, combined with a small quantity of muriate of potash, about 1½ of the subcarbonate of soda, with minute quantities of the sulphate of potash, and the phosphates of lime, iron, and magnesia[4]. There is reason to suppose that the soda which Dr. Marcet obtained in the state of a subcarbonate exists in the blood in the caustic state. Prof. Berzelius's analysis of the salts of the serum agrees with Dr.

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[1] Med. Chir. Trans. v. iii. p. 231; Progress of Animal Chem. p. 16. Raspail endeavours to prove that the lactic acid, which has been supposed to exist in meat and other animal fluids, is acetic acid in combination with albumen; sect. 862 et seq.

[2] Opera, l. ii. de Sanguinis Natura, sect. 52.

[3] Journ. de Médecine, t. xlvi. p. 65 et seq. (1776.)

[4] Med. Chir. Trans. v. ii. p. 370.

Marcet's very nearly, in the quantity of the muriates, which he estimates at 6 grains from 1000 grains of the serum; the absolute amount of the soda we cannot ascertain, as he only gives it as existing in the form of an impure lactate, and with respect to the other salts, the sulphate of potash and the earthy phosphates, he supposes that they did not previously exist in the blood, but were generated during the process of combustion[1]. The difference of opinion which thus appears between two chemists of so much eminence renders it very desirable that the experiments should be carefully repeated, that the existence of the lactic acid may be confirmed, and a further examination made of the animal matter which forms the basis of the serosity[2].

Since we find that a certain quantity of saline matter is constantly present in the blood, as well as in all the other albuminous fluids, we are naturally led to conclude, that these salts perform some useful purpose in the animal œconomy, yet we are at a loss to say what this purpose can be. It has been conjectured that they may stimulate the nerves of the heart, and thus contribute to the contraction of its muscular fibres[3], that they may aid in the operation of the secreting organs, or that they may contribute to the process of digestion; but these suppositions are all gratuitous.

Sulphur has been enumerated among the constituents of the blood, but its existence would appear to be rather problematical. The only direct proofs that have been brought of its presence is the effect of serum in tarnishing silver, when heated in contact with it, and the fact mentioned by Vogel, that when serum is beginning to decompose, a gas exhales from it, which has the property of blackening the acetate of lead[1].

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[1] Med. Chir. Trans. v. iii. p. 231. See also Dr. Prout on the salts in albumen ovi, in Phil. Trans. for 1822, p. 385.

[2] Sir Everard Home, in dissecting an aneurysmal tumour, found a mass of crystals, which were analyzed by Mr. Faraday, and are stated to have been "sulphate of lime, with muriate and phosphate of soda," which, it is added, are "salts usually met with in the blood;" Phil. Trans. for 1820, p. 3. We have lately had an elaborate series of experiments on the analysis of the blood by Lecanu; in addition to the ingredients generally supposed to exist in it, he finds it to contain a crystallizable fatty matter, and an oily matter; the saline contents are said to be soda combined with the albumen, the chlorides of sodium and potassium, the carbonates, phosphates, and sulphates of potash and soda, the carbonates of lime and magnesia, the phosphates of lime, magnesia and iron, and the peroxide of iron; Journ. Pharm. t. xvii. p. 485 et seq., and 544 et seq.; Ann. Chim. et Phys. t. xlviii. p. 308. I may remark, that the existence of an oily matter, as one of the constituents of healthy blood, had been distinctly announced by Dr. B. Babington, in the paper to which I have already referred. Ether was the medium employed for separating the oil from the other parts of the blood; see Med. Chir. Trans. v. xvi. p. 293 et seq. It appears that Chevreul had procured from the blood a fatty matter like brain; Ann. Mus. t. x. p. 443, and that Nourin had found a fatty matter and a substance resembling osmazome in the blood of fish; Ann. Sc. Nat. t. xx. p. 56,7. In addition to the above constituents, Wurzer is said to have detected manganese in the blood; Instit. Journ. No. 2, p. 399; and Sargeau copper Journ. Pharm. t. xvi. p. 505.

[3] Whytt's Works, p. 26.

Although the animal substances which enter into the composition of the blood possess properties, both chemical and physical, which are sufficiently characteristic to distinguish them from each other, yet they may all be resolved into the same ultimate elements, carbon, oxygen, hydrogen, and azote. Gay-Lussac and Thenard have endeavoured to ascertain the respective proportion in which these elements exist in albumen, in fibrin, and in jelly, but the results can scarcely be regarded as more than approximations to the truth. They are as follows:

	Albumen.	Fibrin.	Jelly.
Carbon	52·883	53·36	47·881
Oxygen	23·872	19·685	27·207
Hydrogen	7·54	7·021	7·914
Azote	15·705	19·934	16·998[2]

SECT. 7. *Different States of the Blood.*

We have a number of experiments, although perhaps not altogether very satisfactory, upon the relative composition of the blood in the different periods of life, and in different morbid conditions of the body. From these we may be, perhaps, authorized to draw the inference, that the proportion of azote increases as age advances, and as coinciding with this opinion, that there is more fibrin in the blood of the adult than in that of the infant. Fourcroy informs us that he found the blood of the fœtus to contain no fibrin, but in its stead a gelatinous substance, which was not reddened by the contact of the air, and also that there were no phosphoric salts in it[3]. We may probably admit the general fact, but the imperfect state of animal chemistry when these experiments were performed will not allow us to place implicit confidence in the statement[4].

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[1] Ann. Chim. t. lxxxvii. p. 215. Berzelius objects to the opinion that sulphur is a constituent of the blood, but admits that it is so of the albumen; View of Animal Chemistry, p. 17. This statement appears so singular, that I suspect there must be some inaccuracy in the translation.

[2] Thenard, Chim. t. iii. p. 523, 528, 534. Notwithstanding the ingenuity of the process which was employed by Gay-Lussac and Thenard, and their known skill and address in conducting experiments, it appears that the method which they adopted does not admit of perfect accuracy, and we accordingly find that every subsequent attempt to discover the ultimate elements of organized substances differs more or less from those that have preceded it. For the objections to this process (which appear to be valid) the essays of Prof. Berzelius in Ann. Phil. v. iv. p. 492 et seq., and of Mr. Daniell, in Children's Thenard, p. 358 et seq., may be consulted.

[3] Ann. Chim. t. vii. p. 162.

[4] MM. Macaire and Marcet performed a series of comparative experiments on the blood of herbivorous and carnivorous animals, and found it to be similar in its general *chemical constitution*, and more particularly in the pro-

Pathologists have described the different appearances which the blood exhibits in different diseases, and the alteration which takes place in its physical properties, and the attention has been lately turned to the chemical change which the blood experiences in various morbid conditions of the system. A number of curious facts have been brought to light, but perhaps we have scarcely obtained such information as can enable us to arrive at any very important general conclusions. The state of the blood in what is termed the Asiatic cholera has been made the subject of various experiments, and it appears to be ascertained, that it contains a greater proportion of albumen and red globules than in its healthy state, while the proportion of fibrine and saline matter, and of water, is less than natural; the salts especially are said, in some cases, to be almost entirely wanting. Dr. Clanny has endeavoured to prove, that it contains an excess of carbon, but this point appears to be scarcely established. It is much less disposed to coagulate than in its healthy state, and it exhibits an unusually dark colour, which is not rendered florid by exposure to the air [1].

At the request of Dr. Bright, I examined the blood of a number of patients, who were labouring under that species of dropsy, in which the urine assumes the albuminous character, when I found, that in the serum of these patients the proportion of albumen was less than in the healthy state, while the animal matter in the serosity was considerably increased. When this animal matter was separated from the salts, with which it is naturally combined, it appeared, in many respects, to resemble urea, and, as I conceive, was similar to the substance which was procured by Prevost and Dumas from the blood, after the extirpation of the kidney, and probably to that which, according to the experiments of the Tubingen chemists, enters into the composition of many parts of the body [2]. I was not able to ascertain, to my entire satisfaction, the nature and degree of the change in the animal matter of the serosity; but there appeared no doubt that a considerable change had taken place, and that it at least approximated very nearly to the nature of urea [3].

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portion of azote. They also state that the quantity of azote is the same in arterial and in venous blood, but that the quantity of oxygen is greater in the former and of carbon in the latter; the proportions being as follows; in arterial blood, carbon 50·2 parts to oxygen 26·3, and in venous blood as 55·7 to 21·7 parts; Mém. Soc. Phys. de Genève, t. iii. p. 223 et seq.; Ann. Chim. t. li. p. 371 et seq. In this connexion I may refer to the experiments of Prof. Bèrzelius, who has pointed out some minute circumstances in which human blood differs from that of the ox, and from these is led to the conclusion, that the latter, notwithstanding the nature of its food, contains more azote than the former; Med. Chir. Trans. v. iii. p. 229.

[1] Clanny on Hyperanthraxis; Ed. Med. Journ. v. xxxviii. p. 131 et seq.

[2] A further account of this experiment will be given in a subsequent chapter.

[3] Bright's Med. Rep. v. i. p. 84. and v. ii. p. 263, 447. It does not fall *within my province* to give an account of Dr. Bright's work; but I may offer

A minute examination of the blood in diabetes was instituted by Dr. Wollaston, as it had been supposed probable, that it must contain a portion of the saccharine matter which exists in diabetic urine. No sugar, however, could be detected, nor could any specific change in the chemical constitution of the blood be established, although its appearance and physical properties were obviously affected[1].

There is, however, one change of great importance, which the blood experiences in its passage along the circulation, that from the arterial to the venous state. The most obvious character which distinguishes these two kinds of blood is their colour, which, in the large trunks of the systemic arteries, is a bright scarlet, and in those of the corresponding veins a purplish red. With respect to the other circumstances in which they differ, it is commonly stated that venous blood coagulates more slowly than arterial, and that it contains less fibrin, but that its specific gravity is greater; I conceive, however, that these points are not very accurately ascertained.

Although the relative temperature of arterial and venous blood is a matter of fact, which one should have supposed might have been easily learned by a simple experiment, yet it seems to be still undetermined, at least we have precisely opposite accounts given by those who have professed to relate the results of their own experiments. Upon the whole, the weight of authority seems to be in favour of the temperature of the arterial being greater than that of the venous blood. Dr. Davy, in his experiments, which are the latest and probably the most correct that we possess upon the subject, always found the temperature of the blood in the large arteries to be a degree or a degree and a half higher than in the corresponding veins[2]. .

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my testimony to the correctness of the engravings with which it is accompanied, as I had an opportunity of comparing them with the objects which they are intended to represent; with respect to the execution they are of unrivalled excellence.

[1] Phil. Trans. for 1811, p. 96 et seq.; see also the valuable art. "Diabetes," in the Med. Cyc. by Dr. Bardsley. I may also refer in this place to the 6th chapter of Dr. Thackrah's observations on the blood, for various experiments on "the changes produced in it by disease;" it consists of a number of detached facts, of which it would be difficult to give any connected abstract, but I may remark that the experiments are related with sufficient distinctness, and that the inferences which are drawn from them appear to be, for the most part, the fair deduction from the premises. We have an ample account of the most important experiments and observations that have been made on this subject by Dr. Hall, in the Cyc. of Med., art. "Morbid States of the Blood." See also the art. "Sang (Pathologie)" by Andral (fils), in Dict. de Méd. t. xix. p. 65 et seq., and the art. "Morbid States of the Blood," by Dr. Babington, in the Cyc. of Anat.; where we have a judicious and correct summary of the latest experiments on this subject. Dr. O'Shaughnessy's essay on the cholera contains an interesting account of a number of experiments which he performed, with much apparent minuteness, on the state of the blood in its various morbid conditions.

[2] Phil. Trans. for 1814, *p. 596, 597.* Crawford, p. 273, says that the arte-

A still more important difference is the one which Dr. Crawford laboured so much to establish, the greater capacity for heat of the arterial than of the venous blood; this he estimates to be in the proportion of eleven and a half to ten, and it was upon this fact that he founded his theory of animal heat; but as this subject will necessarily claim our attention in a subsequent part of the work, I shall defer for the present the farther consideration of these experiments, and of the value which is to be attached to them. The change of the blood from its venous to its arterial state being effected during its passage through the lungs, when it is exposed to the action of the atmosphere, is so intimately connected with the subject of respiration, that we shall be more able to estimate the nature and amount of this change when we have inquired into the nature of this function.

Many calculations have been formed of the total quantity of blood in the body, but as the data upon which they have proceeded are extremely uncertain, so the conclusions have been widely different, and, of course, the greatest part of them remote from the truth. Perhaps, upon the whole, the estimate which would seem the nearest approximation is that of Haller, who supposes that the blood may constitute about one-fifth of the weight of the adult body, the proportion of the fluids being greater in youth, and diminishing as age advances. A body weighing one hundred and fifty pounds would therefore contain about thirty pounds of blood, and of this it is supposed that three-fourths or more are in the veins, and one-fourth only in the arteries [1].

The great importance of the blood in the animal œconomy, as the source whence all the parts of the body derive their immediate support, and new matter is obtained to repair the waste occasioned by the exercise of our various functions, induced the earlier physiologists to regard this fluid as the prime seat and direct cause of all diseases. This doctrine is tacitly assumed by Hippocrates, and was strenuously insisted upon and greatly amplified by Galen, who laid it down as the basis of all his elaborate hypotheses, that changes in the state of the blood were the cause of every deviation from health, and

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rial blood which he employed in his experiments was 102°, the venous 99½°, and it may be inferred that this difference existed while the two fluids remained in their respective vessels. Plenk says, generally, that the temperature of the blood is about 96°, and that the arterial blood is warmer than the venous; Hydrologia, p. 32. The results of the experiments of Mr. Coleman and Sir A. Cooper were, that the venous blood had a temperature superior to that of arterial blood; but this apparent discrepancy may perhaps be reconciled by the statement subsequently made, that although, when the heart was first examined, the blood in the right ventricle was the warmer of the two, yet after remaining for some time exposed to the air, the relative temperature of the two was altered, in consequence, as was supposed, of the greater specific heat of the arterial blood; Coleman on Respiration, p. 36.

[1] *El. Phys.* v. 1. 3.

likewise that an original difference in the nature of the fluids gives rise to those different conditions of the constitution called temperaments. This doctrine, which obtained the name of the humoral pathology, was generally adopted by Galen's successors, and maintained its ground, without ever being called in question, for many centuries. Even after the chemists had subverted most of Galen's dogmas, and had produced a total revolution in medical practice, the blood was still regarded as the origin of diseases, and they were ascribed to an acid, an alkaline, a watery, a saline, a putrid, or some other imaginary condition of the fluids. The mathematical physiologists were likewise, for the most part, humoralists, and ascribed diseases to some change in the condition of the particles of the blood, connected with their weight, size, viscidity, or other qualities, which might be supposed to affect their motion along the vessels.

The first regular opposition which was made to the humoral pathology was by Baglivi, who maintained, in a very unequivocal manner, that disease more frequently originates in the solids than in the fluids, and that any change in these latter is to be regarded as an effect and not as a cause[1]. The premature death of Baglivi prevented him from extending and maturing his ideas in the way which might have been expected from the early promise of his talents, and it must be admitted that some of his prominent doctrines are fanciful and unfounded. From this time, however, the solids were more or less regarded as influencing the state of the constitution, until the humoral pathology received its most formidable attack from Cullen. Since his time the doctrine of solidism may be considered as forming the prevailing tenets of the medical schools, and is the one to which we usually have recourse for our explanation of the phenomena of the animal œconomy, which are supposed to be connected with its morbid changes. Upon the whole, I conceive, there can be little doubt, that both our original temperaments and our subsequent diseases are more affected by the condition and properties of the muscles and the nerves, than by any physical or chemical differences in the nature of the blood; yet, I apprehend, that the solidists have gone too far in asserting that there is no original difference in the state of the fluids[2]. When we consider the various sources from which the chyle is derived, we might rather suppose that it would vary in its properties in different cases, than that it should possess a constant uniformity, nor are we in possession of any experiments sufficiently decisive to authorize us in asserting that this absolute uniformity exists. This question will, however, be discussed more fully when we come to the functions of digestion and assimilation.

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[1] The doctrines of Baglivi will be found in his treatise "De Fibra Motrice," a work which exhibits a powerful and original genius, but still struggling with the shackles of authority.

[2] We have some good observations on this subject by Coutanceau, Dict. de Méd. t. xix. p. 339 et seq.; and by Blandin, notes to Bichat, t. ii. p. 387 . . 9.

SECT. 8. *Successive Discoveries respecting the Blood.*

From the great number of individuals who have treated of the blood, pathologists, physiologists, and chemists, and from the erroneous opinions which were so long entertained respecting it, it necessarily follows that very different accounts have been given of the nature of its component parts, and that the same substance has frequently received different denominations. It will therefore be desirable to give a brief sketch of what may be called the natural history of the blood, to notice the successive discoveries that have been made concerning it, and to point out the differences that have existed in the opinions of some of our most distinguished writers on this subject[1]. It will not be requisite for our present purpose to trace the progress of opinion beyond the period of Galen; more especially, as there is some reason to conclude that his doctrines were fundamentally the same with those of his predecessors. He supposed that the blood essentially consisted of four parts, to which he gave the names of gore or blood properly so called, phlegm, bile, and black bile[2]. It is not very easy to determine what substances he intended to designate under these terms, nor, except as a matter of mere curiosity, is it a question of any importance, as we know that his opinions are erroneous. It is probable, however, that under the denomination of *αιμα*, which has been translated cruor or sanguis, he proposed to designate the crassamentum or clot, that the term *φλεγμα* was applied to the serum, and that of black bile to the red globules at the lower part of the clot, which, in consequence of being secluded from the air, had acquired a dark colour. With respect to the *κολη* or bile, as one of the constituents of the blood, it seems probable that its existence was assumed, rather because this substance was separated by the liver, and was therefore supposed to have been previously in the blood, than from their being any distinct part to which the name could be appropriated.

Erroneous as these opinions were, it does not appear that any material correction of them took place until the time of Harvey, who, about the middle of the seventeenth century, described the spontaneous separation of the blood into the crassamentum and the serum[1]. He is also supposed to have been the first who gave an account of the coagulation of the albumen by heat, but I conceive that his claim to the discovery is very dubious, and am more disposed to give the merit of it to Lower, who describes it in a much more correct and unequivocal manner[2]. At the same time Malpighi materially advanced our knowledge respecting the nature of the crassamentum, which he found to consist of a white fibrous substance, united to a red colouring matter; he also saw the red particles, which were afterwards so minutely examined by Leeuwenhoek, and he likewise, to a certain extent, understood the nature of the buffy coat, which forms upon the blood drawn during inflammatory fever. I have already had occasion to give an account of the fanciful speculations which Leeuwenhoek formed respecting the red globules, as well as of the discovery of iron in the blood, and the discussion respecting its state of combination, and the effect which it has in producing the red colour of the blood. Since the time of Leeuwenhoek the structure and composition of these globules have been investigated with much minuteness by various eminent physiologists, but, I conceive, that we must regard the question as still, in some measure, undecided.

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[1] I shall here beg leave to refer to an essay published in the first volume of the Med. Chir. Trans., where some points respecting the natural history of the blood are discussed more at large than is consistent with the nature of the present work.

[2] For a learned account of the opinions of the ancients and the earlier of the moderns respecting the constitution of the blood, I may refer to an essay by Martine, Ed. Med. Essays, vol. ii. p. 67.

We are indebted to Senac for some advance in our knowledge of the serum, which he distinctly remarks as being similar to the white of the egg, and, at the same time, points out the circumstances in which it differs from jelly. As far as I have been able to learn, it was he who brought into general use the terms serosity and coagulable lymph, the first being applied to the serum generally, and the latter to the crassamentum, the simple term lymph having been before given to the serum. About the middle of the last century there was very considerable confusion in the names applied to the different parts of the blood, partly from imperfect notions of the substances that were described, and partly from different writers describing the same substance under different names, or applying the same name to different substances. This was the case with the word serosity[3], which was first applied to the serum at large, but was afterwards restricted by Cullen to that part of it which is not coagulated by heat[4]. Cullen appears to have been the first who introduced the term gluten [1], as applied to what had been before called by Senac the coagulable lymph, and by Guglielmini, with more propriety, the fibre of the blood.

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[1] De Gener. Ex. 52.

[2] De Corde, p. 6.

[3] The earliest work in which I have met with the term "serosité" is in an essay of Lemery's in Mém. Acad. for 1729, on the Action of Borax on the Animal Fluids. It was applied by him to the serum; it was used in the same sense by Senac and by Rouelle; Journ. de Méd. t. xl. p. 68, (1773.) By some of the French physiologists it is applied to the different species of dropsical fluids, or any serous secretion or excretion from the blood; see an elaborate article, "Serosité," by Villermé, in Dict. des Scien. Méd. t. li. p. 155 et seq.

[4] There has been the same diversity in the use of the word cruor: the ancients appear to have understood by it the red blood generally, and it is used in this sense by Guglielmini; *de Sang.* § 44; Haller; El. Phys. v. ii. 8,

It was about this time that we first hear of the jelly or gelatine of the blood, but both the terms employed, and the properties by which it was designated, are so vague, that it is impossible to draw any correct conclusion from them as to what the writers intended to describe. The jelly of the blood was, however, in the year 1790, explicitly announced by Fourcroy and Vauquelin, and four years afterwards a very detailed account of it was given by Parmentier and Deyeux, in which the method of procuring it and its distinctive properties were laid down with a degree of minuteness that appeared to remove all doubt on the subject. I have had occasion, however, to point out the error that has prevailed on this subject, and, at the same time, to observe, that although we can have no doubt about the non-existence of jelly in the serosity, yet we have been unable to ascertain exactly what is the nature of the animal matter which enters into its composition.

The general conclusions that we may form respecting the nature of the blood are, that it is a compound fluid, consisting of several ingredients of various physical and chemical properties, dissolved, or at least suspended, in a large quantity of water. Of these the fibrin and the colouring matter are disposed to unite, to separate partially from the water, and to form the crassamentum or clot, to which the iron is also attached. The albumen, the uncoagulable matter, and the salts, remain in a state of solution in the water, and compose the serum; by heat the albumen is rendered solid, and may, in this way, be detached from the serosity, which consists of a portion of water holding in solution the uncoagulable matter and the salts. By slow evaporation part of the salts may be procured in the crystalline form, but the whole of the saline matter can only be obtained by calcining the residuum after evaporation, when the animal matter is consumed, and the neutral and earthy salts left behind, although probably in a different state of combination from what they originally possessed.

9; and Cullen; Inst. § 249; apply it to the crassamentum, while Blumenbach restricts it to the red globules; Inst. by Elliotson, p. 6.

[1] Institutions, § 4. c. ii.

CHAPTER VII.

OF RESPIRATION.

Next to the circulation of the blood, the function which is the most essential to life, at least in the higher orders of animals, is respiration. Respiration consists in the alternate reception and emission of air into and out of the lungs, at the same time that the blood is transmitted through a set of vessels so situated, as to enable the air to act upon it, and to produce that change in its nature and properties, which fits it for the support of life[1]. I shall arrange my remarks upon this subject under three heads; first, the mechanism of respiration; second, its direct effects; and third, its remote effects on the living system[1].

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[1] It is scarcely necessary to remark, that the above description applies only to the higher orders of animals, the mammalia, birds, and amphibia. In fishes, the process which is equivalent to respiration is performed by the branchiæ or gills, which are placed in a passage communicating with the fauces, and terminating on the surface of the body, through which a portion of the water received into the mouth is forcibly propelled. It is thus brought into a close approximation with the blood which circulates through their fringed extremities, where it receives its appropriate change from the air which the water retains in solution. The mode in which the respiration of fishes is effected, which was imperfectly understood by Boyle; Works, vol. i. p. 109; was correctly described by Mayow; Tract. i. c. 15. p. 259; although, like many of his discoveries, it appears to have been forgotten, when it was again pointed out by Priestley; On Air, vol. iii. p. 342, v. v. p. 136 et seq. of the 1st ser., and vol. iii. p. 382 of the 2d series. The respiration of fishes has been since examined by Carradori; Ann. de Chim. t. xxix. p. 171, 2; and by Cuvier; Leçons d'Anatomie Comp. t. iv. p. 305, 6. We have also a very interesting and elaborate set of experiments on this subject by Humboldt and Provençal; Mém. d'Arcueil, t. ii. p. 359 et seq., to which I shall have occasion to refer more particularly in a subsequent section of this chapter. We have also some very valuable experiments by Dr. Edwards, particularly on the relation which the respiration of fishes bears to that of animals that are furnished with lungs; De l'Influence des Agens, &c., par. ii. ch. 3. Dumeril has given an elaborate dissertation on the mechanism of the respiratory organs of fishes; Nicholson's Journ. v. xxviii. p. 350 et seq., translated from Mag. Encyc. Nov. 1807, p. 35; and we have a number of experiments on the same subject by Flourens; Ann. Sc. Nat. t. xx. p. 1 et seq.; he seems to have been the first to explain the mechanical operation of the branchiæ. In his Exper. sur le Syst. Nerv. § 4, he points out the remarkable development of the medulla oblongata in fish, as connected with the mechanism of their respiratory organs. Confiliachi and Rusconi, in their account of the Proteus Anguinus, inform us that in this singular animal, the respiration is intermediate between that of fish and of reptiles; Journ. Phys. t. lxxxix. p. 278. In many of the invertebrated animals the respiratory organs consist merely of a number of tubes or pores, called tracheæ, provided with open mouths, which simply admit the air to enter into them, while there are numerous tribes in which no distinct apparatus can be detected. Dr. Grant conceives that the cilia of the Beroe Pileus exercise a function analogous to respiration; Zool. Trans. v. i.

SECT. 1. *Mechanism of Respiration.*

The principal organs of respiration in man are the trachea with its ramifications, the pulmonary system of blood-vessels,

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p. 10, 1; see also Henderson's Trans. of Raspail, p. 290. It appears not improbable, that the currents which were discovered by Dr. Grant to issue from the apertures of the sponge may be subservient to the same purpose; Jameson's Phil. Journ. v. xiii. p. 95 et seq., and Roget's Bridgewater Treat. v. i. p. 151..3; and we may perhaps refer to this function the singular alternating circulation, which Mr. Lister observed in the Ascidiæ; Phil. Trans. for 1834, p. 381 et seq. See further on this subject the remarks of Dr. M. Edwards, on the apparatus of the Annelida for aerating the blood; Cyc. of Anat. v. i. p. 170, 1. I may here refer to the observations that have been lately made by Purkinje and Valentin, on what they term the ciliary motions of certain parts of the respiratory organs in the Batracians and other classes of animals; Ann. Sc. Nat. t. iii. 2d ser. p. 347 et seq. The observations have been confirmed and extended by Dr. Sharpey, and made the subject of a distinct treatise; see also Ed. Med. Journ. v. xxxiv. p. 113 et seq. It appears, however, that in all cases the animal produces the same kind of change upon the air. Scheele noticed the effect of a leech in abstracting the oxygen from water; On Air and Fire, p. 167. Vauquelin found that insects and snails consume oxygen and generate carbonic acid; Ann. de Chim. t. xii. p. 273 et seq. Spallanzani repeated and diversified Vauquelin's experiments, and obtained the same results with respect to the oxygen and carbonic acid, but he conceived that they also consumed nitrogen; Mém. sur la Respir. p. 184 et alibi. He also details a variety of experiments, in which animals that possessed no distinct organs of respiration deoxidated the air in the same manner with those that have lungs; Mém. p. 258, 301 et alibi. We may infer that in all these cases the same kind of change is effected on the blood or other analogous fluid, for it is this change which is to be regarded as the ultimate object and essence of the function; Magendie, Phys. t. ii. p. 261. For a judicious summary of the various experiments that have been performed on the lower classes of animals, the reader is referred to the third chapter of Dr. Ellis's "Inquiry," and the additions to c. 3, in the "Further Inquiries." See also Roget's Bridgewater Treatise, v. ii. chap. xi. § 2. We have a series of interesting observations by Rathké on the development of the organs of respiration in the mammalia and in birds, from which it appears, that in the early stages of the fœtus, the structure of the lungs is analogous to that of the gills of fish, thus illustrating the general principle, that the higher orders of animals, in the course of their development, pass through the successive degrees of organization of the lower classes; Ed. Med. Journ. v. xxxiii. p. 280. In Dr. A. Thomson's essay on the development of the vascular system of the fœtus, we have a number of interesting observations on the respiratory organs of different classes of animals, of their gradual development, of the changes which they experience, and of their relation to each other; Jameson's Journ. No. 19. p. 93 et seq. G. St. Hilaire, in the first volume of his Philos. Anat., applies his general principle of identity to the organs of respiration, and endeavours to trace the analogy of their separate parts through the various classes of animals.

[1] For an ample and judicious account of the function of respiration in all its parts, I may refer my readers to the 12th chapter of Dr. Elliotson's Physiology. We have also a well digested account of the principal facts and opinions on this subject by Coutanceau, Dict de Méd., art. "Respiration."

the lungs, and the diaphragm. The first constitutes the passage by which the air is conveyed into its appropriate receptacles; the sanguiferous vessels are the apparatus by which the blood is carried through the lungs in such a manner, as to enable it to receive the influence of the air; these two sets of parts, with the connecting membranous matter, compose the lungs, while the diaphragm is the principal agent in the alternate enlargement and contraction of the cavity of the thorax. The trachea is a tube composed of cartilaginous rings, united together by elastic ligaments, and furnished with muscular fibres, which commences in the fauces and descends into the thorax. It first divides into two branches, which pass respectively into the two lungs; here it is subdivided into smaller and smaller branches, until it finally terminates in the air-cells or vesicles. As the tubes become smaller, they gradually lose their cartilaginous nature, and are at length entirely composed of membrane. The muscular fibres of the trachea are placed both longitudinally [1] and transversely, and as the rings are incomplete at their back part, the tube easily admits of contraction in both its dimensions. A number of air vesicles, connected together by cellular texture, form what are styled lobules; a number of these lobules compose lobes, and a smaller number of these lobes constitute the lungs.

The pulmonary blood-vessels may be considered as forming the most essential part of the respiratory organs, or that to which all the rest are subservient. When the blood leaves the right ventricle of the heart, it is propelled through what has been called the Rete mirabile Malpighi, from its having been first described by this anatomist: the blood is then collected in the pulmonic veins, and is brought back to the left or systemic auricle of the heart. The lungs themselves are two masses of a spongy texture, which completely fill the cavity of the thorax; this cavity is lined by the pleura, and the lungs are also enveloped by a duplicature of the same membrane. In all the different states of respiration the two parts of the pleura, that lining the chest, and that enclosing the lungs, are in contact, no actual cavity being left between them [2].

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[1] The most eminent anatomists admit of the existence of muscular fibres in both directions; Sabatier, however, informs us that he was never able to see the longitudinal fibres of this part; Anatomie, t. i. p. 261. Helvetius doubts of their existence altogether; Mém. Acad. Scien. pour 1718, p. 23, 4.

[2] It was a prevalent opinion among the ancients that there was a quantity of air in the cavity of the thorax between the pleuræ, and the opinion has been sanctioned by the authority of Harvey, de Gener. Ex. 3. p. 6; Hamberger, Disp. de Respir. Mech., as referred to by Haller, El. Phys. viii. 1. 13; Hoadley, Lect. on Respiration, No. i. p. 11 et seq.; Hales, Stat. Essays, v. ii. p. 81; Morgagni, Advers. Anat. par. 5. § 46; and other eminent names among the moderns. See Boerhaave, Prælect. t. v. pars 1. notæ ad § 606; Haller, El. Phys. viii. 2. 3 . . 8; and Dumas, Physiol. t. iii. p. 40 et seq. But the experiments of the latter physiologists, and especially of Haller, Op.

The diaphragm is a strong muscular expansion, possessed of a great degree of contractility[1], which separates the two principal cavities of the trunk of the body, the thorax and the abdomen. In its natural state it assumes an arched form, convex with respect to the thorax; but when it contracts, the curvature is necessarily diminished, and the thorax is of course increased in its capacity. The parietes of the thorax are composed partly of bone and partly of cartilage; the ribs, which form its sides, are arched bones, articulated at their extremities, and with spaces between each of them, that are occupied by muscles, called, from their situation, intercostals. When the ribs are in their natural position, and the muscles are relaxed, their lower edge forms an acute angle with the spine; but when they are raised by the contraction of the intercostals, they are more nearly at right angles to this bone, and thus contribute to enlarge the capacity of the thorax, although this effect is principally brought about by the contraction of the diaphragm[2].

The mechanical act of respiration consists essentially in increasing the cavity of the thorax, which is accomplished principally by flattening the arch of the diaphragm; for although the contraction of the intercostals, by raising the ribs, tends to increase the distance from the sternum to the spine, yet the additional space gained in this way is but inconsiderable, when compared to that produced by the contraction of the diaphragm. Indeed it would appear, that the chief use of the intercostals is to fix the ribs, and thus to afford a kind of resistance to the power which the diaphragm would otherwise exert in drawing them down, and thus partially counteracting its own contraction.

Min. t. i. p. 301..319, appear to have decided this question in the negative. This writer, with his usual candour, states very fully the facts which have been adduced in favour of the contrary opinion; and at the same time offers many important considerations, which may enable us to detect, as well as to obviate, the sources of error to which experiments on this subject are always liable. See also Whytt on Vital Motions, p. 81. note; Bichat, Anat. Descript. t. iv. p. 6; and the same author in his treatise, Sur la Vie, &c. Art. 6. § 1. p. 146, 7; Sœmmering, de Corp. Hum. Fab. t. vi. § 12. 16; Adelon, Physiol. t. iii. p. 151; and Dumas, Phys. par. 3. sect. 2. c 2. t. iii. p. 40..5. Although this point seemed to be so completely established, some experiments have been lately performed by Dr. Williams, a zealous and intelligent physiologist, which were attended with a different result; Ann. of Philos. v. v. N. S. p. 429; and Ed. Med. Journ. v. xix. p. 347 et seq. Notwithstanding the respectability of Dr. Williams himself and of the gentleman who witnessed the experiments, I cannot but suspect that some circumstance escaped their observation, which has led to an erroneous conclusion. Experiments not unlike Dr. Williams's were many years ago performed by Houston, Phil. Trans. for 1736, p. 230; see Hoadley's remarks upon them, Lect. on Respir. Appendix.

[1] Haller in his experiments on the comparative contractility of various parts, remarks, "..... l'irritabilité du diaphragme qui paroit superieure à celle des autres muscles;" Sur les Part. Irrit. et Sens. t. i. p. 257, et ex. 210, 225, 230, 239, 240.

[2] In Sir C. Bell's Dissect. pl. 6, 7, we have an excellent view of the aspect and situation of the thoracic viscera.

As the lungs are every where in contact with the cavity containing them, their expansion must be always equal to that of the chest. The air which they contain, in consequence of this expansion, becomes rarefied; and as there is a free communication with the atmosphere through the trachea, a portion of air will enter the lungs, sufficient to restore the equilibrium. After some time the muscular contraction of the diaphragm and the intercostals ceases, and is succeeded by relaxation; the elasticity of the cartilages and membranes brings back the parts to their former shape, in which they are occasionally aided by the muscles of the abdomen and the loins [1], and as the capacity of the lungs is thus diminished, a quantity of air is expelled from them. In a short time, however, the contraction is renewed, and is again succeeded by relaxation, and this alternation proceeds as long as life continues. From this account of the mechanical process of respiration we learn, that what may be called the quiescent state of the respiratory organs is expiration; that the air enters the lungs in consequence of the increased capacity of the chest, as affected by muscular contraction; that expiration is, in a great measure, a passive operation, and therefore that the act of inspiration is the one immediately connected with the powers of life, the remaining part of the mechanism of respiration depending principally upon the elasticity and other physical properties of the organs concerned [2].

I have now described what takes place in an ordinary act of respiration; but although the function cannot be altogether suspended by any voluntary effort, it is so far under the control of the will that, according to circumstances, it may be exercised in very different degrees. When we wish to make a full inspiration, besides the diaphragm and intercostals, we call into action the external muscles of the breast, shoulders, and other neighbouring parts, which, by elevating the ribs and the sternum, still farther increase the capacity of the thorax [1]. When, on the contrary, we wish to produce a full expiration, the abdominal muscles are contracted, the viscera are thus pushed against the diaphragm, and its convexity towards the thorax is increased.

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[1] Sabatier, Anat. t. ii. p. 274. and Cuvier, Leçons, t. iv. p. 357. are, I believe, the only modern anatomists of eminence, who attribute expiration principally to the contraction of the abdominal muscles, and suppose the elasticity of the parts connected with the chest to be a secondary agent in this operation. Cuvier expresses himself very decidedly on the subject; expiration "est due principalement aux muscles du bas-ventre, qui sont, à cet égard, les vrai antagonistes du diaphragme." Except in forced expiration I conceive these muscles to be nearly passive. We have some observations by Bourdon, on certain points connected with the mechanism of the respiratory organs, which appear to be deserving of attention; they principally regard the state of the chest and its appendages during violent efforts of various kinds. These he conceives depend upon, or are always accompanied by, the closing of the glottis, which is essential to the action of the other parts of the operation: See an account of the work in Med. et Phys. Journ. v. xliv. p. 33 et seq.

[2] Cullen's account of the mechanism of respiration in his "Institutions," sect. 3. c. iv. affords an excellent specimen of his clear and concise manner of handling a subject which, by most of his contemporaries, was but imperfectly understood.

The above account of the mechanical process of respiration will, I conceive, be found sufficient to explain all the phenomena, without having recourse to any occult agents or any gratuitous suppositions, and is the one which is now adopted by the most judicious modern physiologists. But it was not until after many premature and imperfect attempts, nor without numerous and even violent controversies, that the correct theory was established. While the physical properties of the air were little understood, it was natural that many errors should prevail respecting the action of the atmosphere on the human body, and while the abhorrence of a vacuum was assigned as the cause of many of the grand operations of nature, we cannot be surprised that it should be supposed to assist in respiration. Boyle appears to have been the first who explained upon correct principles the cause why the air enters the lungs [2]; but his simple doctrine was not relished in that age of refined hypothesis, where every thing was to be explained by some abstruse mathematical problem, so that for nearly a century after he wrote, a number of learned, but unfounded speculations, continued to prevail upon the subject [3].

An opinion which was supported by high authority, and had direct experiments adduced in its behalf, even by Hales [4], was the existence of a quantity of air in the cavity of the chest, which by its elasticity compressed the lungs, and thus produced expiration. According to another opinion, at one time very prevalent, the lungs were furnished with a number of pores, through which a portion of the air passed and was again absorbed, in the different states of inspiration and expiration[1]; but later observations have decided against the existence of these passages. Most of the older anatomists spoke of the lungs as possessing some kind of innate motion, by which they alternately drew in and expelled the air; but this opinion, although various experiments were adduced in its favour, has been generally discarded, as muscular fibres have not been detected in the lungs[2], and we are acquainted with no other method in which animal motion can originate.

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[1] An elaborate account of the effect of the contraction of these muscles may be found in Boerhaave, Prælect. t. v. p. 1. § 613..7, and in Haller, El. Phys. viii. 1. 17..25. The following authors may be also consulted on the mechanism of respiration; Borelli, p. 2. prop. 81..95; Bellini, de Urin. et Puls. Introd. de Respiratione; Senac, Mém. Acad. pour 1724; Winslow, ibid. pour 1738, p. 65; also Anatomy, sect. 3. art. 13; Dumas, Physiol. par. 3. sect. 2. c. 4; Magendie, Physiol. t. ii. p. 267 et seq.; the opinion of this last author differs considerably from that of Haller, on the motion of the ribs, and the share which they have in the increase or diminution of the chest.

[2] He remarks that it had long been a subject of controversy, whether the organs of respiration acted like bellows, into which the air rushed because they are expanded, or like a bladder, which expands because the air is forced into it; he decides that the thorax acts like bellows and the lungs like a bladder. The lungs having no muscles, must necessarily be passive; the diaphragm is supposed to be the great agent in the expansion of the chest; Works, v. i. p. 102. See also Franc. de la Boe Sylvius, Opera, p. 16; Borelli, par. 2. prop. 82, 83; Mayow, Tract. p. 271 et seq.; Charleton, Œcon. Anim. Exercit. 8, § 8..11; Swammerdam, de Respiratione, sect. 1. c. ii.

[3] Baglivi, Op. p. 454; Hoadley's Lect. p. 12; Bremond, Mém. Acad. pour 1739, p. 333.

[4] *Stat.* Essays, v. ii. p. 81.

When the lungs are removed from the body, and the trachea remains open, they generally collapse and are contracted into a smaller space than they occupied while in the cavity of the thorax. This has been ascribed to the re-action occasioned by the elasticity of their cartilaginous and membranous parts[3], which, while in the body, had been retained in a state of over-distention, in consequence of the pressure of the internal air not being balanced by any air between the pleuræ. Boerhaave and Haller attribute part at least of this effect to the contraction of the muscular fibres of the trachea and bronchia[1], but it must be observed, in opposition to such great authorities, that the contractile power of the lungs remains for a considerable time after their removal from the body, and cannot therefore depend upon a cause which must cease with the vitality of the part.

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[1] Boerhaave, Prælect. t. v. par. 1. notæ ad § 606; Hales is inclined to believe in the existence of these passages; Stat. Essays, v. i. p. 235.

[2] Although the most accurate modern anatomists do not admit of the existence of muscular fibres in the lungs, some of the older physiologists conceived that they had demonstrated their existence; see Willis, Pharm. Rat. p. 9; Malpighi, in Phil. Trans. for 1671, p. 2150; Bremond, Mém. Acad. pour 1739, p. 338 et seq. It is to be presumed that these authors were misled by a false hypothesis, which caused them to believe that the lungs required a muscular structure in order to perform the function of respiration, and afterwards supposed that they were able to detect it. Darwin, as appears by certain passages in the Zoonomia, still adheres to the old opinion; v. i. p. 40; and v. ii. p. 50; but with all respect for the genius and literary merit of this writer, he possesses no authority on a question of anatomical fact. We find the same opinion also maintained by Dumas, Physiol. par. 3. s. 2. c. 3. t. iii. p. 51 et seq. I am aware likewise that Reisseisen has announced the existence of muscular fibres connected with the bronchia; Edin. Med. Journ. v. xxi. p. 450, and Elliotson's Physiol. p. 199; in Cloquet's Anat. pl. 186, and his Man. pl. 212..4, we have a transcript of Reisseisen's views of the lungs. Dr. Alison also conceives that the bronchial tubes possess a vital power of contraction; Physiol. p. 118. See article "Poumons" in Dict. Sc. Méd. t. xliv. p. 512, 527, by Monfalcon. It may be necessary to remark that Haller applies the term "caro" to designate the substance of which the lungs are composed; El. Phys. viii. 2. 26; but it does not appear that the ancients, or the older of the moderns, intended to express by this word what we technically call muscular flesh: Pliny speaks of the "caro" of plants; Nat. Hist. lib. xvi. c. 38.

[3] The diminution in the bulk of the lungs, when the thorax is laid open, has been always ascribed to their elasticity, but in their ordinary state, suspended as it were in a vacuum, and consequently having the pressure of the atmosphere removed from them, this power can only operate as an additional quantity of elasticity imparted to the parietes of the thorax. I conceive that the reasoning of Dr. Carson on this subject is not well founded. His experiments, made with a view to ascertain the amount of the elasticity of the lungs, he candidly confesses to be imperfect in the execution; nor do I think the mode which he employed for this purpose will be found competent to the end in view; Phil. Trans. for 1820, p. 29 et seq. When the lungs collapse they discharge a portion of air, this will pass into the globe which he employed, and expel a part of the water into the connected tube. But before we can obtain in this way a measure of the elastic force of the lungs, we must ascertain the proportion which their bulk bears to that of this globe, and this to the quantity of air *expelled from the lungs*. Dr. Elliotson has inadvertently at-

Few subjects in anatomy and physiology have caused more violent, and even acrimonious disputes, than the nature of the action of the intercostal muscles[2]. Between each of the ribs are two distinct layers of muscular fibres, which are situated obliquely, but in an opposite direction, so as to decussate. It was the general opinion of the ancients, that the external layer, when it contracted, would raise the ribs, and consequently encrease the capacity of the thorax, but that the internal intercostals would depress the ribs, and of course diminish the size of the chest. Mayow, who in so many respects outstript the science of his contemporaries, and whose works afterwards became so remarkably neglected, appears to have been the first who adopted the opinion, that both sets of intercostals, by their contraction, must raise the ribs, and thus encrease the size of the thorax[3]. But his experiments and reasoning were either not attended to, or failed in producing conviction, for the old opinion appears to have been generally entertained, until the middle of the last century. The doctrine of Mayow was, however, zealously embraced by Haller[4], and since his time has been, for the most part, acquiesced in[5]. But it is generally

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tributed the collapse of the lungs, when the thorax is opened, to the same cause which produces ordinary expiration; Trans. of Blumenbach, Note B. p. 82. See his remarks on Dr. Carson's hypothesis; Physiol. p. 204.

[1] Boerhaave, Prælect. § 602 et notæ.

[2] See Haller, El. Phys. viii. l. 13; Dumas, Physiol. par. 3. sect. 2. c. 4.

[3] Tract. p. 278 et seq. It is stated by Winslow, Mém. Acad. pour 1738, p. 92. and by Haller, El. Phys. viii. 1. 14, that Fabricius had previously announced the same opinion, but by referring to his treatise De Respiratione, p. 176, 7, it appears that he unequivocally supports the old doctrine.

[4] Boerhaave, Prælect. t. v. par. 1. § 613 et notæ; Haller, El. Phys. viii. 1. 12. et seq. et viii. 4. 9; an account of his experiments on the subject is contained in his Op. Min. t. i. p. 270..293. Hoadley, Lect. on Respir. p. 5..8, and Hamberger were among the most zealous defenders of the old doctrine; the latter appears to have maintained it with a degree of vehemence quite disproportioned to the importance of the object, if we may rely upon the complaints of Haller, El. Phys. viii. 1. 13. Sabatier takes a directly opposite view of the subject, and supposes that both sets of intercostals will have the effect of depressing the ribs; Mém. Acad. pour 1778, p. 347; also Anat. t. iii. p. 469.

[5] It would appear that Sœmmering still entertains some doubts respecting the action of these muscles: after describing the internal intercostals, and stating that their effect will be the same with that of the externals, viz. to raise the lower towards the upper ribs, and consequently to dilate the chest and serve for inspiration, he asks, "an costas deprimunt?" Corp.

supposed that, in ordinary respiration, the intercostals are not much employed, except for the purpose of fixing the ribs, and that it is only in cases of violent action of the respiratory organs, or where, from accident or disease, their ordinary action is impeded, that these muscles have any effect in encreasing the size of the thorax[1].

The nature of the diaphragm, its muscular action, and its importance in the mechanism of respiration, were but imperfectly understood by the ancients. By some it was supposed to possess a kind of independent life, by others it was thought to be the seat of the soul, and it was generally regarded as possessed of some mysterious or inexplicable power, until Fabricius, at the beginning of the seventeenth century, explained its action and properties upon correct principles[2]. It is now universally regarded as the great agent by which the size of the cavity of the thorax is regulated; in its natural or relaxed state, it is arched up, so as to diminish the capacity of the chest, while this is necessarily increased, when it is flattened by the contraction of its muscular part[3].

Malpighi, to whose researches we are indebted for our knowledge of so many parts of minute anatomy, appears to have been the first who described the structure of the apparatus by which the air is distributed through the lungs and is enabled to act upon the blood[4], and the description which he gave of the parts

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Hum. Fab. t. iii. p. 177. Mérat, also, the writer of the article "Intercostals," in the Dict. Sc. Méd. t. xxv. conceives, that although the effect of both sets of intercostals must be the same, and that this generally is to raise the ribs, and consequently to expand the chest, yet that, under certain circumstances, as where the false ribs are fixed by the action of the abdominal muscles, the contraction of the intercostals must depress the ribs, and thus contract the chest. The present Prof. Monro has committed a singular oversight in asserting that his "Father discovered that both strata," the external and internal intercostals, "are subservient to the elevation of the ribs;" Elements v. ii. p. 9; an oversight the more remarkable, as in a previous passage of the same work, v. i. p. 371, he had correctly attributed the discovery to Mayow.

[1] The older anatomists were aware of the existence of cases, where the cartilages of the ribs were ossified, so as to prevent the action of the intercostals, without any material impediment to respiration; see Winslow ubi supra; Fabricius de Respir. c. 10. sub finem. For farther information on this subject the reader may consult Borelli, par. 2. prop. 81..95; Bellini, lem. 11; Senac, Mém. Acad. pour 1724; Winslow, ibid. pour 1738; also Anat. Sect. 9. § 6; Boerhaave, Inst. § 615; ditto, Prælect. passim; Haller, El. Phys. lib. viii. passim; Dumas, Physiol. par. 3. § 2. c. 4; Richerand, Physiol. p. 199 et seq.

[2] De Respiratione, lib. ii. c. 8.

[3] For an accurate description of the diaphragm and its action and uses, as well as for a very copious list of all that had been published concerning it before his time, Haller's treatise in his Op. Min. t. i. p. 249, may be consulted, as also his experiments on the motion of the diaphragm in living animals; Ibid. p. 293..300, et Mém. sur les Part. Irrit. et Sens. t. i. p. 65 et seq.; for a representation of this organ see his Icon. Anat. fas. 1. tab. 1; Albinus, Tab. Musc. No. 14. fig. 4..7; Cloquet, Man. pl. 80, 81, 83.

[4] Epist. de Pulmonibus, i.

has been generally supposed to be correct. Succeeding writers; as is too frequently the case, while they have professed to adopt the ideas of their predecessors, have indulged their imagination in inventing a disposition of the parts, which cannot be found in the original account of them, and which has probably no actual existence. Willis, for example, gives a figure of a portion of the lungs, according to which they consist of a congeries of rounded vesicles, separated from each other, and every one of them provided with a distinct tube, so as to resemble a bunch of grapes[1], a structure which has probably no existence[2]. On the other hand, Helvetius endeavoured to prove that the bronchia terminate in a cellular or spongy tissue, composed of a membranous substance, the cells of which have no determinate figure or regular connexion with each other[3]. But, upon the whole, the most probable opinion seems to be one of an intermediate nature, that there are separate groups of cells, which are connected together, while these groups are themselves distinct[4].

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[1] Pharm. Rat. p. 2. tab. 3. fig. 1. Cheselden, Anatomy, p. 173, remarks upon Willis's description, that it is "imaginary and false, as he could not but have known, if he had ever made the least inquiry into the lungs of any animal."

[2] Malpighi's description of the parts is as follows: "Diligenti indagine inveni totam pulmonum molem, quæ vasis excurrentibus appenditur, esse aggregatum quid ex levissimis et tenuissimis membranis, quæ extensæ et sinuatæ pene infinitas vesiculas orbiculares, et sinuosas efformant, veluti in apum favis alveolis ab extensa cera in parietes conspicimur." "Membranæ istæ vesiculæ videntur efformari ex desinentia tracheæ, quæ extremitate, et lateribus in ampullosus sinus facessens, ab his in spatia, et vesiculas inæquales terminantur." See plate to the Epist. de Pulm.

[3] Mém. Acad. pour 1718, p. 18. Helvetius appears, however, to have been influenced in his opinion by the texture of the lungs in the amphibia, which, as they differ from the human in the relative sizes of the component parts, may do so likewise in the connexions of these with each other. An account of the characteristic difference between the lungs of the warm and the cold-blooded animals may be found in Shaw's Lectures, v. ii. p. 2, 3.

[4] Haller's general description of the lungs is contained in El. Phys. viii. 2. 9..18; his account of their minute structure, ibid. 26..30. He gives an ample detail of the controversy concerning the question, whether air that is impelled into the bronchial tubes can pass into the intervals between the lobules, and the reverse; the authorities as to the fact appear to be nearly balanced, § 26; but it may be suspected, that when the transmission does take place, it is in consequence of the rupture of a portion of the delicate cellular membrane. Haller himself inclines to the opinion, that the cells do communicate, but that there is no communication between the lobules, § 30. With respect to the figure of the cells, Hales says that they appear in the microscope to be spherical; Stat. Ess. v. i. p. 241. Monro Sec. supposed that the lungs are composed of cells, in which the bronchia terminate, but that the cells communicate with each other, and that this is likewise the case with the lobules; Elements of Anat. v. ii. p. 89 et seq. See also Sprengel, Inst. Med. lib. 4. c. 4. sect. 1. p. 450, and Boyer, Anat. t. iv. p. 268, who appear to adopt the opinion of Haller. For what may be regarded as the current opinion among the French respecting the structure of the lungs, see the 4th vol. of Bichat, Anat. Descrip. p. 69, written by Buisson, who supplied the last portion of the work, which was left imperfect by the premature death of his preceptor: also the article "Respiration," p. 13, by Chaussier and Adelon, in the Dict. Sc. Méd. t. xlviii. published in 1820; also Dumas,

But whatever may be our opinion respecting the precise form of the vesicles, or the mechanical connexion which there is between the air cells and the blood-vessels, we know that the parts are so arranged, as to enable the air and the blood to act upon each other, by the blood being divided into a great number of small portions, and thus to expose as large a surface as possible, and by being separated from the air merely by the interposition of a very delicate membrane[1]. The extent of the surface of the membrane lining the cavity of the air vesicles must necessarily be very considerable; but with respect to the estimates which have been made of it by Keill[2], Hales[3], and other physiologists of the last century, there is reason to suppose that they are, in a great measure, imaginary; nor do we appear to have any data from which we can form a more correct conclusion[4].

Physiol. p. 3. § 2. c. 2. p. 45..50. See also Adelon's Physiol. t. iii. p. 144 et seq.; I may remark, that in this work we have an account of the anatomical and physiological doctrines on the subject of respiration, stated with the usual candour and precision of this writer. Sœmmering, Corp. Hum. Fab. t. vi. § 14, supposes that the individual air cells are separate from each other, but that they all communicate by the bronchial tubes; Magendie, like Helvetius, conceives that the lungs are composed of a spongy substance, the cells of which freely communicate with each other; Physiol. t. ii. p. 262 et seq.; Journ. Physiol. t. i. p. 79; and Richerand, Physiol. p. 206, says, that most anatomists adopt the opinion of Helvetius, an assertion which appears to be scarcely warranted. Blumenbach considers the cells as being unconnected with each other; Physiol. § 139; and this appears to be the opinion of Cuvier; Tabl. Elém. p. 41, 2. For a very complete investigation of the structure of the lungs we are indebted to Reisseisen, who seems to have examined the parts with the greatest minuteness. He describes the vesicles as the closed terminations of the bronchial tubes, possessing a cylindrical and somewhat rounded figure; he states that they do not communicate with each other, or with the cellular substance in which they are enveloped; Edin. Med. Journ. v. xxi. p. 448 et seq. There is a peculiarity in the circulation of the lungs, which appears to have been first announced by Reisseisen, and is confirmed by the present Prof. Monro, Elements of Anatomy, v. ii. p. 96, that there is a direct communication between the bronchial artery and the pulmonary vein, so that the greatest part of the blood which is conveyed to the lungs by the former vessel is returned by the latter; Edin. Med. Journ. v. xxi. p. 454 et seq.

[1] Cuvier, Leç. d'Anat. Comp. t. iv. p. 298. The late experiments of Drs. Faust and Mitchell seem to have fully established the general fact of the transmission of gases through membranes, and these much denser than those which compose the vesicles of the lungs; Amer. Journ. of Med. Science, v. vii. p. 23 et seq. Nov. 1830, and Ed. Med. Journ. v. xxxvi. p. 211.

[2] Tent. Med. Phys. p. 80.

[3] Stat. Ess. v. i. p. 241.

[4] An interesting account of the comparative anatomy and mechanism of the respiratory organs in the five classes of the mammalia, birds, amphibia, fishes, and insects, is given by J. Bell, Anat. v. ii. p. 133..168. It is written in that interesting and impressive manner, which is so characteristic of his works, although, in certain points, it is not technically correct. We have a valuable article on the respiration of birds in Rees's Cyclop.; see also Hunter on the same subject in Phil. Trans. for 1774, p. 205 et seq.; he endeavours to prove that birds possess a proper diaphragm, but unless we use the word quite in a technical sense, it appears not proper to apply this term to any part of their thorax. *Cuvier's "Leçons,"* on this, as well as on every other

Many attempts have been made by physiologists to ascertain the quantity of air taken into the lungs by a single inspiration. All, however, that we can obtain on this point is the average quantity; for, as was remarked above, the action of the chest is so far under the control of volition, that we are able to receive into it at pleasure very different quantities of air. There is also a considerable difference in different individuals with respect to the size and form of the chest, and it is also probable that peculiar states of the constitution, and perhaps even particular habits, may have an effect upon the quantity of air received into the lungs. And besides the question respecting the average bulk of a single inspiration, there are three others connected with it, that are both curious and important. We may inquire, first, what is the quantity of air left in the lungs after an ordinary inspiration, which may be considered as the natural or quiescent condition of the thorax; secondly, what further quantity we are able to expel by the greatest voluntary exertion; and, lastly, what quantity is still left in the lungs after the most complete expiration.

With respect to the bulk of an ordinary inspiration, the first writer who attempted to ascertain this point by experiment appears to have been Borelli[1]; the method which he employed was afterwards improved upon by Jurin, who obtained results which would seem to be nearly correct. By breathing into a bladder, and making the necessary allowance for temperature and pressure, he estimated that he took into the lungs about 40 cubic inches[2]. Since his time many attempts have been made to solve the problem, and results have been obtained which vary from a few inches to above 50. Goodwyn bestowed much attention upon the point, but his apparatus, although more complicated than Jurin's, does not appear to have been capable of furnishing equally accurate results, at the same time that his estimate involves some physiological positions, which are at least very questionable. His apparatus consisted of a closed vessel, provided with two tubes, through one of which he inspired, while the other terminated in a second vessel containing water; when he inspired from the closed vessel, an equal bulk of water was drawn into it from the second vessel, and by weighing the first vessel before and after the experiment, the weight of water raised, and consequently the bulk of air displaced, was ascertained[3]. By taking the average of 30 inspirations, he

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subject on which he treats, cannot be too carefully studied; t. iv. leç. 26. passim, and leç. 27. sect. 2; also Blumenbach's Comparative Anatomy, c. 14. with Mr. Lawrence's valuable notes. We have a good account of the comparative anatomy of the organs of respiration in the different classes of animals by Dr. M. Edwards; art. "Respiration," in Dict. Class. d'Hist. Nat.; see also Carus's Comp. Anat. by Gore, v. ii. p. 141..197.

[1] De Motu Anim. p. 2. prop. 81.

[2] Phil. Trans. No. 355; vol. xxx. p. 757, 8; La Motte's Ab. of Phil. Trans. v. i. p. 415.

[3] *Connexion of life* with respiration, p. 28.

concluded that the bulk of air received in the ordinary action of the lungs is no more than 14 cubic inches[1].

But independently of other considerations, there are two obvious objections to this method of ascertaining the bulk of a single inspiration; first, that in breathing from the closed vessel, the water being raised from the open vessel contrary to its specific gravity, a greater effort would be necessary to receive the due quantity of air into the lungs; and although Goodwyn was aware of this circumstance, and attempted to obviate it[2], the nature of the apparatus seems to render this impossible. In the second place, as was remarked by Menzies[3], when the mouth is removed from the tube, the external air will immediately rush into the closed vessel, and drive back, into the open vessel, a part of the water which had been raised from it, so as to lead the operator to under-rate the volume of air taken into the lungs.

It will not be necessary to enter into an account of the various experiments which were subsequently performed upon this subject, because they may be regarded as, in a great measure, superseded by those of Menzies, which appear entitled to the greatest confidence, both from the nature of the apparatus, and from the uniformity of the results. He employed an allantoid, of the capacity of 2400 cubic inches, to which was fixed a tube furnished with two valves, so that the air of inspiration was kept distinct from that of expiration. He breathed into the allantoid until it was filled, and found that each expiration was somewhat more than 40 cubic inches: he confirmed the result by attaching to the other tube a second allantoid filled with air, from which he inspired, and found, in like manner, that the amount of each inspiration corresponded nearly with his previous estimate of a single expiration. He then instituted a set of experiments of a different nature: a man was immersed above the chest in warm water, the top of the vessel being furnished with a tube, by the rising or falling of the water in which any alteration was rendered visible in the bulk of the body. After using every precaution to ensure accuracy, he had the satisfaction to find that his experiments remarkably corresponded with each other, and also with those of Jurin, making the average bulk of a single inspiration, 40 cubic inches[4].

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[1] Connection of life with respiration, p. 36.

[2] Essay, p. 32, 3.

[3] On Respiration, p. 18, 9.

[4] On Respiration, p. 24..30. Although in fixing the bulk of a single inspiration at 40 cubic inches, I have been principally guided by the experiments of Menzies, yet it may be proper to state that many other eminent physiologists have given their sanction to this estimate. This is the case with Sauvages, Nosol. Meth. t. i. p. 596; Hales, Stat. Ess. v. i. p. 243; Haller, El. Phys. viii. 4. 6; Chaptal, Chem. v. i. p. 133; J. Bell, Anat. v. i. p. 193; Sprengel, Inst. Med. t. i. p. 470; Sœmmering, Corp. Hum. Fab. t. vi. § 65; Ellis, Inquiry, p. 104; and Monro (Tert.), Elements, v. ii. p. 98. Richerand also estimates it at between 30 and 40 cubic inches, Physiol. by Delys, p. 206; Fontana, *at 35, Phil.* Trans. for 1779, p. 349; and Dr. Dalton *at 30, Manch. Mem. v. ii. 2d ser. p.* 26. There are, however, some

The quantity of air emitted from the lungs being very much under the control of the will, it follows that we are still able to expel a considerable portion after an ordinary expiration. The amount of this has been variously estimated, and it probably differs much in different individuals; from the average of some trials which I have made upon myself and others, and from the statements which have been given by different authors[1], I think it may be fixed at 160 or 170 cubic inches, so as to give 200 or 210 cubic inches as the difference between the states of ordinary inspiration and of forced or extraordinary expiration. It may seem not a little remarkable, that some physiologists, who have written expressly upon the functions of the lungs, and even upon the mechanism of respiration, have entirely overlooked this quantity, and have estimated the bulk of the thorax, by adding together the amount of an ordinary expiration with what is left in the lungs after the most complete act of expiration[2].

From the formation and structure of the lungs it is, however, obvious that after the most complete and powerful act of expiration, we are still unable entirely to empty the thorax; and many experiments have been made, to ascertain what is the amount of this residual quantity, or what is the bulk of air which is still left in the lungs. The first experiments on this subject, which are worthy of any particular attention, are those of Goodwyn. He remarks that an animal, immediately before death, produces a full expiration, and therefore, by ascertaining the capacity of the thorax in the dead subject, we obtain a knowledge of the quantity under consideration. As the diaphragm is the only part of the chest which remains

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great authorities among the moderns, who have formed a different conclusion; but, for the most part, it appears to have been deduced from inconclusive reasoning, or from experiments which involve some doubtful or obviously incorrect principle. Sir H. Davy states, as the result of direct experiment, the bulk of a single inspiration to be only 13 cubic inches, Researches, p. 433; while, in another part of his work, he indirectly estimates it at 17 cubic inches, p. 410; Jurine, of Geneva, supposes it to be 20 inches, which Hallé, his editor, considers too large a quantity, Encyc. Méth. Art. Médecine, t. i. p. 494; Mr. Kite fixes the quantity at 17 cubic inches, Essays, p. 47; Mr. Abernethy at 12, Essays, p. 142; and Delametherie at even a smaller quantity, Journ. Physique, t. xlvi. p. 108. Messrs. Allen and Pepys inform us that the operator whom they employed in their experiments took in 16½ cubic inches at an easy inspiration, Phil. Trans. for 1808, p. 256. The estimates that were formed by the older writers may be found in Haller, El. Phys. viii. 4. 6. We are indebted to Dr. Herbst, of Gottingen, for a series of experiments on the capacity of the lungs in their different states; he conceives a natural inspiration to be 20 cubic inches; the average capacity of the lungs to be 200 cubic inches, and that after a forcible expiration, there is very little air left in them; Arch. Gén. de Méd. t. xxi. p. 412 et seq. I may refer my readers to the 5th chapter of Mr. Mayo's physiology, for much valuable information on the topics connected with this part of my subject.

[1] Jurin fixed this quantity at 220 cubic inches, Phil. Trans. v. xxx. p. 758; La Motte's Ab. of Phil. Trans. v. i. p. 415; Menzies, p. 31, and J. Bell. Anat. v. i. p. 193, at 70 inches; Fontana appears to make it only 40, Phil. *Trans. for 1779*, p. 355.

[2] *Goodwyn*, p. 36, 7; Kite's Essays, p. 47; Davy's Researches, p. 411.

moveable, he endeavoured to fix it by applying a firm compress about the upper part of the abdomen. An opening was then made into the thorax, and the lungs collapsing by their natural elasticity, expelled the air which they previously contained, and thus left a cavity between the pleuræ, which he filled with water. This water he conceived would exactly measure the space previously occupied by the air left in the lungs, and taking the average of four experiments, he concluded the quantity to be 109 cubic inches[1].

There are, however, many objections against Goodwyn's method of ascertaining the capacity of the lungs after a complete expiration. Although his position is in the main true, that an animal makes a complete expiration before death, it will require many restrictions before it can be adopted as the basis of a physiological calculation. The mass of blood in the pulmonary circulation, the action of the respiratory muscles, and the state of the vital powers generally, at the moment immediately preceding death, may all be supposed to produce a considerable effect upon the size of the chest. Accordingly Goodwyn himself, when he examined the state of the lungs after hanging, found the residual quantity of air to be very much greater, amounting to 260 cubic inches[2]. This difference he accounts for, upon the principle, that these individuals must have expired under the influence of fear, which always produces a deep inspiration; but it may be remarked, that in cases of natural death, the same feeling exists in a greater or less degree, and must therefore produce similar effects upon the capacity of the thorax. And farther, it would appear impossible, from the apparatus which Goodwyn employed, so to confine the chest, as that when the water was admitted between the pleuræ, the diaphragm, or the parts connected with it, might not be displaced, or caused to protrude into the abdomen. Mr. Coleman also observes, that after a complete expiration, the diaphragm is raised as high as the fourth or fifth ribs, a situation in which it would be beyond the reach of any compression which could be exercised upon it by the bandage, and that the pressure of the water within the thorax would likewise cause the ribs themselves to descend, and consequently draw down the whole substance of the diaphragm[1].

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[1] P. 25, 6. Mr. Coleman, on the other hand, who performed experiments on dogs, for the express purpose of examining the state of the lungs, after hanging, found that they contained only a very small quantity of air. He supposes, that during the violent struggles which precede death, the animal still retains the power of expelling air from the lungs, while the pressure of the cord upon the trachea prevents any from being received; On Respiration, p. 96..8. In order to reconcile this apparent contradiction, Dr. Skey ingeniously suggested, that as the effect of fear is to produce a full inspiration, in many cases of death, the human lungs would not be in the state of complete expiration, whereas this would always be the case in an animal that was unconscious of its fate. Yet this suggestion will scarcely remove the difficulty; for, in the three criminals examined by Goodwyn, the lungs would appear to have been very nearly in their ordinary state of distention, while it may be presumed, that although immediately previous to execution, the thorax, through the effect of fear, might be enlarged to its utmost capacity, yet during the act of dissolution, which is by no means momentary, the same mechanical struggle must probably take place in man, as in another animal.

[2] P. 26, 7.

But there is a still more decisive objection against Goodwyn's estimate, that it proceeds upon the principle of a complete collapse of the lungs being produced by the admission of water into the cavity of the thorax[2]. A considerable pressure would, no doubt, in this case be exercised upon the surface of the lungs, which will produce a material contraction of their bulk, but it cannot obliterate the cavities of the bronchial vesicles, an effect which must take place before all the air can be expelled. Both from the shape and texture of these organs, as well as from actual experiment, we know that scarcely any degree of external force can so far evacuate the lungs, as to render them specifically heavier than water, a state which it is difficult to produce, even by means of the air-pump[3]. From these considerations we may fairly conclude, that Goodwyn's estimate of the capacity of the lungs after a complete expiration is too low, and that we shall be in no danger of over-rating the quantity, if we suppose it to be 120 cubic inches.

The problem respecting the quantity of air left in the lungs after a complete expiration, has been also made the subject of experiment by Sir H. Davy and by Mr. Coleman. Sir H. Davy proved by a previous experiment, that when hydrogen is respired it undergoes no absorption or any chemical change, but is merely diffused through the air contained in the lungs[4]. He then inspired a quantity of this gas, after which he made a complete expiration; and having ascertained what proportion the hydrogen discharged bore to the other gases expelled at the same time, from the quantity of hydrogen still left in the lungs, he calculated what would be the total quantity of air in the thorax; and this, after making a due allowance for temperature, he estimates at no more than 41 cubic inches[5].

This estimate differs so much from the result of direct experiment, and appears to be so incompatible with what we might suppose to be the case, from considering the anatomical structure of the thorax, that it is impossible not to suspect some error or fallacy. And I conceive we may explain the difficulty by supposing, that the hydrogen was not uniformly diffused through the cavities of the lungs, a supposition which seems in itself reasonable, when we consider the minuteness and intricacy of the passages in which the air is lodged, and which is confirmed by the experiments of Jurine, of Messrs. Allen and Pepys, and of Dr. Dalton; Jurine received the air of a single inspiration in four different vessels, and found the four portions of air to exhibit different chemical properties, containing a greater proportion of oxygen and a less proportion of nitrogen, according to the order in which they were expelled from the lungs [1]; and the same difference was detected by Messrs. Allen and Pepys, and by Dr. Dalton, with respect to the proportion of carbonic acid in the different portions of the air of expiration [2]. And if this be the case with the air which exists in the lungs in the natural process of respiration, we may conclude that it is still more likely to be the case with any extraneous gas, which is forcibly received into them. Hence it seems fair to conclude that in the experiments of Sir H. Davy, the hydrogen had not been mixed with the air in the vesicles, in the same proportion as in the larger trunks of the trachea, consequently that he operated upon a gas containing an over-proportion of hydrogen, and has hence formed too low an estimate of the total contents of the lungs.

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[1] On Respiration, p. 89.

[2] Essay, p. 24.

[3] Boerhaave, Prælect. t. v. p. 2. not. ad § 681; Petit, Mém. Acad. Scien. pour 1733, p. 4; Allen and Pepys in Phil. Trans. for 1808, p. 269; and for 1809, p. 410 et alibi. Dr. Dalton, however, conceives that the air remaining in the lungs after a forced respiration "cannot be much, and that it is of little consequence to prove it exactly;" Manch. Mem. v. ii. 2d ser. p. 26.

[4] *Researches*, p. 400..9.

[5] Ibid. p. 409, 0.

The method which Mr. Coleman pursued appears simple and direct, yet the conclusion which he formed is still more extraordinary. He instituted a series of experiments for the purpose of comparing the state of the lungs after drowning, with their natural condition, and he hence deduced the diminution which the chest experiences after a complete expiration. For this purpose he applied a ligature about the trachea of an animal which had been previously drowned, and after detaching the lungs from the chest, he pressed out all the air which they contained into an inverted jar of water. He then inflated the lungs, and ascertained the quantity of air which they were capable of containing when they were fully distended. The proportion which these two quantities bore to each other differed considerably in the different experiments; but the diminution was always very much more than could have been previously expected, even as much as 43 to 1 [3]. As the lungs completely fill the cavity in which they are contained, no reduction could take place in their capacity without a corresponding change in that of the thorax; yet it appears inconceivable that the thorax can, by any process, be reduced to $\frac{1}{43}$ of its ordinary dimensions [4].

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[1] Encyc. Méthod, Art. "Médecine," t. i. p. 494.

[2] Messrs. Allen and Pepys found the first portion of the air emitted from the lungs to contain only 3, the latter part as much as 8 per cent.; Phil. Trans. for 1808, p. 257. Dr. Dalton informs us that the first proportion contained 3 per cent. of carbonic acid, and had lost 4 per cent. of its oxygen, while the last portion contained 6 per cent. and had lost nearly 8; Manch. Mem. v. ii. 2d ser. p. 25, 26.

[3] On Respiration, p. 96.

[4] See Borelli, par. 2. prop. 94. Haller, El. Phys. viii. 4. 11, conceives it

It is not easy to account satisfactorily for the results of Mr. Coleman's experiments, but some circumstances may be pointed out, which will, I think, remove a part of the difficulty. When an animal is in the act of drowning, a sense of suffocation is experienced, which produces a violent effort to expire. The muscles that are connected with the chest are therefore brought into strong contraction, and the lungs are reduced to the smallest bulk; but relaxation quickly succeeds, and the elasticity of the chest brings it back to nearly its former dimensions. The external air is, however, unable to enter the lungs, and consequently, the air still remaining in them becomes highly rarefied; and, at the same time, a portion of the aqueous secretion, which lines their cavities, will be converted into the gaseous state. After a short time a second effort is made to expire, by which a portion of the remaining air, mixed with aqueous vapour, is discharged, in consequence of which the air still left in the lungs will be farther rarefied, and a still greater proportion of aqueous vapour formed. This process will continue until the contractility of the muscles is entirely destroyed, and we may presume that, at this period, nearly all the air originally contained in the lungs will be expelled, and its place occupied by the aqueous vapour. Now, according to the method in which Mr. Coleman performed his experiments, a great portion of this vapour would be condensed, the moment that the lungs, by being removed from the thorax, were subjected to the pressure of the atmosphere, and any part of it that remained would be destroyed in passing through the water of the inverted jar. And besides the complete destruction of the aqueous vapour, a considerable portion of the permanently elastic gas contained in the lungs would be carbonic acid, a part of which would also be absorbed in its passage through the water. It may be farther observed, as in the case of Goodwyn's experiments, that from the form and texture of the vesicles, and still more of the bronchia, it is not possible, by mere pressure, to expel all the air which they contain. It appears therefore evident, that Mr. Coleman has very much underrated the capacity of the lungs in their state of complete expiration, although it is obviously impossible to ascertain the amount of the error[1].

From the above data, which, although confessedly imperfect, are the best which we possess, we may form some approximation

impossible that the lungs could be contracted even to half their natural dimensions, unless they were removed from the chest and deprived of air by boiling. See references to note 3, p. 318.

[1] The same cause may probably operate, to a certain extent, in death, produced by any cause, except by hanging; that the violent effort to expire will expel a portion of air, the place of which will be partly occupied by aqueous vapour, and thus make the residual contents of the lungs appear too small. Messrs. Allen and Pepys estimate the quantity at 108 cubic inches; Phil. Trans. for 1809, p. 412; this, I conceive, from various considerations above stated, to be below the average. In another part, indeed, of their papers they state 141 cubic inches as the residual quantity; Phil. Trans. for 1808, p. 270.

to the knowledge of the quantity of air contained in the lungs in their different states of distention. Assuming 170 cubic inches as the quantity which may be forcibly expelled, and that 120 will be still left in them, we shall have 290 cubic inches as the measure of the lungs in their natural or quiescent state; to this quantity 40 cubic inches are added by each ordinary inspiration, giving us 330 cubic inches as the measure of the lungs in their distended state[1]. Hence it will appear that about ⅛ of the whole contents of the lungs is changed by each respiration, and that nearly ⅔ can be expelled by a forcible expiration. Supposing that each act of respiration occupies 3 seconds, or that we respire 20 times in a minute, a quantity of air rather more than 2¾ times the whole contents of the lungs will be expelled in a minute, or about 4000 times their bulk in 24 hours. The quantity of air respired during this period will be 1,152,000 cubic inches, or about 666½ cubic feet.

There are two curious subjects of inquiry, connected with the mechanism of respiration, which have abundantly exercised the genius of physiologists; what is the cause of the first inspiration in the newly-born infant, and what is the cause of the regular alternations of inspiration and expiration during the remainder of life. The first of these queries was proposed by Harvey as a problem for the consideration of his contemporaries. "Quomodo nempe embryo," he asks, "post septimum mensem in utero matris perseveret? cum tamen eo tempore exclusus statim respiret; imo vero sine respiratione ne horulam quidem superesse possit; in utero autem manens ultra nonum mensem, absque respirationis adminiculo, vivus et sanus degat."[2] The question may be stated more generally, why is the animal which has once respired, under the necessity of continuing the respiration without intermission, when, if the air had never been received into the lungs, the same animal might have remained for some time without exercising this function?[3]

Many solutions were proposed of this problem, depending upon principles which are obviously erroneous, and are now totally discarded; but the hypothesis of Whytt deserves attention, on account of the reputation which it long maintained, in some of the most distinguished schools of physiology. He observes, that before birth the blood of the fœtus is properly elaborated by the mother, but that when the communication is cut off, it becomes necessary for the young animal to produce the requisite change in its fluids by means of its own respiration. In furtherance of this end he supposes that, immediately after birth, an uneasy sensation is experienced in the chest from the want of fresh air, which may be regarded as the appetite for breathing, in the same manner as hunger and thirst are the appetites for food and drink. To supply this appetite, the sentient principle, with which the body is endowed, causes the expansion of the chest, in order to prevent the fatal effects which would ensue, were not the lungs to be immediately brought into action. This appetite for air is supposed to commence at birth, because, in consequence of the struggles of the fœtus at this period, the circulation will be quickened, and an additional quantity of blood will now pass through the lungs, which stimulates them into action, and seems to be the immediate cause of this appetite. He considers the exercise of the function of respiration "as owing to a peculiar sensation of the body, which determines the mind or sentient principle to put certain muscles or organs into motion." With respect to Harvey's problem, he regards it "to be of so very easy solution, that it is not a little surprising, that many physiological writers should have attempted it in vain." He explains it upon the principle of the change which takes place in the direction of the blood, the whole of which now passes through the vessels of the lungs, and which would stagnate in them, were it not propelled through them by the alternate motions of the chest[1].

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[1] See Sprengel, Instit. Med. t. i. p. 470.

[2] Excr. de Gener. p. 361.

[3] There is scarcely any opinion in physiology, however absurd it may appear at first view, which has not found some supporters, and accordingly attempts have been made to prove that the fœtus breathes whilst still in the uterus. See Boyle's Works, v. i. p. 110; Haller, El. Phys. xxix. 4. 54; also Whytt on Vital Motions, sect. 9. p. 111..114, where the subject is very fully and satisfactorily discussed.

Haller refers the cause of the first inspiration to the habit which the fœtus had acquired, while in the uterus, of taking into the mouth a portion of the fluid in which it is immersed, and supposes that it still continues to open its mouth, after it leaves the mother, in search of its accustomed food; the air will therefore rush into the lungs, expand them, and thus reduce them to the state of a breathing animal, in consequence of which change they will require a regular supply of fresh air, to prevent the blood from stagnating in its passage from the right to the left side of the heart[2].

A somewhat similar view of the subject is taken by Darwin. He coincides with Haller so far as to conceive, that the fœtus acquires the power of deglutition before it leaves the uterus; but he remarks, that the acts of swallowing and of breathing are essentially different. When the fœtus is separated from the mother, an uneasy sensation is experienced from the want of air; to remove this uneasiness all the muscles of the body are called into action, and among others those of the thorax, and the uneasiness being by this means relieved, to use his own expression, "respiration is discovered," and the same action is afterwards repeated when the same uneasiness recurs[3]. Dr. Philip thinks the difficulty may be solved by regarding the muscles of inspiration as entirely under the control of the will, and thrown into action by the uneasy sensation which the young animal experiences when it is separated from the mother, and can no longer have the necessary change produced upon the blood by her organs. He supposes the first inspiration to be entirely analogous to the first act of deglutition; the will, in both cases, causing the contraction of certain muscles for the purpose of removing an uneasy sensation[1]. I shall not enter into a formal examination of these hypotheses; they appear to me to be built upon the assumption of principles, which are at least doubtful, if not altogether untenable; and with respect to the explanation offered by Whytt, it labours under the radical defect of all the metaphysical reasoning of the spiritualists, that it confounds the final with the efficient cause, and supposes the agency of an imaginary power, of the existence of which we have no evidence.

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[1] On Vital Motions, sect. 9. p. 109..122. The author remarks that many physiologists have ascribed the first inspiration to instinct, but as he dislikes "the use of words whose meaning may be obscure or indefinite," *p. 114, he prefers* the explanation which is given in the text.

[2] *El. Phys.* viii. 5. 2.

[3] Zoonomia, v. i. sect. 16. § 4.

I think it may be doubted whether we are in possession of any data which will enable us fully to explain the difficulty; but there are some circumstances connected with the mechanical change which the lungs experience at birth, in consequence of the alteration of the position of the animal, that may throw some light upon it. Before birth the lungs only receive one-third part of the quantity of blood which afterwards circulates through them[2], and are squeezed up into as small a space as possible from the posture of the fœtus[3], as well as from the larger size of the heart and the liver, and by the thymus gland[4], so that the cavities of the vesicles and bronchia are nearly obliterated. The arch of the ribs is depressed, and the diaphragm is pushed up into the higher part of the chest, so that its concavity toward the abdomen is greater at this period than it ever afterwards becomes when the animal has once respired[5].

As soon, however, as the position of the animal is changed upon its leaving the uterus, the trunk is extended, and the pressure removed from the thorax and abdomen. The elasticity of the parts being then at liberty to act, the arch of the ribs is raised, and the distance increased between the sternum and the spine, the liver and the other abdominal viscera now fall into their natural position, and permit the diaphragm to assume its ordinary curvature. All these changes necessarily increase the capacity of the thorax, and cause the air to rush down the trachea of the animal into the bronchial vesicles, when the blood, meeting with less resistance to its passage through the lungs than through the foramen ovale, the whole of it passes through the pulmonary artery. The organs are thus brought into the state of ordinary expiration, or what I have termed their quiescent condition, when the necessity for inspiration will depend upon the same cause, which renders the alternation of inspiration and expiration essential to the future existence of the animal. According to this view of the subject, the first degree of expansion, which is produced in the lungs of the newly-born infant, depends merely upon the removal of external pressure, which permits the different parts of the trunk to assume their ordinary position. The farther increase of the size of the chest will depend upon the contraction of the diaphragm, and perhaps, strictly speaking, this contraction should be regarded as constituting the first act of inspiration[1].

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[1] Quart. Journ. v. xiv. p. 100.

[2] Boerhaave and Haller, in Prælect. t. ii. § 200 cum notis.

[3] Harvey de Gener. p. 353; Denman's Midwif. p. 218; Murat, in Dict. Sc. Méd. art. "Fœtus," t. xvi. p. 55.

[4] Haller, in Boer. Præl. t. v. par. 2. not. ad § 681; and El. Phys. xxix. 4. 39; Denman, p. 158..161; Blumenbach's Physiol. p. 361; Magendie, Physiol. t. ii. p. 435; Monro's Elem. v. i. p. 576; pl. 10; v. ii. p. 113.

[5] Petit, Mém. Acad. pour 1733, p. 6; Senac, ibid. pour 1724, p. 171. See Hunter on the Gravid Uterus, pl. 12, 13, 20; also Sœmmering, Icon. Embryon. Hum. fig. 18, 19, 20; for the representation of the posture of the fœtus.

Nearly allied to the question respecting the first commencement of respiration is the inquiry into the cause of the regular alternations of inspiration and expiration, a subject which has given rise to as many hypotheses and speculations as the former, but being perhaps in itself more difficult of explanation, still remains at least equally involved in obscurity. Some physiologists have considered the necessity for the alternations of respiration to the support of life as a sufficient reason for its existence, thus substituting the final for the efficient cause of the action [1]. Others have attributed it to some mechanical effect, depending upon the pressure on the brain or a particular nerve, by the lungs or the diaphragm, at certain stages of the act of respiration [2]. Others again have accounted for it by some speculative principle assumed concerning muscular contraction in general, which they have applied to the organs connected with the chest, among whom we may class Willis [3], Pitcairne [4], and Hartley [5]; while others ascribe it to the effect of habit, association, or instinct [6].

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[1] Besides the hypothesis of Whytt, Haller, and Darwin, which, in consequence either of their supposed merits, or the celebrity of their authors, have acquired some degree of consideration, many others have been formed by physiologists of eminence; as by Borelli, who resolves the question simply into the necessity which now exists for the young animal to perform those functions, which were before exercised by the mother, par. 2. prop. 118; by Pitcairne, Dissert. p. 62; and by Petit, Mém. Acad. pour 1733, p. 6; who refer it to certain general laws, which they suppose to prevail with respect to the action of the muscles and the animal spirits; by Lister, who explains it upon the principle that the blood, which before birth passed through the umbilical, is now transmitted through the pulmonary vessels, de Respir. in Exercit. Anat.; by Swammerdam, who conceives that there is in the fœtus a space between the lungs and the thorax, which is filled with an aqueous vapour, which being expelled when the animal first attempts to breathe, enables the air to enter the lungs, De Respir. Sect. 2. c. 1; by Boerhaave, Instit. § 691; Hartley on Man. v. i. p. 95; Buffon, Nat. Hist. v. iii. p. 111; and Blumenbach, Physiol. § 151; who ascribe it to the struggles of the fœtus when it leaves the uterus, by which the muscles generally, and the diaphragm in particular, are thrown into action, and the uneasy sensations which are experienced from diminished temperature, and the contact of surrounding bodies. Dr. Elliotson ascribes it solely to the impression of the cold air upon the surface of the body; Notes to Blumenbach, p. 84; also Physiol. p. 214 et seq. Wrisberg appears to make no distinction between the cause of the first expansion of the chest and the subsequent act of inspiration; De Respir. prima, in Sandifort, Thes. t. iii. p. 253..260; Sprengel, Instit. Med. t. i. p. 464; and Parr, Dict. art. "Fœtus," adopt an opinion very nearly similar to that in the text. Sœmmering, like Borelli, confounds the final with the physical cause; Corp. Hum. Fab. t. vi. § 70. Sir D. Barry ascribes the commencement of respiration to the constant effort of the heart to contract its cavities, depending on the principle, which he has endeavoured to establish, that the action of the respiratory organs tends to produce a partial vacuum round the heart; *Med. Chir.* Rev. v. vii. p. 434.

It will not be necessary to enter into any minute account of these hypotheses, and still less into any examination or refutation of them, as they appear to have been scarcely maintained except by the individuals who originally proposed them. But it may be proper to examine a little more in detail the opinions which were entertained upon this point by Haller and Whytt, because at one time they acquired considerable reputation, and probably approach somewhat more nearly to a correct view of the subject. Haller sets out with the position, that the passage of the blood through the lungs is impeded during expiration; this produces a reflux of blood into the veins, and causes a degree of pressure upon the brain. Hence arises a painful sense of suffocation, in consequence of which the will calls into action the muscles of inspiration, in order to enlarge the thorax, and, in this way, to remove the impediment. But the same uneasy feelings which were produced by expiration, ensue from inspiration, if too long protracted; the muscles therefore now cease to act, and by their relaxation produce the contrary state of the chest [7]. Whytt, like Haller, conceives that the passage of the blood through the pulmonary vessels is impeded by expiration, and that a sense of anxiety is thus produced; this unpleasant sensation acts as a stimulus upon the nerves of the lungs and the parts connected with them, which excites the energy of the sentient principle, and this, by causing the contraction of the diaphragm, enlarges the chest and removes the painful feeling; the muscles then cease to act in consequence of the stimulus no longer existing [8].

Upon these hypotheses we may remark, that they each of them involve three distinct positions, in the first two of which they agree, that during expiration, the passage of the blood through the pulmonary vessels is retarded, and that this produces a sensation of uneasiness in the chest; while they differ in the third position, the means employed to remove the uneasiness, Haller ascribing it to a voluntary effort, and Whytt to the operation of the sentient principle. Now we may venture to affirm that all these assumptions are at least questionable, if not absolutely erroneous, so that notwithstanding the great names under the sanction of which they were advanced, we may pronounce the hypotheses to be untenable. Whytt and Haller have both fallen into the error of explaining the phenomena of natural or ordinary respiration by what occurs only in the unnatural or extraordinary efforts of the lungs. Now, as it will hereafter appear, it is doubtful whether the circulation through the pulmonary vessels be ever affected by the motions of the thorax, except in extreme cases of accident or disease; and although, in laborious respiration, we experience an uneasiness, which may prompt us to expand the chest, we are not sensible of this operation in ordinary cases, nor have we any evidence of its existence. Hence we must dismiss all those speculations which are founded upon the idea of the lungs having an appetite for air, analogous to the sensation of hunger, which is experienced by the stomach [1], for it is impossible to conceive of an appetite which is unattended with consciousness or perception. And with respect to the third point of Haller's hypothesis, the interference of the will, we may remark, that respiration is performed in the most perfect manner by the infant immediately after birth, before any object of volition can be contemplated, or indeed before the faculty can have been called into existence; nor shall we be more disposed to place any confidence in Whytt's doctrine of the sentient principle as affording any actual explanation of the phenomena. I conceive therefore that we must consider this much agitated question as still undecided, nor do I think that we are in possession of any facts which will enable us to afford a satisfactory answer to it. I shall, however, offer some considerations, which may tend to show us towards what points our observations should be directed in order to obtain its solution.

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[1] Borelli, par. 2. p. 117; Bellini, de Urinis, Intr. Lemma 18.

[2] Boerhaave, Instit. § 419, 0; Martine, Ed. Med. Ess. v. i. p. 156.

[3] Pharm. Rat. par. ii. p. 18.

[4] Dissert. par. 4. p. 62.

[5] On Man, v. i. ch. 1. prop. 19.

[6] See Sprengel, Inst. Med. § 210.

[7] El. Phys. viii. 4. 17, 18. 28. and not. 6. ad. Boer. Præl. § 619. t. v. p. 62.

[8] Whytt on Vital Motions, *sect. 8. p. 81..109.*

When we reflect upon the intimate nature of the process of respiration, we shall find that our inquiry will assume the following form: what is the cause which produces the contraction of the diaphragm, when the same portion of air has remained for above a certain length of time in the lungs? and why, after a short interval, does this cause cease to operate, and permit the diaphragm to become relaxed? I am disposed to think that in the investigation of this subject, we must have recourse to three distinct principles of action corresponding to the different states of distention which the thorax experiences, or the different modes by which it is effected. A portion of air has been received into the lungs, and has acted upon the blood which circulates through them, and then becomes unfit for the farther performance of this office. Were the air not renewed, the blood, by not undergoing its appropriate changes, would be rendered incapable of carrying on its functions, one of the most important of which is to supply the muscles with their contractile power; hence the heart would be no longer able to contract, and the circulation would cease. But at the very commencement of this state of the organs, or even probably before it has commenced, by a mode of communication, which we are perhaps unable to explain[1], an impression is conveyed to the diaphragm, which causes it to contract, so as to enlarge the chest, and thus admit a portion of fresh air to enter the lungs. According to the usual operations of the animal œconomy, the impression, whatever may be its nature, no longer acting upon the diaphragm, its contraction is succeeded by relaxation, when the air, having now performed its due office, of rendering the blood proper for the support of life, is expelled, and the parts are brought back to their former state. The final cause of the operation is sufficiently obvious, and we are able to trace it through its successive steps; but we are not able clearly to understand the physical connexion between the state of the air and the contraction of the diaphragm, or rather, what is the exact nature of the stimulus which excites the nerves of this organ so as to cause it to contract[2].

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[1] *Chaussier*, Dict. Sc. Méd. art. "Respiration," t. xlviii. p. 23.

The above remarks apply to what may be termed the ordinary act of respiration, where the effect appears to depend simply upon muscular contraction, although, as I conceive, we are unable to explain the nature of the stimulus which acts upon the contracted part, or the mode in which it is communicated to it. But there are other cases, in which the action of the respiratory organs seems to depend upon a cause of a different nature, where we are sensible of the existence of the stimulus, and are conscious of the effect which it produces upon the system. The chest is here thrown into a state of extraordinary action, from the more quick and violent contraction of the diaphragm, and the co-operation of various auxiliary muscles. In this case, of which sneezing may be adduced as an example, we are better able to explain the mechanical origin of the train of actions, while the interesting observations of Sir C. Bell[3] point out the nervous communication which subsists between the parts which connect the successive steps of the process, although it may be still somewhat difficult to explain the relation which it bears to the ordinary act of respiration.

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[1] How far we can throw any light upon this train of actions, by supposing that the part of the eighth pair of nerves which is distributed over the lungs, imparts to them the power of receiving the perceptions of pain, which acts as a stimulus upon the diaphragm, will be considered hereafter.

[2] The diaphragm appears to be an organ which is singularly adapted for receiving the impressions of various stimuli, both on account of its peculiarly contractile nature, which has been frequently remarked upon, and from the relations of its nerves, which, as Sprengel remarks, are connected with every part of the system; Inst. Med. lib. i. cap. 4. sect. 1. p. 443.

[3] Phil. Trans. for 1821, p. 398 *et seq.*, and for 1822, p. 284 et seq.

Both of the above species of action are independent of the will, and, indeed, in the latter case, are often in direct opposition to it; but there is a third species which is completely voluntary, as sighing, straining, &c., in which certain muscles are thrown into contraction, in order to accomplish some purpose, from a previous knowledge of their effects, in the same manner as the motions of the arms and legs. We may therefore conclude that ordinary respiration depends upon a cause, which, in some way that we are not able to explain, acts on the diaphragm, so as to produce its contraction; but when any circumstance requires the parts to be more fully expanded, it is either by the application of a stimulus to a sensitive part, which is connected with the respiratory muscles, or by the intervention of the will, according as the effect regards our immediate existence, or is only subservient to our comfort or accommodation[1].

SECT. 2. *Mechanical effects of Respiration.*

After explaining the mechanism of respiration, I proceed to consider its direct effects; these may be arranged under three heads, the mechanical effects, the effects produced upon the air, and those upon the blood. The older anatomists and physiologists insisted much upon the mechanical effects that were produced by the alternate motions of the thorax, upon the organs contiguous to it. Hales and Haller announced, as the result of direct experiments, made expressly for the purpose of ascertaining the point, that the circulation, and the other functions the most essential to life, were promoted or retarded according as the lungs were in the different states of inspiration and expiration, while they conceived that the due performance of the circulation and other vital functions was closely connected with the regular motions of the thorax. The parts or organs which have been supposed to be immediately influenced by the mechanical act of respiration are the following,—the circulating system, especially the pulmonary vessels; certain nerves which are contiguous to, or in contact with the diaphragm; the abdominal viscera generally, and the liver in particular; and lastly, the lacteals and the thoracic duct.

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[1] Magendie points out three degrees of inspiration, which he says are well marked; an ordinary, a great, and a forced inspiration; Physiol. t. ii. p. 274; but although they nearly correspond in their effects to what I have described above, they are supposed to depend either upon the action of different parts, or a different degree of action of the same parts. Dr. M. Hall lately presented to the Zoological Society an account of a series of experiments which he performed on a decapitated turtle, from which he deduces some important conclusions respecting the agency of the nervous system in respiration, and the particular part of it which is concerned in this process. He considers the three following positions to be established by his experiments: "1. If the cerebrum be removed, respiration continues as an involuntary function through the agency of the eighth pair of nerves; 2. If the eighth pair be divided, respiration equally continues, but as an act of volition; but, 3. If the cerebrum be first removed, and the eighth pair be then divided, respiration ceases on the instant. Volition is first removed with the cerebrum; the influence of the eighth pair is then removed by its division. The two sources of the mixed or double function being both cut off, the function ceases." Phil. Mag. for Jan. 1835, p. 71, 2. This conclusion, it will be seen, is in favour of the doctrine maintained in the text, but the value of Dr. Hall's experiment consists in affording a more precise and definite explanation of a doctrine which previously rested upon general considerations only, and substituting experimental proof for *plausible* hypothesis.

As the earlier physiologists were entirely ignorant of the chemical effects of respiration, they were the more disposed to pay attention to those of a mechanical nature, and as their knowledge even on this point was very imperfect, we can scarcely be surprised at the errors which they committed. The intimate connexion which appeared to subsist between the respiration and the circulation induced them to examine minutely, whether the blood was transmitted through the lungs with equal facility during inspiration and expiration; and besides much hypothetical reasoning and elaborate calculation, numerous experiments were performed upon living animals to determine the question. We may, however, conclude that the mode of investigation which they adopted was quite inadequate to the purpose. The state into which the animal must necessarily be reduced, during experiments of this description, can at most only show us what takes place during the most violent or forced actions of the chest; the loss of blood, the pain inflicted, and the general derangement of the ordinary actions of the system could not fail to affect the organs of respiration so far as to render it impossible to learn, by this means, what effect their ordinary changes would produce upon the circulation and the other functions.

We may point out three distinct sources of error into which Hales and Haller, together with almost all the physiologists of the last century, fell, when they attempted to apply the results of their experiments to the question under consideration; first, they either entirely overlooked the unnatural situation in which the animals were placed, or did not make sufficient allowance for it; secondly, they observed the effects produced upon the circulation or other functions by the most violent efforts of the respiratory organs, and reasoned from these to their ordinary action; and, in the third place, they much exaggerated the change of bulk which the chest experiences in the different states of inspiration and expiration[1].

We shall first examine how far the circulation is affected by the mechanical action of the lungs, as it was upon this function that the most important changes were supposed to be produced, and that to which the experiments were more particularly directed. The first experiments of this kind, which possess such a degree of precision as to render them deserving of our attention, are those of Hales; he inserted a tube into the crural artery of a horse, and observed that the blood was evidently raised to a greater height in the tube, when the animal made a deep inspiration; this he attributed to the greater ease with which the blood was enabled to traverse the pulmonary vessels at this period, and thus furnish a more copious supply of it to the heart[1]. Perhaps from this experiment we may go so far as to infer, that during a very full inspiration, and in an exhausted state of the system, the blood will be transmitted with more facility through the lungs. Yet we are not warranted to conclude from it that this would be the case under ordinary circumstances, while, at the same time, we may remark that Haller himself, who adopts Hales's conclusion[2], supposes that a very full and long protracted inspiration would retard the passage of the blood through the lungs[3].

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[1] Haller, El. Phys. *viii. 4. 11. and the* authors to whom he refers.

The opinion of many eminent physiologists was that, during expiration, when performed in its ordinary and natural manner, the bronchia and vesicles are, to a certain extent, folded up or compressed, so as to be less pervious to the blood, while, when they are expanded by inspiration, the blood is suffered to pass through them with facility[1]. This appears to have been the opinion which was generally entertained among Haller's contemporaries, and which was, by some of them, exaggerated in a most extraordinary degree[2]. This opinion professed to be directly deduced from experiment, but the experiments were of that vague and unsatisfactory kind, which were so frequently employed by the older physiologists, while no allowance was made for the peculiar situation in which the animals were placed, and for the consequent derangement which must necessarily ensue in every part of their œconomy. And we have, in support of the opposite opinion, an experiment of Goodwyn's, which appears to have been carefully performed, and which bears directly upon this question. He introduced a quantity of water between the pleuræ of a dog, and when even as much as one-third of the cavity of the thorax was filled, he could not perceive that the passage of the blood through the lungs was retarded, although the respiration was rendered laborious[3]. The same view of the subject was also taken by Bichat, who has detailed various experiments and observations, made for the express purpose of ascertaining how far the circulation is affected by the state of distention of the lungs, the results of which appear to be quite decisive[4].

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[1] Stat. Ess. v. ii. p. 6.

[2] El. Phys. viii. 4. 11.

[3] El. Phys. viii. 4. 13. Boerhaave also maintained this opinion, and founded upon it his hypothesis of the cause of the alternation of inspiration and expiration; Instit. § 619, 0. I have already had occasion to refer to the experiments of Sir D. Barry on the condition of the heart and great vessels during the different stages of the act of respiration, from which he concludes, that during inspiration the vena cava is considerably encreased in its dimensions, in consequence of the partial vacuum which he conceives is then formed in the thorax; Ann. Sc. Nat. t. xi. p. 113 et seq. He has employed the same principle in explaining the phenomena of the circulation, and endeavoured to establish it by a series of experiments. They consisted essentially in inserting one end of a tube into the jugular vein, while the other end was immersed in a coloured fluid, when it was observed, that whenever the animal inspired, the fluid ascended in the tube, while during expiration, the fluid remained stationary or was even repelled; Recher. Exper. sur les Causes du Mouvement du Sang; Ann. Chim. et Phys. t. xxx. p. 191..3. I had the good fortune to be present at some experiments of this kind which were performed by Sir D. Barry himself, at the Veterinary College. Making those allowances for unforeseen and unavoidable difficulties, which it is but fair to admit on such occasions, it appeared sufficiently obvious, that when one end of a glass tube was inserted either into the large veins, into the cavity of the thorax, or into the pericardium, the other end being plunged into a vessel of coloured water, the water was seen to rise up the tube during inspiration, and to descend during expiration. We may hence conclude, that under the circumstances in which the experiment was performed, there was less resistance to the entrance of the venous blood into the heart during inspiration than during expiration; but it will still remain for us to inquire whether the principle will apply to the organs in their natural and entire state, or in what degree it is applicable. Now, there are some facts, which would lead us to suppose, either that the effect does not take place, or that it does so in a very slight degree only. 1. During each act of respiration, we have three or four pulsations of the heart, which are exactly of the same strength, although they must have occurred during the various states of the thorax. 2. In the fœtus, where the lungs are quiescent, we have still the circulation proceeding without any apparent difficulty. 3. There are various tribes of animals that possess a circulating system; but which are either without lungs, or have them constructed upon a principle entirely different from those of the mammalia.

It has been observed that when a portion of the cranium is accidentally removed, an alternate elevation and depression is visible, corresponding with expiration and inspiration[1], and Haller extends the operation to some of the other viscera[2]. This effect has been attributed to the greater resistance which the blood experiences in its passage through the lungs while in the state of expiration ; a stagnation is thus brought about in the right side of the heart, and consequently the blood is retained in the veins, which, in a highly vascular part, produces an increase of its bulk. When from the enlargement of the thorax the lungs become more pervious to the passage of the blood, the veins are enabled to empty themselves, and the increase of bulk is removed. It is probable that the phenomenon depends upon the cause that has been assigned : but it must be remarked, that in cases of this kind, where so considerable an injury is received by the cranium, or where any of the internal viscera are laid open, the respiration may be supposed to be more laborious than natural, so that the air will be taken in at longer intervals and consequently in larger quantity, while the system being, at the same time, in an exhausted state, the circulation will probably be more easily affected by the operation of slight causes, which would not be perceived in its healthy and natural condition[3].

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[1] Haller, El. Phys. vi. 4. 10; viii. 4. 11; viii. 5. 21 et alibi; notæ ad Boerhaave, Prælect. t. ii. § 200. The same opinion appears to be implied in the remarks of Sœmmering, Corp. Hum. Fab. t. ii. § 68. Boerhaave is inclined to think that not more than one-third of the blood can pass through the lungs during expiration, ubi suprà, not. 11. An opinion of a similar tendency had been previously maintained by Malpighi, Oper. Post. p. 19; and by Winslow, Anat. sect. 9. § 139..1.

[2] J. Bell, Anatomy, v. ii. p. 188 et seq. in his usual impressive manner, very aptly exposes the extravagancies and inaccuracies of his predecessors on this subject, although his own opinions will perhaps be found not to be entirely correct. Harvey appears to be the first physiologist who clearly pointed out the independence of the actions of the heart and the lungs; yet he was of opinion that the passage of the blood along the pulmonary vessels is assisted by the motion of the thorax; De Motu Cordis, p. 11, 71, 77. Mr. Kite has given us a series of experiments, the object of which is to prove, that in the state of complete expiration, which he supposes to be produced by drowning, very little blood can pass through the vessels of the lungs; Essays, p. 47..58. But even if we admit his reasoning, it would not apply to the ordinary act of respiration.

[3] Essay, p. 45. Morbid cases of a similar nature, as far as this point is concerned, are not unfrequently met with, where fluids of various kinds are effused between the pleuræ, and where the respiration is much affected, while the circulation proceeds without any impediment. See Morgagni, on the Seats of Diseases, by Alexander, v. i. p. 408.

[4] Sur la Vie, &c. part. 2. art. 6. § 1.

But there is a consideration which is of more weight in the determination of this question, than the result of any experiments can be, in which it appears impossible to obviate every source of uncertainty. In the healthy state of the system we respire upon the average about twenty times in a minute, while the average velocity of the pulse may be estimated at eighty, so that the heart contracts four times during each act of respiration; and must consequently receive the blood during all the various states of distention to which the lungs are subject, yet we do not perceive that the pulse exhibits any corresponding variations either in its strength or its velocity. And farther, we shall find it extremely difficult to produce any effect upon the pulse by the most powerful voluntary efforts of inspiration or expiration; yet, in such cases, the capacity of the thorax will certainly undergo a much greater change than it can possibly experience in its ordinary action[1].

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[1] Haller, El. Phys. vi. 4. 9 ; viii. 4. 27 et alibi ; Mém. sur les Parties Irrit. et Sens. t. ii. p. 172..192 ; see also the experiments of Haller's correspondents, Tossetti and Caldani, ibid. t. ii. p. 198. and t. iii. p. 141 ; with his own observations, t. iv. p. 15 ; also Opera Minora, t. i. p. 131..7, 141. I had once an opportunity of witnessing this phenomenon, in a very striking manner, in a patient who had lost a portion of the cranium, owing to the growth of a fungous tumour ; upon removing the tumour, the bone was found to be in a diseased state, and was likewise removed, leaving the brain exposed. It would appear that the motion of the brain was observed by Lamure about the same time that Haller was engaged in his experiments on this subject. See Haller's Letter in Op. Min. t. i. p. 242 ; Lamure in Mém. Acad. Scienc. pour 1749, p. 541 et seq. His experiments were performed in the years 1751 and 1752, and were read to the Academy in 1752 ; the volume was published in 1753.

[2] El. Phys. vi. 4. 9 ; Op. Minor. t. i. p. 137.

[3] The so much celebrated experiment of Hooke, in which, after the motion of the heart had ceased, it was re-produced by inflating the lungs, has been adduced to prove that the lungs are rendered more pervious to the blood by inspiration ; Haller, El. Phys. viii. 4. 12. But it must be borne in mind, that in this case, the thorax was laid completely open, and that the lungs would consequently be collapsed into a much smaller bulk than while they were within the cavity of the thorax, at the same time that we account for the renewed motion of the heart upon a different principle, totally unconnected with a mechanical operation. With respect to the experiment itself, it is not a little remarkable, that although it excited so much attention when it was performed by Hooke, and was viewed by his contemporaries with almost childish astonishment, the very same process had been performed long before by Vesalius ; see Corp. Hum. Fab. lib. 6. c. 19. in Oper. t. i. p. 571, 2. This great work was published in 1543 ; Hooke's experiment was performed in October 1667. Hooke drew the correct inference from his experiment, and directly states his opinion that it was the fresh air, and not any *alteration* in the capacity of the lungs, which caused the renewal of the heart's

The connexion, which was supposed by the older writers to exist between the heart and the thorax depended upon their idea of the structure and mechanism of these organs, but some of the modern physiologists, who have insisted upon the reality of this connexion, have ascribed it to a cause rather of a metaphysical, than physical nature. Hunter speaks of a sympathy or association existing between the actions of the heart and the lungs[2]; we also find an opinion of the same tendency advanced by Currie[3], and Darwin still more explicitly refers to the effects of association. He says that "innumerable trains or tribes of other motions are associated with these muscular motions that are excited by irritation; as by the stimulus of the blood in the right chamber of the heart the lungs are induced to expand themselves, and the pectoral and intercostal muscles, and the diaphragm, act at the same time by their associations with them."[4] But, in opposition to this hypothesis, we may remark, that the lungs of a newly born animal act with full force immediately upon being placed in a situation where they can have access to the air, long before the effect of association can possibly operate. Besides, in after life, the periods of the contraction of the heart and the diaphragm bear no ratio to each other, the one being often much increased or diminished in frequency, while the other is not affected. The heart

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motion. He farther informs us that the circulation through the lungs went on freely when the lungs were suffered to subside. For the original account of the experiment see Phil. Trans. No. 28. p. 539; see also Lowthorpe's Ab. of Phil. Trans. v. iii. p. 66; and Sprat's Hist. of the R. S. p. 232.

[1] This consideration appears to me, in a great measure, to counteract the force of Magendie's reasoning in a paper in his Journal, t. i. p. 132 et seq.; most of the experiments which he relates must be regarded as showing what takes place in an unusual action of the respiratory organs.

[2] On the blood, p. 54.

[3] Med. Reports, p. 77.

[4] Zoonomia, v. i. p. 40. Beddoes, proceeding upon this principle, goes so far as to form a project for rendering animals amphibious; Observ. on Fact. Airs, part i. p. 41. Upon this I shall have occasion to offer some remarks in a subsequent part of this chapter.

and the lungs are, however, indirectly connected with each other, inasmuch as they are both of them affected by the quantity and the quality of the blood which is transmitted through them, and, to a certain extent, are both of them under the influence of the nervous system. But this connexion is not to be referred to the effect of habit or association, but to the direct application of the same agent to each separate organ. Upon the whole then we may conclude, that in the ordinary act of respiration the blood is transmitted through the lungs at all times with nearly equal facility, and that it is only in extreme cases that the retardation described by Haller and his contemporaries can be supposed to take place.

Besides the pulmonary system of blood-vessels, there are other parts of the sanguiferous system which it was supposed would be affected by the mechanical actions of the thorax, and this it was conceived would be especially the case with the aorta and the vena cava, by the pressure of the diaphragm upon them during its contraction. Yet on inspecting the anatomy of the parts, it would scarcely appear that this effect should be expected to take place, as the passages through which these vessels are transmitted are guarded, as if for the express purpose of preventing the stagnation of their contents[1]. Haller indeed informs us that, in some of his experiments, he observed the vena cava to be compressed by the diaphragm, so as perceptibly to impede the passage of the blood through it[2]. Yet we may here, as on so many former occasions, attribute the effect to the state to which the animals were reduced, with all the functions deranged, the circulation probably much exhausted, and the diaphragm convulsed by the near approach of death.

Certain nerves, which perform very important functions in the animal economy, as, for example, the par vagum and the great sympathetics, pass through the diaphragm, and are so situated, that it was supposed they must be compressed by the contraction of this organ; it was therefore supposed that very important effects would hence result in the parts to which the nerves are sent, that there would be an alternate transmission and obstruction of the nervous influence, which would explain the motions of the chest, the pulsation of the heart, and the vermicular actions of the stomach and intestines[3]. But more accurate observations seem to prove that there is no foundation for this opinion, that we have no evidence of this pressure upon the nerves, and that we are able to explain the phenomena more satisfactorily upon other principles.

As the alternate motions of the chest, produced by the diaphragm, must cause a corresponding motion of the whole of the abdominal viscera, it has been thought that the agitation and pressure which they would hence experience will be instrumental in propelling the blood through the lungs, and in this way indirectly contribute to the formation of the various secreted fluids [1]. But we may presume that this effect has at least been much over-rated; for it appeared in the experiments of Menzies, that the increase of bulk which the body experiences during inspiration is precisely equal to the volume of air taken into the lungs [2], the change of capacity which the abdomen would experience from the contraction of the diaphragm being exactly balanced by the relaxation of the abdominal muscles, and *vice versâ*, so that although the viscera may be supposed to be at all times under a certain degree of compression, the measure of it will be at all times nearly the same. No accelerations of the contents of the veins can therefore be produced by this cause, and indeed, as they are not furnished with valves, we may presume that any unusual pressure upon them would tend to diminish rather than to increase the flow of blood through them [3]. Perhaps, however, we may, to a certain extent, admit of the mechanical effect of the respiratory organs upon the liver, in consequence of its situation with respect to the diaphragm and the ribs, and there may be supposed to be a peculiar necessity for some mechanical force of this kind with respect to the gall bladder, which is not furnished with muscular fibres, and would therefore appear to have no means of evacuating its contents, except what is derived from external pressure [4].

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[1] Winslow's Anat. [illegible] § 570...572; Haller, El. Phys. viii. [illegible] 15; and Icon. Anat. Fasc. [illegible]; Bell's Anat. [illegible] p. 226, 7.

[2] El. Phys. [illegible] 40; viii. [illegible] 36; [illegible] iii. p. 23.

[3] Martine, in Ed. Med. Ess. [illegible] p. 56 et seq.

It may seem also not improbable, that the state of compression in which the abdominal viscera are retained, and the alternate motion to which they are subjected, may have an effect in propelling the chyle along the lacteals and the thoracic duct [5]. These vessels differ from the abdominal veins in the essential circumstance of being plentifully furnished with valves, and thus any increase of pressure, or perhaps even any change of position in the parts, which must be always attended with some partial compression, will have the effect of pushing forwards their contents. In the forced or extraordinary actions of the thorax, these effects may be even considerable, although we cannot suppose that they will be very powerful in ordinary respiration, and upon the whole we may conclude, that they are much less than was supposed to be the case by the physiologists of the last century.

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[1] Haller, El. Phys. viii. 5. 23.

[2] Essay, p. 24 et seq.

[3] Haller, El. Phys. iii. 2. 2; viii. 5. 23; C. Bell's Dissect. v. i. p. 48.

[4] Haller, El. Phys. xxiii. 3. 29. Senac, in an elaborate essay on the diaphragm, Mém. Acad. Sc. pour 1729, p. 118 et seq., supposes that the posterior muscles, or pillars, as they have been termed, when they contract, must obstruct the passage of the œsophagus, but this does not appear to be confirmed by subsequent observations.

[5] Senac, Mém. Acad. pour 1724, p. 173; Haller, El. Phys. xxv. 2. 6. Cruikshank supposes that the thoracic duct may be affected by the motion of the diaphragm in certain states of unusual contraction, but not by its ordinary action; On the Absorbent Vessels, p. 168, 9.

SECT. 3. *Changes produced upon the Air by Respiration.*

From an inspection of the anatomy of the pulmonary organs, and particularly of the complicated apparatus by which the air and the blood are brought within the sphere of their mutual action, it was natural to conclude that the principal use of the lungs is to produce some change in the blood through the medium of the air. Intimations of this kind are frequently met with, even in the writings of the ancients [1]; but it was not until comparatively of late years, that we obtained any clear conception of the nature or effect of this action. Perhaps the most generally received opinion among the earlier physiologists was, that the air, by being taken into the lungs, removes from the blood a part of its heat and water, for even the most superficial observer could not overlook the fact, that air which is expired from the lungs, differs from that which is taken into them, by the addition of warmth and moisture. During the reign of the mathematical sect, there was a great controversy respecting the question, whether respiration had the effect of rarefying or of condensing the blood. Those who supposed that a principal use of this function is the exhalation of water from the lungs, concluded that the blood must be condensed; while others, who conceived that in the process of respiration a quantity of air is absorbed by the lungs, were equally strenuous in maintaining that the blood must be rarefied.

Although every one was aware of the necessity of the uninterrupted continuance of respiration, yet this was attributed more to some mechanical effect, which it produced upon the motion of the blood, than upon a change in its qualities: and it was not until the time of Boyle, that physiologists were fully sensible of the fact, that a perpetual supply of successive portions of fresh unrespired air is essential to life. Boyle discovered this fact in the course of his experiments with the newly invented machine, the air pump; but as his ideas were principally directed to the investigation of the mechanical properties of the air, it was in this point of view that he almost exclusively viewed its action upon the lungs. Yet he was not altogether inattentive to the other changes which it experiences; he noticed the moisture which was exhaled along with it, and he further supposed that it carries off, what he styles, recrementitious steams; but he does not give us any explanation of their nature. He also observed, that under certain circumstances, the air in which an animal had been confined for some time, was diminished in bulk; and this he accounts for by saying that it had lost its spring [2]. Many of

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[1] It has been remarked, that Cicero, who borrowed his philosophy, both natural and moral, from the Greeks, speaks of the air possessing a vital spirit, but nothing is said about its nature or mode of operation; De Nat. Deor. lib. ii. § 47.

[2] *Boyle's* Works, v. i. p. 99 et seq. and v. iii. p. 383.

Boyle's contemporaries agreed with him in the opinion, that the blood parted with something to the air during its passage hrough the lungs; but there were, on the contrary, many eminent physiologists, and particularly among the chemists, who supposed that the blood acquired something from the air [1]. This was he opinion of Sylvius [2], Baglivi [3], Borelli [4], Lower [5], and Willis [6], who imagined either that a portion of the whole mass of air, or hat some particular part or element of it, entered the blood. Among these, the first in point both of genius and originality was Mayow, who investigated the nature of the change which he air experiences in respiration with peculiar felicity of experimental research and acuteness of reasoning. He announces the ir of the atmosphere to be a compound body, and that it contained as one of its constituents, a peculiar gaseous substance, which, from its supposed connexion with nitric acid, he termed nitro-aerial spirit. It was this nitro-aerial spirit which gave he air its power of supporting flame, and it was the same volatile spirit which imparted to the air its vital properties, and which he blood abstracted from it during its passage through the lungs. The hypothesis of Mayow seems to have attracted considerable notice at the time when it was proposed, yet it was shortly afterwards generally abandoned, and what is more remarkable, the experiments and discoveries of Mayow respecting the constitution of the atmosphere, which were extremely curious, and admirably calculated to advance our knowledge of natural philosophy, fell into neglect, and at length were almost totally forgotten [7].

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[1] We have a curious statement by Boyle, Works, v. i. p. 107, of a person named Debrelle, who is said to have contrived a submarine vessel, and also discovered a kind of fluid, which supplied the navigators with what was necessary for their respiration. Boyle is well known to have been too much disposed to credulity; and we may venture to affirm, that the story, as he ells it, cannot be correct; yet it is scarcely probable that the whole was entirely without foundation.

[2] Praxis Med. lib. i. c. 21.

[3] Opera, p. 459, 0.

[4] De Motu Anim. pars 2. p. 113.

[5] De corde, p. 179 et seq.

[6] Pharm. Ration. pars 2. p. 34. The great name of Newton may be added to his list, as we find that he supposed there was an acid vapour in the air, which was absorbed by the lungs during respiration; Opera, a Horsley, t. iv. Optics, p. 245.

[7] The total neglect into which the experiments of Mayow had fallen, during he greater part of the last century, must be regarded as a very singular occurrence in the history of science. He lived at a period when experimental research was becoming fashionable, and had been sanctioned by many illustrious examples; his experiments were simple in their contrivance, and many of them very decisive in their results, and there does not appear to have been any circumstance in the mode of their publication or the situation of the author, which should have prevented them from meeting with due regard from the public. He appears indeed to have been pretty fairly estimated by his contemporaries, as we find his works referred to both by the English and French physiologists, and generally spoken of with a due degree of applause. See Lubbock, in London Med. Journ. v. i. p. 220..2. After an attentive perusal of his works, I consider myself justified in pronouncing Mayow to have been a man of extraordinary genius, and one who, on many points, far outstript the

After the period of Mayow our knowledge respecting the change which the air experiences by respiration was decidedly retrograde. Even Hales, who devoted so much of his time and attention to the investigation of the properties of air generally, and to the subject of respiration in particular, appeared to have no distinct conception of any proper chemical effect being produced by this function, a circumstance which is the more remarkable, as it would appear that he was not unacquainted with Mayow's discovery[1]. The result of Hales's experiments was, that the air receives the addition of a quantity of aqueous

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science of his age. Yet it must be confessed, on the other hand, that if, at one period, he was unjustly neglected, he has, at other times, been far too highly extolled. He saw the analogy of respiration to combustion, as well as the connexion which subsists between these processes and one of the constituents of the atmosphere, and he had also some conception of the modern doctrine of the formation of acids. But, I conceive, it would be no difficult task to prove, that on all these subjects, he had imperfect notions only, and that many of his ideas respecting the nitro-aerial spirit, as he terms it, are inapplicable to the modern oxygen, or to any other chemical principle. See note 30 of the Essay on Respiration.

In order to enable the reader to form a clear conception of the respective merits of our three countrymen, Boyle, Hooke, and Mayow, and of their claims to originality, I have subjoined the dates of some of their publications which give an account of the constitution of the atmosphere, and its effects on combustion, respiration, and other analogous processes. From these dates it may be fairly questioned, whether Mayow was always sufficiently candid in noticing the labours of his contemporaries. Boyle was born in 1627, and died in 1691; his principal works on the air were published between the years 1660 and 1675. Hooke was born in 1635, and died in 1702; he published the "Micrographia," in 1665, and the "Lampas," in 1677. It is in the former of these works that he first detailed his ideas respecting combustion; Obs. 16, entitled "of Charcoal and Burnt Vegetables," p. 100 et seq. He commences the Lampas by referring to the Micrographia as containing "his hypothesis of fire and flame, which", he adds, "has so far obtained, that many authors have since made use of it and asserted it;" p. 1. See also Waller's Life of Hooke prefixed to his posthumous works, and Ward's Lives of the Gresham Prof. p. 190. Mayow was born in 1645, published his tracts in 1674, and died in 1679. The substitution of the year 1697 for 1679, in Mr. Brande's Manual, as the date of Mayow's death, is, no doubt, an error of the press; compare p. 64 with p. 69. With regard to the estimate of Mayow's philosophical character, although, in many respects, I have the satisfaction of coinciding with Mr. Brande, in the high commendation which he bestows upon it, p. 68..79; yet I cannot but think that he, like Beddoes and Dr. Yeats, has over-rated his merits; at the same time, I admit, that in the contrivance and the execution of his experiments, he appears to have been more successful than any of his contemporaries. See Beddoes's "Chemical Experiments and Opinions" &c., and Yeats's "Observations" &c. Robison strongly advocates the merits of Hooke against those of Mayow; Black's Lectures, notes 13. v. i, p. 535..8 and note 31. vol. i. p. 553; see also Aikin, Gen. Biog., in loco. Fourcroy, I conceive, formed a more correct estimate of Mayow's merits; Encyc. Méth. art. Chim. t. iii. p. 390, and Ann. Chim. t. xxix. p. 42 et seq. Perhaps one of the most striking proofs of the neglect into which Mayow's works had fallen about the middle of the last century is the circumstance of his discoveries on air not having been alluded to by Pringle, in his address to the Royal Society, on presenting Priestley with the Copley Medal; Discourses, No. 1.

[1] Statical Essays, v. i. p. 234..6.

vapour and certain noxious effluvia, and has its elasticity diminished [1]. Boerhaave ingenuously confesses his ignorance on the subject[2], and Haller, after reviewing with his accustomed candour the opinions of his predecessors, coincides very nearly with the doctrine of Hales[3].

It was about this period that Black commenced his chemical discoveries, one of the most important and best established of which was, that a quantity of what was then termed fixed air, r as we now more correctly designate it, carbonic acid, is roduced in the lungs, and that the air of expiration essentially iffers from that of inspiration by the addition of this substance[4]. his may be justly regarded as the first step towards a correct iew of the nature of the function of respiration, and the brilliant areer of discovery respecting the gaseous bodies, upon which riestley was now entering, most fortunately contributed still urther to advance our knowledge[5]. After he had made us acquainted with the nature and properties of oxygen, and shown hat it is one of the constituents of the atmosphere, he instituted n extensive train of experiments for the purpose of ascertaining he state of the air under different circumstances in relation to he quantity of oxygen which it contains, and he found that espiration, in the same manner with combustion and other nalogous operations, diminishes the proportion of oxygen, and reduces the air to the state, which, in conformity with the hypothesis at that time generally adopted, was termed phlogisticated. Priestley's conclusion therefore was, that respiration deprives he air of a portion of its oxygen, and imparts to it a quantity f phlogiston and aqueous vapour[6].

Shortly after the publication of Priestley's experiments, the ubject of respiration was taken up by Lavoisier. Although his philosopher is perhaps less distinguished for his own discoveries, than for the acuteness which he displayed in generallizing and reasoning upon the discoveries of others, yet there is no individual to whom the science of chemistry is more indebted, from the minute attention to statical accuracy of which he showed the first example, and in which the experiments of the chemists of the present day so much exceed those of the most celebrated of their predecessors. He examined with his accustomed address the conclusion of Priestley; he agrees with him respecting the important fact of the consumption of oxygen, but he points out a distinction, which had been hitherto disregarded, between the various processes that had been classed together under the title of phlogistic; that some of them, as, for example, the calcination of metals, consisted merely in the abstraction of oxygen from the air, whereas in others, among which he classes respiration, we have not only the abstraction of oxygen, but the production of carbonic acid. His general conclusions therefore are, that the effect of respiration is to abstract oxygen and to produce carbonic acid; he further conceived that the total volume of the air is diminished, while the nitrogen he supposes is not affected by the process, but that it simply serves the purpose of diluting the oxygen[1]. Lavoisier, in this memoir, does not notice the aqueous vapour which is discharged from the lungs, and which he afterwards made the subject of an elaborate train of experiments; we may therefore presume that, at this period, he considered it as merely diffused through the air by evaporation from the trachea and air vesicles, and not as formed by the operation of any chemical affinity.

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[1] Statical Essays, v. ii. p. 94, 100 et alibi.

[2] Prælect. t. ii. § 203 cum notis; t. v. § 625 cum notis et alibi.

[3] Not. 40. ad Boer. Præl. t. v. § 625; El. Phys. viii. 3. 11; viii. 5. 19, 0 t alibi.

[4] Black's Lectures by Robison, v. ii. p. 87, 100, 204. Haller's third olume, which contains an account of respiration, was published in 1760; ee a list of his publications, with their dates, prefixed to his Op. Min. p. xix. lack appears to have made his discovery about 1757, and we may presume hat he shortly after announced it in his lectures.

[5] Priestley's first account of his experiments and discoveries on the gases was read to the Royal Society in March 1772; and from the prodigious xtent and originality of the matter which it contains, it necessarily follows, hat his attention must have been, for some time, occupied with the subject, while from the peculiarly ingenuous and open disposition which he always anifested, we can have no doubt that, from time to time, he informed his umerous friends and correspondents of the success of his investigations.

[6] Phil. Trans. for 1776, p. 147. In the first of his original volumes on air, ublished in 1774, he announced that air which had been respired was affected n the same manner as by putrefaction, and that one use of the blood is to arry off putrid matter from the living body, p. 78.

The doctrine of Lavoisier concerning the chemical effects of respiration on the air was generally acquiesced in by the contemporary physiologists, at least in its more important parts, so that since his time, the attention has been principally directed to estimate the amount of the various changes which he supposed to take place. These I shall now therefore proceed to examine in succession, under the five following heads; the quantity of oxygen abstracted from the air, the carbonic acid formed, whether the volume of the air be affected by respiration, whether the nitrogen be affected, and lastly, I shall inquire into the origin and quantity of the aqueous vapour.

With respect to the first of these subjects, the abstraction of oxygen, besides the inquiry respecting the absolute quantity of it which disappears, there is another point which must be attended to, what proportion of it is it necessary for air to contain in order to render it fit for the support of life. Although it might have been supposed that these questions would have been easily answered, by a sufficient number of well directed experiments, it appears in fact that this is not the case, for we find that there are many circumstances connected with the living body, and especially influencing the action of the lungs, which it is very difficult either to guard against or duly to appreciate. The capacity of existing in air of a certain standard, or of abstracting oxygen from the mass of air, depends very much upon the peculiar constitution of the animal as to its temperature and other functions, and still more, upon the structure of its lungs and organs of circulation. As a general principle, it appears that animals which possess the highest temperature, whose lungs have their air cells the most minutely sub-divided, and the whole of whose blood passes through the lungs at each circulation, consume most oxygen, require a greater proportion of it in the air which they breathe, and possess the power of deoxidating it in a less degree. This will appear to be the case if we compare together the four great divisions of birds, mammalia, amphibia, and mollusca : birds, whose temperature is upon the average about 104°, consume more oxygen, require a purer air, and less completely deoxidate it than the mammalia, whose temperature is about 98°, while the mammalia, on all these points, much exceed the cold-blooded animals[1]. As far as we know, the power of all the animals belonging to the great division of the mammalia is nearly the same, and is similar to that of the human species. This will, of course, be the main object of our investigation, although I may occasionally refer to the other classes of animals, either for the purpose of illustration, or where the facts or experiments are themselves so important, as to be an object of attention on their own account.

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[1] Mém. Acad. Scien. pour 1777, p. 185 et seq. It is impossible to omit noticing that in this paper Lavoisier makes no mention of Dr. Black, although he had publicly taught in his lectures for twenty years, that carbonic acid is produced by respiration. Nor is Black mentioned by Morveau in his account of the successive discoveries that had been made on the subject of respiration, given in the Encyc. Méth. Chimie, art. "Air," in the sect. on respiration, under the head "Acide Méphitique;" this was written in 1786.

In forming an estimate of the absolute amount of oxygen consumed in respiration, there are many circumstances which tend to interfere with the results, and to render it difficult to obtain a correct estimate. Besides various points connected with the management of the apparatus, and the nicety of the manipulations, there is one of a physiological nature, which was first noticed by Crawford, was afterwards more fully developed by Jurine and Lavoisier, and has been lately still farther investigated by Dr. Prout and Dr. Edwards. I refer to the fact, that the respiration of the same individual affects the air in a very different degree, according to the state of the functions and the system at large. The points which have been more particularly attended to are the temperature of the air respired and of the animal, the degree of muscular exertion, the state of the digestive organs, the presence of febrile action, and the hour of the day, to which some others may be added, probably of less moment. It is hence obvious, that all which we can accomplish in experiments of this kind is to obtain an average result, and even this subject to many modifications, which must materially diminish the certainty of any conclusions that we form concerning it.

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[1] Chaptal's Chem. v. i. p. 128 et seq.; Higgins's Minutes, p. 158, 9; Vauquelin, Ann. Chim. t. xii. p. 273 et seq.; Edwards, de l'Influence &c. part iv. c. 7.

The first experiments on the actual amount of oxygen consumed in respiration, which were performed with any degree of accuracy, were two of Lavoisier's, in which he confined a guinea pig in pure oxygen, and afterwards in air containing a much greater proportion of it than exists in the atmosphere[1]. The gas was confined by mercury; in the first, the quantity was 248 cubic inches, and the experiment was continued during an hour and a quarter; in the second, which lasted for an hour and a half, he employed 1728 cubic inches; the animal was then withdrawn, the bulk of the gas was noticed before and after the experiment; pure potash was introduced, in order to absorb the carbonic acid, and the bulk of the gas was again noticed. In the first case, 100 parts of air were diminished 3.5 per cent., and of the 96.5 that remained, 16.5 were absorbed by potash, leaving a residuum of 80 per cent. In the second experiment, the diminution, in the first instance, was from 100 parts to 96.82, which after the action of the potash, was farther reduced to 77.82. These experiments were probably performed with great care, and are highly interesting, as being among the first which this distinguished chemist performed on the subject of respiration; but, owing to the peculiar circumstances under which they were made, they can scarcely be applied to what takes place in natural respiration; in the commencement of the experiment the animal was respiring an air considerably purer than that of the atmosphere, while, towards the conclusion, the reverse would take place.

In order to obviate this objection, Menzies proceeded upon the plan of examining the state of the air after it had been only once respired, when, by ascertaining what proportion of oxygen was abstracted, he calculated the total quantity which would be consumed in a given time. The result of Menzies's experiments was, that air, by being once respired, had $\frac{1}{20}$ part of its bulk converted into carbonic acid, from the known composition of which he estimated, that the quantity of oxygen consumed by a man in 24 hours would be 51840 cubic inches, equal to 17496 grs.[2] This estimate of Menzies may be regarded as a considerable approximation to the truth, yet it depends upon several data of which only a vague average had been formed. It however deserves to be recorded, as the first experiment in which an attempt was made to estimate the amount of oxygen consumed by the human subject.

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[1] Mém. Acad. Sc. pour 1780, p. 401..8; Mém. Soc. Roy. Méd. pour 1782, 3, p. 572..4; Ann. Chim. t. v. p. 261 et seq. This latter paper is written by Seguin, and contains a copious extract from the second of the above memoirs.

[2] Essay, p. 50; the weights as given by Menzies are 22865.5 grs. depending probably upon his having adopted a different number for the specific gravity of oxygen; I have followed the estimate given by Mr. Brande; Manual, v. i. p. 316.

An elaborate train of investigation was conducted by Lavoisier, in conjunction with Seguin, upon whom the experiments were performed, in which an apparatus was employed more complete, and on a larger scale than any which had hitherto been introduced into physiological inquiries. One principal object of the experiments was to point out the effect which different states of the system produce upon the respiration, and they prove them to be much more considerable than had been previously suspected, while they show that we can do no more than obtain an average result, and that after making many allowances for circumstances, the exact amount of which we are unable to appreciate. The general conclusion of Lavoisier was, that the average consumption of oxygen, during 24 hours, is 46048 cubic inches, equal to 15541 grs.[1]

Lavoisier was still continuing his researches, and had constructed a new and more perfect apparatus, with which he had already performed some experiments, when he fell a victim to the barbarous fury of Robespierre. The results of his latest operations have been fortunately preserved, and are recorded by La Place; although on some points they differ from the former, yet they nearly agree with them in the amount of oxygen consumed in 24 hours, which they state to be 15592·5 grs.[2]

Since the death of Lavoisier this point has been made the subject of experiment by Sir H. Davy, who by adopting a plan nearly similar to that of Menzies, which consisted in ascertaining what proportion of its oxygen the air lost by being once respired, estimated the quantity of oxygen consumed in a minute at 31·6 cubic inches, which will give us 45504 cubic inches, or 15837 grs. in 24 hours [1].

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[1] An account of the operations of Lavoisier and Seguin is contained in two papers in the Memoirs of the Academy of Sciences for the years 1789 and 1790, which give a detail of the results of two distinct sets of experiments, p. 566 et seq. and p. 601 et seq. But notwithstanding the apparent attention to accuracy in every part of the processes, the details do not, in all cases, correspond, nor is any reason assigned for their discrepancy. The weight of oxygen consumed is given very nearly the same in both the papers, in the first being 19060 French grains, in the second 19090. But the volumes are stated very differently; the first being 24 and the latter something more than 22 Paris feet. This difference, we may presume, depends upon different estimates of the specific gravity of oxygen. To make the numbers correspond, we must take the weight of 100 cubic inches of oxygen at 31·572 grs. in the first case, and in the second at 34 grs., after reducing the French weights and measures to the English standard; I have adopted the volume mentioned in the second experiment, as we may presume this was supposed to be a correction of the former. It must be remembered that in these experiments, the measures are the direct results, and the weights estimated from the measures.

[2] Suppl. to Enc. Brit. v. ii. p. 594.

In the Philosophical Transactions for the years 1808 and 1809, we have an account of a series of experiments which were performed by Messrs. Allen and Pepys, on the changes produced in atmospheric air and in oxygen by respiration. With respect to the correctness of the detail, and the minute accuracy with which the various chemical processes were conducted, it is impossible to speak too highly, but I think it may be questioned, whether the operators were equally fortunate in some of the mechanical parts. The apparatus consisted of a large water gazometer and a mercurial gazometer, which were so arranged, "that the inspirations were made from the water gazometer and the expirations from the mercurial gazometer alternately;" while, in order to keep the portions of gas separate from each other, it was necessary for the operator to open two cocks during each act of respiration, one connected with each of the gazometers. The individual on whom the experiment was performed began by making a complete expiration, and after having continued the process as long as was thought necessary, he concluded, by emptying the lungs as completely as possible [2].

It must, I apprehend, be sufficiently obvious, that upon this plan the respiration was not carried on in the natural manner, and the observations of the experimentalists, although intended to convey the contrary impression, prove that this was the case. They give us the result of ten experiments, the longest of which lasted eleven minutes, and in each of which between 3 and 4000 cubic inches of atmospheric air were respired. They remark that "the operator was scarcely fatigued, and his pulse not raised more than about one beat in a minute; the respirations, however, were deeper and fewer than natural, amounting only to about 58 in 11 minutes, whereas, from repeated observations at different and distant times, he makes 19 in a minute." [3] I conceive it will be thought that a mode of respiring, which could produce such considerable effects in the short period of 11 minutes, must have been very far from exhibiting the state of the lungs in their natural action [4]. As far as respects the present question, the absolute amount of oxygen consumed in a given time, it was found that air, after having been once respired, contained only 12·5 per cent. of oxygen, 8·5 per cent. having been consumed and its place supplied by an equal bulk of carbonic acid [5]; hence we deduce that the average quantity in 24 hours is, under ordinary circumstances, 39534 cubic inches[1], equal to 13343 grs. It may appear somewhat remarkable, that, although according to the method in which these experiments were performed, the lungs were more completely emptied than in natural respiration, and therefore that the change produced in the air should have been proportionally greater, yet the quantity indicated is less than in the experiments either of Menzies, Lavoisier, or Davy[2].

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[1] Researches, p. 431..4.

[2] Phil. Trans. for 1808, p. 250, 2.

[3] Phil. Trans. for 1808, p. 253.

[4] As a proof that the mechanical action of the lungs was not performed in a perfectly natural state, I may remark that the quantity of air taken in at a single respiration was no more than 16½ cubic inches, p. 256.

[5] P. 255, 279. This remark only applies to the respiration of atmospheric

Since the experiments of Messrs. Allen and Pepys, the chemical effects of respiration have been very minutely examined by Dr. Edwards, and he has given us the result of his investigations in a treatise of uncommon merit, whether we regard the decisive nature of the experiments, the clearness and simplicity with which they are narrated, or the extensive information which is diplayed on all points connected with the animal œconomy[3]. Among the most valuable facts of Dr. Edwards's essay is the conclusion which we are led to form respecting the difference in the effect of the respiration of the different animals, and of the same animal under different circumstances. This, while it increases the difficulty of ascertaining the absolute amount of the changes that are produced in the air, shows us that many of the discrepancies which had been noticed between the results of former experiments, depend not so much upon any inaccuracy in the process, as upon an actual difference in the effect produced, of which the experimentalists were not aware. Although we shall have occasion, in numerous instances, to profit by Dr. Edwards's labours, he does not give us any information respecting the immediate subject of our pre-

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air under ordinary circumstances; when the same air was respired as frequently as possible, until symptoms of suffocation were produced, the quantity of oxygen absorbed is 10 per cent. p. 262.

[1] Phil. Trans. for 1808, p. 265, 6.

[2] Consumption of oxygen in 24 hours,

According to Menzies	51840 cubic inches,	or 17496 grs.
Lavoisier	46048	or 15541 grs.
Davy	45504	or 15337 grs.
Allen and Pepys	39534	or 13343 grs.

But this apparent discrepancy will probably be removed by the consideration, that when these philosophers performed their experiments, it was generally supposed that the atmosphere consisted of ·27 oxygen and ·73 nitrogen, and as they would no doubt employ the same method of analysis in all cases, we must diminish the proportion of oxygen in all the steps of the calculation. The 27 per cent. of oxygen before respiration, which was supposed to be diminished to 22·5, leaving a deficiency of 4·5, will therefore become 21 before, and 17·4 after respiration, leaving a deficiency of 3·6 only; if we apply this scale of proportion to Sir H. Davy's estimate, it will reduce it from 15337 grs., to 12272 grs., leaving, as might be expected, a superiority in quantity to Messrs. Allen and Pepys's experiments; and the same remark, of course, applies to the others. We have some judicious observations on these experiments by Dr. Apjohn, Dubl. Hosp. Rep. v. v. p. 525 et seq., and Ed. Med. Journ. v. xxxv. p. 205 et seq.

[3] De l'Influence des agens physiques sur la Vie.

sent inquiry, the absolute quantity of oxygen consumed by a man in a given time[1].

With respect to the question which was alluded to above, what proportion of oxygen there must be in air, in order to render it fit for the support of life? we have no facts that can lead us to any very decisive conclusion. As a general principle, we find that animals enclosed in a given portion of air die long before the consumption of the whole of the oxygen[2], and that the fatal effect, in this case, immediately depends not upon the absence of oxygen, but upon the presence of the carbonic acid which is substituted in its place. The only experiment that I have met with, on which we can depend, as giving us any accurate information on this point, is one of Lavoisier's, in which he found, that when the carbonic acid was carefully removed by caustic potash, as fast as it was formed, a guinea pig could live, without any apparent inconvenience, in air that contained only 6·66 per cent. of oxygen, and even when the proportion was still farther diminished, the only obvious effect produced upon the animal was a degree of drowsiness[3]. As the temperature of the guinea pig, and the structure of its lungs, is the same with that of man, it may be presumed that, under the same circumstances, human life might be supported by air of similar composition; but this, we must bear in mind, could probably only be the case under the most favourable circumstances, where every extraordinary source of expenditure was guarded against, or did not exist[4].

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[1] Thenard informs us, in a general way, and without specifying any particular authority, that air, after being once respired, contains from 18 to 19 per cent. of oxygen, a quantity which is greater than what has been assigned by any of the experimentalists who have minutely attended to the subject; Chimie, t. iii. p. 666.

[2] From this remark we must except many of the lower tribes of animals; Vauquelin found that certain species of limax and helix have the power of completely deoxidizing the air in which they are confined; Ann. Chim. t. xii. p. 278 et seq. Spallanzani also obtained the same results with various kinds of worms; Mém. sur la Respiration, p. 62.

[3] Mém. Acad. Sc. pour 1789, p. 573. Messrs. Allen and Pepys also witnessed the same effect in a similar kind of experiment, in which a guinea pig was inclosed in a portion of air consisting of a mixture of oxygen and hydrogen; Phil. Trans. for 1809, p. 424, 428.

[4] Halley, in his proposal for improving the construction of the diving bell, Phil. Trans. v. xxix. p. 492 et seq., observes that a gallon of air will become unfit for respiration in a little more than a minute. Lavoisier, Ann. Chim. t. v. p. 261, informs us that a man cannot live more than an hour in 5 cubic feet of air. Sir G. Blane gives us an important practical observation on this subject, which is deduced from very extensive observation; that, in calculating for the arrangements of a hospital, each individual should be allowed a space of 600 cubic feet, below which it will be found impossible to maintain the requisite purity of the air; Med. Chir. Trans. v. iv. p. 115. Dr. Edwards has given us the result of his experiments on the extreme limits of the rarefaction of the air, when it appears incapable of supporting life for any perceptible length of time; this for birds he found to be a pressure of a little more

The next point which we proposed to examine is the quantity of carbonic acid produced by respiration. The fact of its presence in air that has been respired, I have already mentioned, as one of the most interesting of the discoveries of Black, but it does not appear that he made any attempt to ascertain its quantity. Lavoisier, in his first memoir on respiration, informs us that the air in which an animal had expired contains $\frac{1}{6}$ of its bulk of carbonic acid[1], while in the experiments on the guinea pig which was confined in pure oxygen, it appeared to constitute about $\frac{1}{5}$ of the total bulk of the air, when it was reduced to a state in which it was no longer fit for the support of life[2]. These experiments, however, do not give us any information concerning the quantity which is produced under ordinary circumstances.

The solution of this problem appears to have been first attempted by Jurine; he endeavoured to ascertain what proportion of the air that is emitted from the lungs in natural respiration consists of carbonic acid, and this he estimates at about $\frac{1}{10}$ or $\frac{1}{12}$[3], a quantity very much beyond the truth. The same kind of calculation was afterwards made by Menzies, although with a very different result, for he conceived that the quantity of carbonic acid in air which had been once respired is no more than $\frac{1}{20}$ of its bulk; from the estimate which he had formed of the total quantity of air respired in 24 hours, he deduced the amount of carbonic acid formed to be 51840 cubic inches[4], equal to 24105·6 grs.

The quantity of carbonic acid produced in the lungs was one of the points to which Lavoisier and Seguin particularly directed their attention in the experiments which they performed in conjunction; but, notwithstanding the nature of the apparatus, and the minute attention which they appear to have paid to every circumstance which might ensure accuracy, the estimates, in the different sets of experiments, differ considerably from each. In the first set, published in the memoirs of the Academy of Sciences for the year 1789, the average quantity of carbonic acid in 24 hours is stated to be 17720·89 grs.[5], in the following year,

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than 5 inches, and for guinea pigs of a little more than 3½; De l'Influence &c. p. 495.

[1] Mém. Acad. pour 1777, p. 189. Crawford informs us that when an animal is confined over water about $\frac{1}{3}$ of the air is absorbed, which must have been carbonic acid; and that the same diminution takes place over mercury if potash be introduced; On Animal Heat, p. 146, 7.

[2] Ann. Chim. t. v. p. 261 et seq.

[3] Encyc. Méth., Art. Médecine, t. i. p. 494, 5. Jurine's experiments were published in 1787, and said to have been lately performed. Menzies's thesis was published in 1790; it is, however, extremely probable that the account of Jurine's experiments, which, I believe, first appeared in the Encyc. Méth., had not reached this country when Menzies was engaged in his investigations.

[4] Essay, p. 50.

[5] Mém. Acad. Sc. pour 1789, p. 577. In referring to the memoirs of the Academy of Sciences, it is necessary to observe that the dates prefixed to the volumes do not correspond to the actual date of publication, as in the present

the quantity is diminished by more than ½ to 8450·20 grs.[1], and according to La Place's account, it was reduced still lower in Lavoisier's last experiment, to 7550·4 grs.[2] Sir H. Davy, on the contrary, estimates the amount of carbonic acid in 24 hours at nearly the quantity which was announced by Lavoisier and Seguin in their first experiment, 17811·36 grs.[3], and Messrs. Allen and Pepys very nearly coincide with him in this estimate[4].

I have already stated, that the absolute quantity of oxygen consumed, and of carbonic acid generated by respiration, is materially influenced by various causes which affect the state of the constitution and the functions, and it will now be necessary for us to attend to them a little more minutely. Priestley, in an early stage of his experiments, noticed the different effect which different animals of the same species produced upon air, principally as connected with their age or apparent vigour[5]. Crawford extended his observations to temperature, and showed, by actual experiment, that the quantity of air which an animal deoxidates in a given time is less in proportion to the elevation of the temperature[6]. Jurine appears to have performed a number of experiments on this subject, and to have ascertained that different states of the constitution, in the same individual, influence the chemical change produced upon the air; that whatever quickens the circulation, as the process of digestion,

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instance, where the volume said to be for 1790, was not published until 1793. In tracing the progress of experimental research, were we not to pay attention to this circumstance, we might be misled as to the respective claims of contemporary writers to the priority of discovery. On this account, although we cannot suppose that any deception was intended, yet the plan of publication adopted by the Academy is objectionable.

[1] Mém. Acad. Sc. pour 1790, p. 609.

[2] Suppl. to Encyc. Brit. v. ii. p. 549. No explanation is offered by Lavoisier of the cause of these different results, but they may perhaps be reconciled by some circumstances which will be mentioned hereafter.

[3] Researches, p. 434. It is stated that in natural respiration 26·6 cubic inches of carbonic acid are formed in a minute, which will be 38304 inches in 24 hours. Dr. Dalton estimates the carbonic acid produced in 24 hours at 2·8 lbs., or 16128 grs. Manch. Mem. 2d ser. v. ii. p. 27.

[4] Phil. Trans. for 1808, p. 256. In their 11th experiment, which they regard as the one that is the most to be depended upon, they found 26.55 cubic inches produced per minute, or 38232 in 24 hours. They always found that air which had been once respired contained about 8·5 per cent. of carbonic acid; p. 255; and it is remarkable that whether the respiration proceeded at a quicker or slower rate, the proportion of carbonic acid remained the same; p. 257. When the same air was respired for a number of times, the maximum of carbonic acid was 10 per cent.; p. 262. Dr. Prout has given us a valuable synopsis of the results of the different experiments which have been performed for the purpose of ascertaining the quantity of carbonic acid produced; it appears to vary from 10 to 3·45 per cent. of the air inspired; Ann. Phil. v. ii. p. 333; but before we can draw any correct deduction from them, it would be necessary to know the bulk of a single inspiration, and the number of inspirations performed in a given time, in each of the cases.

[5] Experiments on Air, (1st series,) v. i. p. 72 et alibi.

[6] On Animal Heat, p. 311..5; 387, 8.

exercise, or the hot stage of fever, increases the quantity of carbonic acid, while, on the contrary, it is diminished by the cold stage of fever or by bleeding[1]. The experiments performed by Lavoisier, in conjunction with Seguin, confirmed the results of Jurine, and afford much important information respecting the absolute amount of the effect produced under the various circumstances of temperature, exercise, and the process of digestion. As a standard to which the rest of the experiments might be referred, they state that a man at rest, with the stomach empty, and at the temperature of 82° Fah., in the space of an hour, consumes 1210 French cubic inches of oxygen: If the temperature be lowered to 57°, he will consume 1344 inches; during the process of digestion the quantity will be increased to between 18 and 1900 inches; by violent exercise, the stomach remaining empty, it was farther augmented to 3200 inches, and after taking food it was still farther augmented to 4600 inches[2].

A series of very curious experiments, which bear upon this point, was performed by Dr. Prout in the years 1813 and 1814. He discovered the remarkable fact, that the amount of carbonic acid discharged from the lungs appeared to be influenced by the hour of the day, and that this occurred in a very uniform and regular manner. The greatest quantity of carbonic acid discharged from the lungs in a given time was about noon, or more generally, between 11 a. m. and 1 p. m.; from this hour the production of carbonic acid diminishes until it has reached its minimum quantity, at about 8½ in the evening; it remains in the same state until about 3½ in the morning, when it suddenly begins to increase[3]. The maximum quantity he found to be 4·1 per cent., and the minimum 3·3 per cent. of the oxygen inspired; the mean quantity given off in 24 hours is stated to be 3·45 per cent.[4]

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[1] Encyc. Méth. Art. "Médecine," t. i. p. 494.

[2] Mém. Acad. Sc. pour 1789, p. 575. It is impossible to avoid noticing and regretting, that Lavoisier, in this paper, has made no mention of the experiments of Crawford, although they were published 14 years previous to the date of his own memoir.

[3] Can these curious results of Dr. Prout's be explained by the observations that have been made by Dr. Edwards on the transpiration, which he found to be increased during sleep, and also to be greater in the forenoon than towards the evening; De l'Influence &c. p. 316, 321 et alibi.

[4] Ann. Phil. v. ii. p. 330; and v. iv. p. 331..4. If we assume the quantity of air respired in the diurnal period to be 1152000 cubic inches, it will give us 39744 inches of carbonic acid produced, an estimate which approaches very nearly to that of Messrs. Allen and Pepys, and to the corrected estimates of Menzies, Lavoisier, and Sir H. Davy. We must remark here upon the different proportion of carbonic acid in the experiments of Messrs. Allen and Pepys, and in those of Dr. Prout, the first being 8·5 per cent., the latter only 3·45. Yet the total quantity of carbonic acid produced is very nearly the same; this depends probably upon the respirations in the former experiments being considerably less frequent, and therefore, we may presume, the inspired air would be more completely mixed with the contents of the lungs, and also from the bulk of a single inspiration being so small in

Another singular circumstance noticed by Dr. Prout is, that whenever, from any cause, the production of carbonic acid has been either increased or diminished above and below the usual maximum or minimum quantity, it will be found to be inversely diminished or increased in an equal proportion during a subsequent diurnal period. We have the results of a number of experiments, placed in the tabular form, which seem fully to establish the fact; and by comparing the circumstances under which the experiments were made, however difficult it may be to conceive of the mode of operation, it would appear that there is no other assignable cause to which the difference can be attributed[1]. He afterwards examined into the effect of other circumstances upon the production of carbonic acid, and we learn that exercise, if long continued and violent, always diminishes the quantity of carbonic acid produced; that fasting has the same effect; also alcohol taken into the stomach under any form[2], probably also sleep, and certainly the depressing passions, or even strong mental emotions of any kind, while no perceptible effect could be ascribed to a change of temperature. It appears, indeed, that whilst almost every variation in the system decreases the quantity of carbonic acid, there are very few circumstances which were found to increase it, and those only in a slight degree[3].

About the same time that Dr. Prout was engaged in his inquiry, Dr. Fyfe entered upon a set of experiments respecting the effects which certain substances taken into the stomach produce upon the generation of carbonic acid: the following are the most important of his results. He fixes upon 8·5 per cent. as the quantity discharged from his lungs under ordinary circumstances[4]; he found that it was much diminished by wine, that it was still farther reduced to nearly half by vegetable diet, and to nearly one-third by a course of mercury[5]. The apparatus which Dr. Fyfe employed would not allow of his observing the diurnal variations; and to the same cause we may probably attribute the large quantity of carbonic acid which he supposed to be produced in natural respiration, as it would be

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the individual upon whom they were performed; these circumstances may be partly explained by the nature of the apparatus which they employed.

[1] Ann. Phil. v. ii. p. 330, 338..0.

[2] This appears to be the fair inference from Beddoes's experiments, although his hypothesis inclined him to the contrary conclusion. The experiments generally tend to show that muscular action promotes the consumption of oxygen; On Fact. Airs, part 1. § 7. p 23..26.

[3] Ann. Phil. v. ii. p. 335..7; also v. xiii. p. 269, 0.

[4] This quantity agrees exactly with the estimate of Messrs. Allen and Pepys, while it very far exceeds that of Dr. Prout; but we cannot, from this datum alone, ascertain the total amount of carbonic acid formed in 24 hours, unless we knew the bulk of each inspiration, and the number of respirations, in a given time.

[5] Ann. Phil. v. iv. p. 334. I take my account from Dr. Prout's abstract, not having been able to procure Dr. Fyfe's work.

necessary for him to use a considerable effort in expelling the contents of the lungs; and we may suppose that he might be induced to empty them more completely, and would therefore discharge a portion of air in a more deoxidated state.

Upon comparing the experiments of Dr. Prout and Dr. Fyfe, which are the more interesting, as they agree in their most important results, although made without consent or co-operation, we must remark that the causes which were found to diminish the production of carbonic acid, are some of them the very same, which, according to Jurine and Lavoisier, produce the directly contrary effect, such as exercise, and the process of digestion, while there are others, as increased temperature, which, according to Crawford and Lavoisier, appeared to produce so powerful an influence upon the respiration, but which, from Dr. Prout's experiments, would seem to be totally ineffective[1]. It is difficult to conceive how Lavoisier and Seguin could have been mistaken in their results; and it is impossible to impeach the accuracy of Dr. Prout, so that we must consider these discrepancies as among those difficulties which we occasionally meet with, and are unable to explain, but which we may hope will be elucidated by future investigations[2].

The researches of Dr. Edwards were directed, among other objects, to the examination of the influence of certain circumstances, both external and internal, upon the respiration, especially as manifested by the production of carbonic acid, of which the most important were the effects of the different periods of life and of the seasons. With respect to the first of these points, he remarks, that from the apparent activity of the system in youth, it had been generally supposed, that all the functions must partake of this energy, and that the respiration would afford the additional supply of air which would seem to be required for the growth and development of the body. But upon making the experiment, the fact was found to be otherwise. It appeared, that with respect both to birds and to different species of the mammalia, the consumption of oxygen and the production of carbonic acid is less in the early periods of life; and it is probable that there is a progressive increase in these operations until the animal arrives at its mature state[3].

[1] Still further to increase the difficulty of arriving at any certain conclusion upon this point, we are informed by Spallanzani, that his experiments led him to conclude, that the quantity of oxygen consumed is in the direct ratio of the temperature; Mém. sur Respiration, p. 89, 185.

[2] We have some interesting experiments by Nysten, on the generation of carbonic acid in the lungs, as affected by different states of disease; the general result is, that, in obstructions of the lungs, the quantity is obviously diminished, while, on the contrary, it is increased in the state of acute fever; Recherches, p. 187 et seq.

[3] De l'Influence, &c. par. 3. c. 5; tab. 51 and 52, p. 633, 4. This circumstance was noticed by Boyle; he placed a kitten, a day old, in his air-pump, and found that it "continued three times longer in the exhausted receiver than other animals *of the same bigness* would probably have done;" Works,

In giving an account óf the effect of the different seasons upon the respiration, Dr. Edwards observes that there are several ways in which the air may be supposed to have its physical properties altered, so as to influence its action upon the lungs; of these the most obvious is its temperature, and, as connected with this, its density and its pressure. It appears to be pretty well established that the increase of temperature, and the consequent diminution of the density of the air, tend to diminish its consumption; although it may be somewhat doubtful what proportion of the effect is to be ascribed respectively to each of these changes. It still, however, remained to be examined whether the variation of the seasons produced any effect upon the respiration, as a vital function, independent of the physical state of the air; and this, upon making the experiment, was actually found to be the case. He confined birds in a limited quantity of air, under similar circumstances, at different seasons of the year, when it was found that, in the winter, the lungs possessed a greater capacity for decomposing the air than in the corresponding summer months; an effect which appeared to be brought about by the long continued action of a low temperature upon the constitution[1].

There is a circumstance respecting the two effects of respiration, which we have been contemplating, the consumption of oxygen and the production of carbonic acid, which we must examine in this place, whether they always take place to the same extent and are proportional to each other. As the question involves many important theoretical conclusions, it has been made a particular object of attention, yet there appears to be still some difficulty in arriving at an accurate conclusion concerning it. In most of the earlier experiments the quantity of oxygen consumed appeared to be greater than that of the carbonic acid produced[2], although the exact amount of this dif-

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v. iii. p. 360. Morozzo had found, in his experiments, that young animals lived longer in a given quantity of oxygen than adult animals of the same species; Journ. Phys. t. xxv. p. 103.

[1] De l'Influence &c. par. 3. ch. 6; tab. 53 and 54, p. 635, 6.

[2] It is worthy of observation, that this was the opinion of Priestley, on Air, v. iii. p. 378, 9; and it may be mentioned as one among many instances, where, although the nature of the apparatus which he employed, and the mode in which many of his experiments were performed, seemed but little calculated to obtain very correct results, yet the number of his experiments, and the ingenuity which he displayed in varying them, and comparing them with each other, had the effect of more than supplying the deficiency. In Lavoisier's earlier experiments he does not appear to have noticed this disproportion between the quantity of oxygen consumed and of carbonic acid produced; but he afterwards observed it, and ascertained its exact amount to be in the proportion of 229·5 to 54·75, or nearly $\frac{1}{4}$. This surplus quantity of oxygen he supposed to unite with hydrogen abstracted from the blood, and to form water; Mém. Soc. Roy. Méd. pour 1782, 3, p. 574. There seems to have been some confusion in the references that have been made to the papers of Lavoisier on Respiration, in consequence of the want of correspondence between the time when the papers were read, when they were

ference varied considerably in the different experiments. Lavoisier and Seguin supposed the oxygen consumed in 24 hours to be 15661·66 grs., while the oxygen necessary for the formation of the carbonic acid produced was no more than 12924 grs., or in the proportion of about 100 to 81·5. These numbers coincide almost exactly with what may be deduced from Sir H. Davy's experiments, the oxygen consumed being 15337 grs., while the carbonic acid produced was 17811·36 grs., which would contain 12824·18 grs. of oxygen, giving a proportion of 100 to 81·66.

But although these experiments appear to prove so decisively that there is a surplus quantity of oxygen consumed, more than is required for the production of the carbonic acid, and although they agree so nearly in the amount of this surplus, yet the conclusion which we might have been induced to draw from them appeared to be shaken, if not overthrown, by the experiments of Messrs. Allen and Pepys, whose researches, conducted as they were with such minute accuracy, were equally convincing in showing, that the oxygen which disappears is exactly replaced by an equal volume of carbonic acid, and that therefore the whole of it must have been employed in the formation of this acid[1].

The state of uncertainty in which we were thus left, the judgment being, as it were, almost equally poised between such high authorities, has been most happily removed by Dr. Edwards, who investigated this point, among others to which he directed his attention; and has shown, by a train of remarkably

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actually published, and the year inserted in the title of the volume as already mentioned, p. 347. Lavoisier and La Place published a joint essay on Heat, one section of which is on Respiration and Animal Temperature, in the Mem. of the Academy of Sciences for the year 1780; the essay was, however, not read until June, 1783, and the volume not printed until 1784. The paper of Lavoisier's referred to above, entitled, "Sur les alterations qui arrivent à l'air dans plusieurs circonstances où se trouvent les hommes réunis en société," was published in the Mem. of the Royal Medical Society for 1782 and 3; it was not read until February, 1785, nor printed until 1787. This 2d paper appears to be referred to by some writers as the memoir of 1783, while others speak of the former under that designation, the one adhering to the nominal date of the volume, the other to the actual time when it was read.

[1] Phil. Trans. for 1808, p. 279. In the experiments of Dr. Prout, it is always taken for granted that the oxygen consumed is exactly replaced by an equal volume of carbonic acid; Ann. Phil. v. ii. p. 330 et alibi. The same conclusion is also formed by Dr. Ellis, after a very full and candid examination of all that had been done on this subject by preceding physiologists; Inquiry, p. 116 et seq. Dr. Henry also regards the experiments of Messrs. Allen and Pepys sufficiently decisive to prove that the amount of oxygen consumed exactly coincides with the carbonic acid produced; Elements, v. ii. p. 403. Magendie expresses himself very decisively upon the subject; "Quant au volume de l'oxygène en *déficit*, comparé au volume de l'acide carbonique expiré, je dois dire que toutes nos expériences, sans en excepter aucune, sont entièrement d'accord avec celles des chimistes Anglais: nous avons constamment vu l'oxygène disparu pendant l'acte respiratoire représenté exactement par l'acide carbonique de l'air expiré;" Sur la Trans. Pulm. p. 9.

well devised experiments, that the discordancies did not depend so much upon the inaccuracy of the operator, as upon an essential difference in the results. He begins by remarking that experiments of this kind are better performed on small animals, which can be immersed in a large quantity of air, than on man, where the air must be renewed at each respiration[1]. When experiments were conducted in this manner upon various warm-blooded, vertebrated animals, which with respect to their respiration, possess a close analogy with man, it has been found that there is generally a diminution of the bulk of the air, but that the quantity of the diminution varied much in the different experiments, and that on some occasions it did not exist.

As it is ascertained that when oxygen is converted into carbonic acid, the bulk of the gas is not changed, the diminution in these cases is presumed to depend upon the absorption of oxygen, or upon the quantity of it which is consumed being greater than what is necessary for the production of the carbonic acid. In three experiments upon young dogs, the whole quantity of air employed was 91·542 cubic inches; the process was carried on over mercury, and lasted for five hours: the average results were that the gas was diminished by 5·675 cubic inches, or about $\frac{1}{16}$th of its volume, and that 10.9 cubic inches of carbonic acid were produced[2]. Here, therefore, the total quantity of oxygen consumed is 16·575, in proportion to that employed in the formation of the carbonic acid as 16.575 is to 10·9, or as 100 to 60·66. In another series of 10 experiments, in which yellow-hammers were employed, and remained, each of them, for 15 minutes in 100·6 cubic inches of air, the total consumption of oxygen was upon the average 4·437 inches, while the carbonic acid produced was 3·65, or in the proportion of 100 to 82·26[3]. Dr. Edwards's general conclusion is, that the proportion of oxygen consumed to that employed in the production of carbonic acid, varies from more than one-third of the volume of carbonic acid to almost nothing; that the variation depends upon the species of the animal employed, upon its age, or some peculiarity in its constitution, and also that it varies considerably in the same individual at different times[4]. The general fact, therefore, of the surplus quantity of oxygen, in a great majority of cases, is abundantly proved, while we may fairly conclude that the source of error in the experiments of Messrs. Allen and Pepys depended upon the difficulty of bringing the lungs into the same state of distention at the beginning and the end of the experiment, and upon the results being complicated by the gas originally contained in the lungs[5].

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[1] De l'Influence &c. note to p. 408.

[2] Ibid. p. 410 et seq.

[3] Ibid. p. 415.

[4] Ibid. p. 418.

[5] Dr. Ellis, in his "Farther Inquiries," has offered many valuable observations on the mechanical part of Messrs. Allen and Pepys's experiments, p.

The third point which I proposed to examine, whether the bulk of the air be diminished by respiration, is essentially connected with the question which has been discussed above, and indeed almost resolves itself into the same inquiry. All the earlier physiologists supposed the diminution to take place, and they accounted for it upon the idea, that the air had lost part of its elasticity, or, as they termed it its spring[1]. Mayow appears to have been the first who attempted to ascertain the exact amount of the diminution; he estimated it at $\frac{1}{14}$[2], while Hales, in different experiments, found it to vary from $\frac{1}{13}$ to $\frac{1}{30}$[3]. But all these statements are much over-rated; a circumstance that depends, in part at least, upon the air of expiration being passed through, and confined over water, which would necessarily absorb a part of the carbonic acid[4]. Lavoisier, Goodwyn, and Sir H. Davy, however, in their more correct experiments, although they found the diminution to be much less, did not fail to recognize it. Lavoisier, in his Memoir of 1777, fixes the amount at $\frac{1}{60}$[5], Goodwyn obtained the same

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280 et seq. Nysten and Spallanzani, from their experiments, the former on the human subject; Recherches, p. 214; the latter on various species of the mollusca; Mémoires, p. 66 et alibi; come to the same conclusion, respecting the want of correspondence between the oxygen and carbonic acid. Legallois also found this to be the case in his experiments, and although they were performed under particular circumstances, and for a specific object, yet they are generally applicable, so far as this question is concerned; Ann. de Chim. et Phys. t. iv. p. 115. Dr. Thomson, Chem. vol. iv. p. 619, and Dr. Dalton, Manch. Mem. vol. ii. 2d ser. p. 25, likewise obtained a surplus quantity of oxygen, although, from certain considerations, they were induced to ascribe this difference to incidental circumstances, not essentially connected with respiration; see Dalton, p. 36.

The specific gravity of the serum of arterial blood has been generally found to be less than that of venous; in the experiments of Dr. Davy, the proportion was as 1047 to 1050; Phil. Trans. for 1814, p. 591. It has been supposed that, as during the change from the venous to the arterial state, there is an absorption of oxygen and a discharge of carbonic acid, (a point which will be more fully investigated hereafter,) we might conclude from this change of the specific gravity, that the quantity of gaseous matter absorbed is greater than that discharged. This observation proceeds upon the idea, which is probably correct, that the water which is discharged from the lungs does not immediately proceed from the blood in the pulmonary vessels, but that it is the result of secretion. But it also takes for granted, that the blood loses nothing by serous transudation; for if we conceive that this process takes place as the blood passes through the lungs, it accounts for the difference in the specific gravity of the two kinds of serum. It may be farther remarked upon this subject, that the specific gravity of the serum of different individuals differs at least as much as the difference indicated by Dr. Davy, between the arterial and venous blood; See Med. Chir. Tr. v. ii. 170, 363.

[1] Boyle informs us, that in one experiment, in which a mouse was confined in a portion of air over mercury, the volume of the air was not diminished; Works, v. iii. p. 380.

[2] Tract. p. 105.

[3] Stat. Essays, v. ii. p. 238, 320.

[4] Crawford found that when the air was confined over water, $\frac{1}{5}$th of the whole was absorbed; on Animal Heat, p. 146.

[5] In the experiments where the *guinea-pig* was confined in oxygen, the

result[1], Sir H. Davy found that air, which had once passed through the lungs, as is the case in ordinary respiration, suffered a diminution, which varied, in his different experiments, from $\frac{1}{70}$ to $\frac{1}{100}$[2]; Berthollet always found a diminution, although somewhat less in quantity[3], and the same result was obtained by Jurine[4] and by Spallanzani[5]. It might be supposed that these results afforded sufficient proof of the general fact of the diminution of the air by respiration, yet we have here, as on the former occasion, a great diversity of opinion. Crawford expressly states that he could not observe any diminution of the volume of the air, when the process was conducted over mercury[6]; it is not adverted to by Lavoisier and Seguin in their conjoined experiments, while Messrs. Allen and Pepys, in their elaborate researches, although they generally found a slight diminution, attributed it to some accidental circumstance connected with the management of the apparatus, or the nature of the process, and concluded that the bulk of the air is not essentially affected by respiration[7]. But the experiments of Dr. Edwards again most fortunately relieve our embarrassment, by showing us that the diminution really takes place in a great majority of cases, although, in such various degrees, that we are not able to reduce it to any fixed amount[8].

The fourth point that we proposed to examine respects the absorption of nitrogen; and on this we shall find as much diver-

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diminution was, in one case, $\frac{1}{31}$, and in the other $\frac{1}{32}$ of the volume of the air, the greater diminution in these experiments probably depending upon the increased consumption of oxygen, in consequence of the greater purity of the air employed; Mém. Acad. Sc. pour 1780. p. 401; Ann. Chim. t. v. p. 261; Mém. Soc. Roy. Méd. pour 1782, 3. p. 572.

[1] Connexion of Life &c. p. 51.

[2] Researches, p. 431..3.

[3] Mém. Soc. d'Arcueil, t. ii. p. 454..463.

[4] Mém. Soc. Roy. Méd. t. x. p. 25.

[5] Mém. sur la Respir. p. 102. Cuvier also states the fact of the diminution of the volume of air, and fixes it at $\frac{1}{30}$; it does not, however, appear, that he himself performed any experiments on this subject; Leçons d'Anatomie Comp. t. iv. p. 303. Dr. Thomson also found a diminution in the volume of the air, but it varied so much in his different experiments, that he was disposed to ascribe it to some accidental cause; Chem. v. iv. p. 617. Dr. Henry also agrees in the general fact; Elements, vol. i. p. 293.

[6] On Animal Heat, p. 147.

[7] The average diminution of the first ten experiments, p. 253, is not [illegible]; in the eleventh experiment, which they appear to regard as the most correct, it is about $\frac{1}{150}$, p. 254; but they state that the general average of all their experiments is about six parts in 1000, or $\frac{1}{166}$; p. 281. It may be presumed, from various expressions in Dr. Prout's papers, that he did not suppose there was any diminution of the volume of the air; Ann. Phil. vol. ii. p. 380 et alibi. Dr. Ellis likewise concludes that the volume of the air is not diminished, Inquiry, p. 99, 0; and Magendie's experiments led him to the same opinion; Mém. sur la Transp. Pulm. p. 7..9. Mr. Abernethy, on the contrary, supposes that the volume of the air is increased; Essays, p. 147.

[8] De l'Influence &c. p. 411 et alibi; it follows from the view which Dr. Edwards takes of the action of the lungs, that occasionally the bulk of the *air may* be increased by respiration, that at other times the bulk may be *unaffected, but that* in a majority of cases it will be diminished.

sity of opinion as on those that have already passed under our review. Lavoisier's experiments led him to conclude that the nitrogen is entirely passive in respiration, or that it serves no other purpose than to dilute the oxygen[1]; and Messrs. Allen and Pepys deduced the same conclusion from their experiments[2]. Priestley, on the contrary, supposed that there was an absorption of nitrogen[3]; but his experiments being performed in an early stage of the pneumatic chemistry, and with a less perfect apparatus, notwithstanding the confidence with which he maintained his opinion, the result was generally attributed to some accidental occurrence. Priestley's conclusion has, however, been powerfully confirmed by subsequent experiments; Sir H. Davy conceived it to be the case in his experiments, and he estimated that 5·2 cubic inches of nitrogen were absorbed per minute[4], or about 7488 inches in the 24 hours, a quantity equivalent to 2240 grains. The absorption of a portion of nitrogen is maintained by Cuvier[5], and has been proved by the researches of Dr. Henderson[6], and Prof. Pfaff[7], each of whom instituted a series of well-conducted experiments, which nearly coincided in their results. They both of them indicated a deficiency of nitrogen in the air of expiration, although they differed somewhat in the amount; Dr. Henderson supposing it to be less, and Prof. Pfaff more, than the estimate of Sir H. Davy. There are indeed certain points in which these experiments would appear not to be altogether unexceptionable, but they fully warrant the conclusion which the authors deduce from them with respect to the question now under consideration. It may be observed also that they both of them agree in supposing that the total bulk of the air is diminished by respiration[8]. To add to the apparent confusion of opinion on this subject, Jurine was induced to conclude, from the result of his experiments, that nitrogen is generated by respiration[9]; and the same result was obtained by Berthollet[10], and Nysten[11]. The experiments of

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[1] Mém. Acad. Sc. pour 1777. p. 193; and he still continued to support this opinion in his later essays; see Mém. pour 1789. p. 574; where he says that he has proved this by very decisive experiments.

[2] Phil. Trans. for 1808, p. 264 et alibi; and Phil. Trans. for 1809, p. 412..5. Messrs. Allen and Pepys conceive that in natural respiration the nitrogen is not affected, but that when the same portion of air is frequently respired, a quantity of nitrogen is discharged; Phil. Trans. for 1808, p. 263. The same effect was also produced by the respiration of pure oxygen, Phil. Trans. for 1809, p. 404, 415..421, 427. They remark, with justice, that an apparent increase in the proportion of nitrogen may depend upon the quantity of it which exists in the lungs before the experiment; they proved, however, by causing an animal to respire a mixture of oxygen and hydrogen, that, in certain cases at least, nitrogen is actually evolved, p. 420..427.

[3] On Air, v. iii. p. 380.

[4] Researches, p. 434. [5] Leçons d'Anat. Comp. t. iv. p. 303.

[6] Nicholson's Journ. v. vii. p. 40..5. [7] Ibid. v. xii. p. 249 et seq.

[8] Ibid. v. vii. p. 43, 4; and v. xii. p. 251, 2.

[9] Encyc. Méth. "Médecine," t. i. p. 493..7.

[10] Mém. d'Arcueil, t. ii. p. 454..463.

[11] Recherches, *p. 186, 215; from p. 187* to 200 is an account of his experiments.

Berthollet and Nysten seem to warrant the conclusion that is drawn from them; but with respect to those of Jurine, it may be doubted whether they are equally conclusive, as the nitrogen which he supposed to be generated may, with more probability, be referred to a portion of the residual air of the lungs mixed with the air of expiration.

The experiments which have been referred to above were performed either on man or on some of the warm-blooded vertebrated animals, whose respiration may be conceived to produce similar changes on the air. But we are in possession of many very curious facts respecting the respiration of the cold-blooded animals, which are not to be disregarded in forming our judgment upon this subject. Spallanzani's researches on the respiration of the cold-blooded quadrupeds appear to show very clearly that they absorb nitrogen in respiration[1]; and the experiments of Humboldt and Provençal, on fishes, place the fact beyond all doubt, as far as these animals are concerned. The quantity of nitrogen varied very considerably in the different experiments, from 20 to as much as 89 per cent., while the relation which it bore to the carbonic acid produced was also variable, although, for the most part, an increase in the one was attended with an increase in the other[2].

The researches of Dr. Edwards on this point have been no less successful than on the other objects to which he directed his attention. By immersing small animals in a large quantity of air, for a limited period, and calculating what effect the air contained in their lungs before and after the experiment would have upon the whole mass on which he operated; he found that, in many instances, there was an evident increase in the quantity of nitrogen, while in others there was a deficiency of it. He observed that the former change took place when the experiments were performed in spring or summer, or when young animals were employed, while the latter occurred during the winter. Hence, we have the important fact established, that nitrogen is, according to circumstances, either exhaled or absorbed in respiration; the probability is, that in all cases, both these operations are going forwards, that they are often exactly balanced, so as to show neither excess nor deficiency of nitrogen in the expired air, while in other cases, depending, as it would appear, principally upon temperature, or upon the age of the animal, either the absorption or the exhalation is in excess, producing a corresponding effect upon the composition of the expired air[3].

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[1] Mém. sur la Respir. p. 184, 258.

[2] Mém. d'Arcueil, t. ii. p. 359 et seq.; p. 378 consists of a tabular view of the results of the experiments. I shall refer my reader to Dr. Ellis's judicious observations on these experiments, which may lead us to doubt whether we can implicitly rely upon the exact quantity of effect produced; they do not, however, appear to me, in any degree, to invalidate the general conclusion; *Farther* Inquiries, p. 264 et seq.

[3] *De l'Influence* &c. p. 420 et seq.; Tab. 62..66.

It now remains for me to offer some remarks upon the aqueous vapour which is contained in the air of expiration. The discharge of water from the lungs was a circumstance which must have been noticed by the most cursory observers, and we shall accordingly find that it was much insisted upon by the earlier physiologists, who indeed regarded it as one of the principal uses of the function of respiration [1]. Sanctorius appears to have been among the first who attempted to estimate the amount of the pulmonary exhalation with any degree of accuracy; he supposed it to be half a pound in the 24 hours; but neither the mode in which he conducted his experiments, nor the reasoning which he employed respecting them, were calculated to produce any correct conclusion [2]. Hales adopted the method of passing the air that was emitted from the lungs through a flask filled with wood-ashes, and by observing what addition of weight it had acquired, he ascribed this to the moisture which the potash contained in the ashes had imbibed; this he estimated at 9792 grs., or about 20 oz. in the 24 hours [3]. Menzies received the air of expiration in an allantoid, and by weighing it before and after the experiment, ascertained what additional weight it had acquired; in this way he calculated, that the quantity of water discharged in 24 hours is equal to 2880 grs. or about 6 oz. [4] Mr. Abernethy breathed into a glass vessel, adapted for the purpose, and collected 180 grs. in an hour, which will give us 4320 grs. or 9 oz. in 24 hours; but he supposes that the fluid contains a quantity of mucus dissolved in it, the proportion of which he did not ascertain, but which must be deducted from the total amount [5].

The difficulty which there is in actually collecting the water exhaled from the lungs may probably have induced Lavoisier, in his later and more elaborate experiments, to endeavour to ascertain the quantity by an indirect method. He first determined the quantity of oxygen consumed, and of carbonic acid produced; and as he always supposed that the oxygen which had disappeared was more than sufficient to form the carbonic acid which he obtained, he conceived that the excess of oxygen was employed in uniting with hydrogen that was given off by the lungs, and thus generating water [6]. The quantity of oxygen being known, that of the water was easily calculated, but the estimates of the aqueous vapour, which Lavoisier formed in his different sets of experiments, differ very much from each other. In the memoir of 1789 the water is stated to be 337·18 grs., while the carbonic acid is 17720·89 grs. or nearly as 19 to 1000; in the memoir of 1790 the quantity of water was increased to 11188·57 grs. while the carbonic acid was reduced to 8451·24 grs., or as 1323 to 1000; and in the posthumous experiments the water is stated to be 13704 grs., the carbonic acid being 7550·4 grs., or nearly as 1815 to 1000. From these discordant estimates it is impossible to draw any conclusion, except that the method itself is one which cannot afford us any accurate results.

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[1] See the remarks of Collard de Martigni, in Magendie's Journ. t. x. p. 111 et seq., where the subject of the pulmonary exhalation is treated in detail, but in a somewhat diffuse, and perhaps incorrect manner.

[2] Medicina Statica, by Quincy, Aphor. v. p. 45.

[3] Statical Essays, v. ii. p. 322..4. I may observe that Haller has deviated from his usual accuracy in speaking of the estimates that have been formed on this subject. Home states, not quite correctly, that Hales obtained 23 oz. of water in 24 hours; Med. Facts, p. 238; and Haller, in relation to Home's estimate, says, "ad uncias 23 æstimat Cl. Home," and refers to the above passage in Home's work; El. Phys. viii. 5. 40.

[4] Dissertation, p. 54.

[5] Essays, p. 141.

[6] I have already stated that Lavoisier, in his first memoir, does not advert to the aqueous vapour which is exhaled from the lungs; it is in the

Nor, independent of this circumstance, does it appear to be one on which we ought to rely with any degree of confidence. The position on which the whole reasoning rests, the exhalation of hydrogen from the lungs, has never been attempted to be directly proved; it does not appear to bear any analogy to the other operations of the animal œconomy, nor is there any fact with which I am acquainted that seems to countenance it, while it is impossible not to perceive that there is a much more direct and probable source of the aqueous vapour, in the evaporation of water from the surface of the pulmonary passages, or even in transudation through the membranes investing these parts; but I shall have occasion to revert to this subject when I come to consider the changes produced upon the blood by respiration[1].

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memoir on the respiration of the guinea-pig in oxygen, that he first advances the hypothesis stated in the text. He notices the excess of oxygen above what is necessary to form the carbonic acid, and remarks, that it must either have been absorbed by the blood, or have combined with hydrogen discharged from the lungs, and have produced water; the latter supposition he conceives to be the most probable; Mém. Soc. Roy. Méd. pour 1782, 3. p. 574; Ann. Chim. t. v. p. 264, 5. He does not very clearly state the grounds of this preference, but it may be inferred that it depended upon his conceiving that more caloric was given off by the formation of the carbonic acid in the lungs, than by the formation of the same quantity of carbonic acid by the combustion of charcoal; and this excess of caloric he imagined might be accounted for by the union of the excess of oxygen with a quantity of hydrogen; see Mém. Acad. pour 1789, p. 569. I may remark that Crawford had previously stated, as the result of his experiments, that water, as well as carbonic acid, is generated by respiration; On Animal Heat, p. 154, 347, 8. In the memoir for 1789, Lavoisier refers to the memoir of 1780, written in conjunction with La Place, for a proof of the fact here stated, respecting the excess of caloric; but upon examining the latter paper it appears to me to warrant the contrary conclusion; see p. 405 and 407. See the remarks of Magendie, in his Mém. sur la Transp. Pulm. p. 4..6. This physiologist gives a curious case of an individual who had an opening in the upper part of the trachea, and it appeared that when he breathed through this aperture, scarcely any vapour was mixed with the expired air; he also relates some experiments on animals, which lead to the conclusion, that at least a large portion of the expired vapour proceeds from the membrane lining the mouth and fauces; Ibid. p. 13..5.

[1] Dr. Thomson, by a calculation founded upon the force of vapour in the expired air compared with that in the atmosphere, estimated that he dis-

Having now examined in succession the various effects which respiration has been supposed to produce upon the air, I shall briefly recapitulate the result of our inquiry. 1. Air which has been respired loses a part of its oxygen; the quantity varies considerably, not only in the different kinds of animals, but in different animals of the same species, and even in the same animal at different times, according to the operation of certain external agents, and of certain states of the constitution and functions. Upon an average we may assume that a man, under ordinary circumstances, consumes about 45000 cubic inches, or nearly 15500 grs. of oxygen in 24 hours. 2. A quantity of carbonic acid is produced, the amount of which varies very much according to circumstances, both external and internal; its quantity depends, to a certain extent, upon the quantity of oxygen consumed, but the two are not in exact proportion to each other; in a great majority of cases the quantity of carbonic acid produced will be found to be less than that of the oxygen consumed, so that there will be a surplus quantity of oxygen more than is necessary for the production of the carbonic acid. In consequence of the variations which take place in the amount of the carbonic acid produced, it appears almost impossible to fix upon any number which may indicate the average quantity; but it may be stated to be somewhere about 40000 cubic inches in 24 hours. This will weigh 18600 grs. or nearly 3 lbs., and will contain 5208 grs. of charcoal and 13392 grs. of oxygen, which will be 2100 grs. less than the quantity of oxygen consumed[1]. 3. The volume of the air is

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charged from the lungs nearly 19 oz. in 24 hours; Chem. v. iv. p. 621, 2. Dr. Dalton, by a similar process, estimates the quantity at 1·55 lb.; Manch. Mem. v. ii. 2d ser. p. 29. As far as we are able to apply to the living body the results of experiments made upon the dead subject, we may suppose, from the statement of Reisseisen, that the arteries of the lungs are peculiarly adapted for exhalation; Ed. Med. Journ. v. xxi. p. 453 et seq. I may refer in this place to some experiments which were performed by MM. Edwards and Vavassour, the object of which was to illustrate the facility with which the surface of the lungs admits of absorption and exhalation; the experiments were performed on horses; Art. "Respiration," Dict. Class. d' Hist. Nat. In the recently published number of the British and Foreign Medical Review, p. 241..6, we have an account of a series of experiments by Prof. Tiedemann, on pulmonary exhalation; they consisted principally in injecting odorous substances into the blood vessels, as the femoral veins of dogs, when the odour of the substance employed was very quickly experienced in the exhalation from the lungs.

[1] It may be not uninstructive to the student of physiology to remark upon the singular vacillation of opinion that has taken place on this subject. About 20 years ago the doctrine of the absorption of oxygen was very generally embraced, all the facts and analogies appearing to be in its favour. After some time, however, it was almost universally discarded, in a great measure, as it would appear, in consequence of the experiments of Messrs. Allen and Pepys; see Berzelius on Animal Chemistry, p. 30..2; while, I apprehend, that the more recent investigations of Dr. Edwards, taken in conjunction with the former facts and analogies that were adduced in its favour, will cause us to revert to the conclusion which is stated in the text. I may refer

diminished by respiration, but this, like the changes mentioned above, varies so much at different times, that it is almost impossible to form any statement of the quantity; perhaps, we may assume that air, which has been once respired, is diminished by about $\frac{1}{80}$ of its bulk. 4. It appears probable that nitrogen is both absorbed by the lungs, and exhaled from them; but the two processes of absorption and exhalation differ very much, both in their absolute quantity, and in the relation which they bear to each other, so that the proportion of nitrogen in the air is sometimes diminished by respiration, is occasionally increased, and frequently remains without alteration. 5. A quantity of aqueous vapour is discharged from the lungs, mixed with, or diffused through the air of expiration; but we have not sufficient data from which to decide upon its amount, and it is probable that the quantity varies considerably in the different conditions of the system and the different situations in which the body is placed.

SECT. 4. *The Change produced upon the Blood by Respiration.*

The change which is produced upon the blood by respiration involves an inquiry of a much more difficult solution than that respecting the change in the air, in proportion to the greater difficulty of ascertaining the chemical nature of the ingredients of the blood. Indeed, so complicated is this fluid in its composition, and so peculiar is its constitution, that scarcely any attempts have been made to investigate the effect which respiration produces upon it, by examining the substance itself; all that we are able to accomplish is to deduce this effect from observing the changes which we find to have taken place in the air, assuming that the blood has been the medium by which they were brought about [1].

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to the amicable controversy that took place on this point between Dr. Ellis and myself; Ed. Med. Journ. v. iv. p. 159, 320. I conceive that the opinion which I attempted to defend in my paper has since received, from various quarters, but especially from Dr. Edwards, the most unequivocal support. I may say this with the more propriety, because the experiments of Messrs. Allen and Pepys appeared so favourable to Dr. Ellis's doctrine, that I became a convert to it, and supported it in my lectures on physiology; and in the article "Physiology," in Dr. Brewster's Encyc., written in 1823.

[1] It was a question with the older physiologists, whether there was any essential difference between arterial and venous blood; and it would appear, that those who believed that there was a difference, derived their opinion rather from theory than from actual observation. Haller himself doubts, or rather disbelieves, the difference; El. Phys. v. 1. 4, 5. A considerable degree of the uncertainty which prevailed among physiologists, before the time of Harvey and Lower, depended upon their being ignorant of the relation between the systemic and the pulmonic circulation, and of the exact point in the circulation, where the venous was converted into arterial blood. Magendie has given us a useful synopsis of the external characters of the two species of blood in a tabular form; Physiol. v. ii. p. 288.

The extreme vascularity of the lungs, and the great proportion of blood which is sent to them, induced even the earliest physiologists to suppose, that some important effect is produced upon this fluid by respiration: this idea was strongly countenanced by the discovery of Harvey, that every portion of the blood passes through the lungs at each complete circulation; and it was still farther confirmed by the observation, that the change from venous to arterial blood takes place in the capillaries of the lungs, and that the air is essential to it. The opinions that were entertained respecting the nature of this operation, and the manner in which it is effected, were very various, but they may be all reduced to three classes[1]. A numerous and learned body of physiologists supposed the effect produced on the blood to be merely mechanical[2]. Some of them thought that the particles, by the agitation which they must experience in passing through the pulmonary vessels, were more completely comminuted or mixed together, so as to render the mass of an homogeneous consistence. This idea depended upon the supposition, that the velocity of the blood was greater during its passage through the lungs than in the other parts of the circulation; a supposition which Hales conceived to be decisively proved by actual observation[3], and which was, at one time, very generally adopted. There were others, however, who thought that the motion of the blood could not be quicker through the lungs, and others again who thought it must even be slower in this part of its course, founding their opinions principally upon certain anatomical considerations, connected with the structure of the heart and its great vessels. We shall probably be induced to coincide in the opinion of Haller, that the average velocity of the blood through the lungs is not greater than through the other parts of the body; but that its momentum must be much less, because it has fewer obstacles to its progress, a circumstance which is sufficiently indicated by the comparative weakness of the right ventricle[4]. Boerhaave and his disciples thought that the blood acquired its peculiar organization in the lungs, but they do not appear to have thought it necessary to inquire in what way the effect was brought about[5]. The question whether the blood was rarefied or condensed in the lungs was zealously contested by the mechanical physiologists, one party supposing that the addition of a portion of the air must render the blood specifically lighter[1], while others conceived that the exhalation of the aqueous vapour, and the contact of the cold air, must increase its specific gravity[2].

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[1] An interesting, and, upon the whole, a correct account of the various opinions entertained on the use of respiration is prefixed to Priestley's Essay; Phil. Trans. for 1776, p. 226 et seq., or On Air, v. iii. p. 350.

[2] As a specimen of the mechanical method of reasoning upon this subject, the dissertation of Sauvages, on the action of the air upon the blood, which was written about the middle of the last century, may be read with advantage, being the production of a man of extensive information, who may be supposed to have been possessed of all the science of his age; see Œuvres Diverses, t. ii. p. 139 et seq.; see also Pitcairne's Dissert. No. 4.

[3] Statical Essays, v. ii. p. 66.

[4] El. Phys. viii. 5. 21.

[5] Prælect. notæ ad § *200. t. ii. p. 93;* § *210.* t. ii. p. 115, 6.

A second class of physiologists, in which we find the illustrious names of Harvey[3], Boyle[4], Hales, and Haller, supposed that the blood, in its passage through the lungs, discharged some noxious matter, which, together with the aqueous vapour, was removed by respiration[5]; while a third class, among whom we may rank Lower, Hooke, Mayow, and many of the Italians[6], conceived that the air imparted something to the blood, by which it was converted to the arterial state. We shall find that none of these opinions is strictly correct, even in the outline, and when their respective advocates proceeded to give them more in detail, they quickly degenerated into mere fanciful hypotheses.

Soon after Harvey had completed the discovery of the circulation, the difference between the colour of the arterial and the venous blood was clearly pointed out, and Lower ascertained that the change of colour took place in the capillaries of the lungs. Before his time the bright scarlet colour of arterial blood had been ascribed by some to a kind of combustion, which is kept up in the heart, by others to the breaking down of the red particles, or to other causes equally inadequate and equally unfounded. By opening the thorax of a living animal he perceived the exact point in the circulation where the change of colour takes place, and he proved that it was not in the heart, because it still remains purple when it leaves the right ventricle.

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[1] Baglivi, Opera, p. 457.

[2] See the elaborate dissertation of Helvetius; Mém. Acad. Sc. pour 1718, p. 230 et seq.

[3] De Motu Cordis, p. 232.

[4] Works, v. i. p. 99 et seq.; v. iii. p. 371 et seq.

[5] It may be interesting to observe how far the genius of Vesalius enabled him to ascertain the nature and uses of respiration. "Postquam vero aër ab hac substantia" (pulmonis) "cordi quodammodo præparatus est, a venalis arteriæ surculis, pulmoni etiam intextis, ex asperæ arteriæ ramis elicitur et in sinistrum cordis ventriculum delatus, tenui admodumque fervido, quem cor inibi continet, sanguini commiscetur. Hujus aëris qualitates, contenti in hoc ventriculo caloris qualitas eventilatur, substantia autem caloris (quæ aëre et spirituosi sanguinis exhalatione constat) istius aëris substantia enutritur. Quo vero velut fuliginosum ex hoc peculiari cordis functione congeritur, rursus per venalem arteriam in pulmonem allegatur; &c." Corp. Hum. Fab. lib. 6. c. i.; t. i. p. 492, 3.

[6] See Boer. Prælect. § 203 cum notis, et Haller, El. Phys. viii. 5. 12, 3, for an account of the earlier physiologists who adopted this opinion. To the names mentioned by Haller we may add that of Mead, Works, vol. ii. p. 42; and of Whytt, Works, p. 31. Robinson, who was a physiologist of considerable acuteness, lays down the following proposition, and endeavours to prove it by experiment. "The life of an animal is supported by acid parts of the air mixing with the blood in the lungs; which parts dissolve and attenuate the blood and preserve its heat; and by both these keep up the motion of the *heart;*" *Prop.* 24. p. 187.

He then kept the lungs artificially distended, first, with a regular supply of fresh air, and afterwards with the same portion of air without renewing it, when the result was that, in the first case, the blood underwent the usual change of colour, while in the second it returned to the left side of the heart, still retaining its purple hue. Hence he naturally and correctly concluded, that the alteration of colour is effected by the air, and he still farther enforced his opinion by observing the action of the air on the crassamentum of the blood out of the body, which, as far at least as the colour is concerned, he found to coincide exactly with what takes place in the lungs[1]. We are now so familiar with the facts mentioned by Lower, and are so well assured of the general correctness of the method by which he accounted for them, that it is impossible not to feel surprise at the little impression which his opinions produced upon his contemporaries. We learn, however, that they were almost entirely disregarded, and so completely was the attention fixed upon the mathematical hypothesis[2], and so permanent an influence had it acquired over the minds of physiologists, that even Haller decidedly opposed the doctrine of Lower[3].

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[1] De Corde, p. 175..181. Experiments similar to those of Lower have been so frequently repeated, as scarcely to require any particular reference; as a specimen those of Dr. Philip may be mentioned; Phil. Trans. for 1815, p. 71, 2. ex. 7, 8. The effect of respiration upon the colour of the blood is well illustrated by those cases which are termed Cæruleans, where, in consequence of a mal-conformation of the heart or its appendages, the blood is not duly transmitted through the lungs. See Wm. Hunter's two cases in Med. Obs. and Inq. v. vi. p. 291; Sandiforth, Obs. Anat. Path. t. ii. p. 11 et seq.; the same translated, with some additional observations, in Beddoes on Calculus, p. 62; Abernethy's Essays, p. 2. p. 158. There is a case of this kind related by Mr. Standert, in Phil. Trans. for 1805, p. 228, which deserves notice, in consequence of the structure of the heart being exactly similar to that of some of the amphibia; it had only one auricle, and one ventricle. See also on this subject the remarks of Dr. Paget, on what he terms Cyania, in his essay on malformations of the heart; Ed. Med. Journ. v. xxxvi. p. 306; also the art. "Cyanesis", by Dr. Crampton, in the Cyc. of Med. in loco.

[2] It is amusing to observe the air of confidence and self-satisfaction with which Pitcairne opposes his mathematical hypothesis to the experiments of Lower; Dissert. p. 69, 0.

[3] Boerhaave, Prælect. notæ ad § 203. t. ii. p. 107; El. Phys. vi. 3, 17. The manner in which Haller speaks of Lower is still more worthy of remark, than the above observations of Pitcairne, as proceeding from one so much better fitted to form a judgment upon the subject. Speaking of the effect of the air upon the part of the crassamentum which was exposed to it, he adds, "Hoc vulgare experimentum non a Lowero solum, verum etiam ab Helvetio serio propositum est." The remark with which Haller concludes his section on the use of respiration is much more characteristic of his candid and philosophical turn of mind. "Parum forte satisfactum est multis, neque certe non laude dignissimis viris, qui tanta in respirationis per universum animalium genus constantia perspecta, nobilius aliquod per pulmones beneficium vitæ animali accedere suspicantur, quam quidem sunt a nobis exposita munia. Eos viros unice velim mihi non succensere, quod id officium, ut mihi nondum cognitum interim omittam, usquedum quid sit, perspicacior intellexero;" El. Phys. viii. 5. 24. He had informed us in the previous section, 23, that he considered the formation of the voice as the principal use of respiration.

After a considerable time the doctrine of Lower was revived by Cigna of Turin, who performed a set of experiments for the purpose of proving that the change in the blood from the purple to the scarlet colour, always depends upon the action of the air; but although they appear sufficient to establish this point, they excited little attention, and Cigna himself, in a subsequent memoir, seems half inclined to desert his former opinion. The opinion of Cigna, as I have already observed, was taken up by Priestley, and confirmed by a series of new and varied experiments, while they led him to the farther discovery of a train of facts, which have served as the basis of all the information that has been since gained upon the subject. The action of the air on the blood, which, as we have seen, had been previously admitted, rather as a plausible conjecture than as a deduction from facts, was now proved by direct experiment. It was found that a piece of purple crassamentum, when introduced into a portion of air, assumed the scarlet colour, while the air experienced the same change as by respiration. Priestley afterwards examined the effect which would be produced on the blood by the constituents of the atmosphere applied separately, as well as by the other gaseous fluids which had been recently discovered. Purple crassamentum was reddened more rapidly by oxygen than by the air of the atmosphere, while the contrary effects were produced by nitrogen, hydrogen, and carbonic acid, the scarlet crassamentum being reduced by these to the purple colour. The conclusions from these experiments are highly important; they show that the alteration of colour which the blood experiences in the lungs depends upon the oxygenous part of the atmosphere, and reciprocally, that the change produced on the air by being received into the lungs depends upon the action of the blood in the pulmonary vessels. In order to render the resemblance between his experiments and the actual state of the lungs more complete, Priestley introduced a piece of moistened bladder between the crassamentum and the air, when he found that the same change was effected as in the former case; he also found that the action of the air upon the blood was not interrupted by the intervention of a stratum of milk or serum, but that water and some other fluids which he tried, prevented the change from taking place. The change which, in these cases, takes place in the air, Priestley supposed to be similar to that produced by combustion, and, according to the hypothesis then generally embraced, it was conceived to consist in the addition of phlogiston; he consequently concluded, that the abstraction of a portion of phlogiston constituted the principal difference between venous and arterial blood, and that this removal of phlogiston was the chief use of respiration[1]. I have noticed above the modification which Lavoisier introduced into Priestley's hypothesis, depending upon his more correct views of the nature of what had been styled the phlogistic processes; proceeding upon Black's discovery of carbonic acid in the air of expiration, and his own discovery of the constitution of this acid, as consisting of oxygen and carbon, he concluded that the essential difference between arterial and venous blood consists in the latter containing a larger proportion of carbon. To this deduction from well established facts Lavoisier afterwards added the more doubtful hypothesis of the discharge of hydrogen; and although no direct evidence was adduced in favour of this doctrine, so great was the authority attached to every opinion of Lavoisier's, that it obtained almost universal consent[1], and the phlogiston of Priestley was accordingly converted into hydrocarbon[2]. But the discharge of hydrogen from the lungs, as it rested upon little more than conjecture, was gradually abandoned, and the former doctrine was again adopted, that the chemical change in the blood, consists principally in the separation of a portion of carbon[3].

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[1] *On Air*, v. iii. p. 362..374; Phil. Trans. for 1776, p. 147.

We are, however, under the necessity of modifying, or rather of correcting this conclusion in consequence of the views which have been taken respecting the changes produced on the air; for, besides the conversion of oxygen into carbonic acid, by the abstraction of carbon from the blood, it also appears that a portion of oxygen is, in some way or other, received into the system, and that a mutual interchange of nitrogen is always going forwards between the air and the blood, so that at some times the blood has its proportion of this element absolutely diminished, and at other times increased. With respect to the water which is carried off by the expired air, it is probable that this depends upon evaporation from the surface of the pulmonary cavities; or if any part of it should be secreted from the blood itself as it passes through the lungs, this must be regarded as only indirectly connected with the process of respiration.

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[1] See Essay on Respiration, p. 228, for references to various writers, both English and Continental, who embraced the opinion that hydrogen is discharged from the lungs; the list, if necessary, might be extended.

[2] The most complete, and, as we may presume, matured account of Lavoisier's doctrine is contained in the paper written by himself, in conjunction with Seguin, and published in the Mém. Acad. Sc. pour 1789, p. 566 et seq. We have also a good abstract of Lavoisier's doctrine, and his successive discoveries given by Fourcroy in his "Médecine Eclairée," t. i. p. 56 . . 61, published in 1791. See also Seguin's paper on various topics respecting heat in Ann. Chim. t. v.; in this essay he points out the connexion between respiration and animal temperature, p. 259, 0, and afterwards gives an account of the nature of the change by which arterial is converted into venous blood; this he supposes is by the addition of hydrogen, and that by the union of this hydrogen in the lungs with oxygen, the blood becomes again arterialized; he remarks that hydrogen, as produced from animal substances, always contains carbon, and that this carbon also unites with oxygen and produces carbonic acid. The production of the carbonic acid is therefore considered as a kind of incidental or secondary effect; p. 262 . . 8.

[3] Thenard, Chimie, c. 3. sect. 2. t. *iii. p.* 666.

It would appear, therefore, that by far the most important change which the blood experiences, at least so far as quantity of effect is concerned, consists in the removal of a portion of its carbon. Some attempts have been made to prove that venous actually contains more carbon than arterial blood, and the results are said to correspond with the hypothesis[1], but the evidence of its truth must principally rest upon a knowledge of the changes which take place in the air. The air certainly acquires carbon by being brought into proximity with the blood; there is no assignable source whence the carbon can be acquired except the blood; the blood obviously undergoes some change from the action of the air upon it, and the crassamentum, when removed from the vessels, affects the air in the same manner with respiration.

But although the fact be thus established, the manner in which this change is effected is much less easy to comprehend than the nature of the change. Two hypotheses have been formed to account for the operation, each of which is supported by the authority of great names, and by many ingenious arguments, as well as by direct experiment. According to one hypothesis, which may be regarded as that originally proposed by Black, and adopted by Priestley[2], Lavoisier, and Crawford[3], the oxygen of the inspired air immediately attracts carbon from the venous blood, the carbonic acid being directly generated by their union. According to the other, the oxygen is absorbed by the blood, is mixed with it, and unites with a portion of its carbon; when the blood, in the course of the circulation, again arrives at the lungs, the carbonic acid that had been formed is discharged, while a fresh portion of oxygen is absorbed. The essential difference between the two hypotheses may be expressed in the following query; are the changes induced by respiration entirely effected in the lungs, or are they brought about in the body at large, the lungs serving merely as the organ by which the substances are absorbed or discharged? The first of these hypotheses has the recommendation of being the most simple, but several objections were urged against it, which gave rise to the more complicated hypothesis that was proposed by La Grange.

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[1] Abildgaard, in Ann. Chim. t. xxxvi. p. 91 et seq.

[2] Priestley originally took this view of the subject, but he afterwards thought it more probable that the oxygen is absorbed by the blood; an opinion which he appears to have adopted in consequence of his supposing that the quantity of oxygen which disappears is greater than what is necessary to form the carbonic acid; Phil. Trans. for 1790, p. 106 et seq.

[3] It is not intended by this expression to signify, that, at this period, Black, Priestley, and Crawford, had a correct conception of carbonic acid, as consisting of carbon and oxygen, which was a subsequent discovery of Lavoisier; Black announced the actual formation of carbonic acid, while Crawford and Priestley supposed that an inflammable matter was discharged from the blood, which converted part of the inspired air into carbonic acid; see Mém. Acad. pour 1775, p. 520..6; also Black's Lect. by Robison, v. i. p. 99, where he fully admits of Lavoisier's claim. The successive steps by which we arrived at a correct opinion respecting the constitution of carbonic acid, and the share which Lavoisier had in the discovery, are well pointed out by Mr. Aikin, in the article "Lavoisier," Gen. Biog., v. vi. p. 162. Lavoisier's paper referred to above was first read to the Academy in 1775, read a second time in 1778, and published in the same year. In his memoir of 1777, p. 191, he clearly states the two hypotheses of the absorption of oxygen and of its direct conversion into carbonic acid, and thinks it probable that both the operations may take place, although, as we have seen, he afterwards determined exclusively in favour of the latter opinion; see Dr. Edwards, De l'Influence &c., p. 437, 8.

In order to form a judgment of their respective merits, as well as to complete the theory of respiration generally, it is necessary to inquire into the source of the carbon which is removed from the lungs, and to consider the probable effect which would result from its union with oxygen, according as it may take place in the lungs only, or in the course of the circulation. The first attempt to explain the mode in which the blood acquires its inflammable matter was made by Crawford. He observes that the particles of which the body is composed have a tendency to change, the old ones are perpetually removed, while fresh matter is continually deposited in their room. This gradual interchange of particles is effected by the capillary vessels; the arterial blood conveys nutritious matter to all parts of the body, and employs it in repairing the waste that is necessarily going on, while, at the same time that the blood loses its nutritive particles, it receives the effete or putrescent matter, which is now become useless or even noxious to the system; this is carried by the veins to the lungs, and is there discharged, after being united to oxygen[1]. It is to this change of particles that the difference between arterial and venous blood is ascribed, and it follows, according to this view of the subject, that the matter which is received into the systemic veins contains more carbon than that which is carried off by the arteries and is employed in the growth and nutrition of the body.

Crawford's hypothesis possesses much ingenuity; it accords with some well established facts, and seems to afford a simple and natural explanation of them, yet, upon a closer inspection, it will be found to be inadmissible. We have no evidence of the existence of any set of vessels or other apparatus, by which the carbon can enter the veins at their capillary extremities, while there is an obvious source of this matter in the chyle which is poured into them, near their termination in the right side of the heart, immediately previous to the passage of the blood through the lungs. The properties and uses of the chyle will be fully considered hereafter; but I may remark in this place, that there can be no doubt that it is the substance destined for the support of the system, by which the waste of the body is repaired, and materials are furnished for its growth and increase. Hence we are led to the conclusion, that arterial blood becomes venalized, not in consequence of any thing which it receives while it is passing into the veins, but from what it loses in forming the various secretions, or in contributing to the growth and nutrition of the body.

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[1] On Animal Heat, p. 150, 1. He brings forward a direct experiment of Hamilton's, in order to prove that blood is venalized by the addition of the basis of hydrogen, p. 149, 0; but the experiment is not of that nature which can enable us to draw any important consequences from it. The same experiment, as well as some of Priestley's, on the action of hydrogen on the blood, is also referred to by Seguin, in order to prove the absorption of hydrogen as stated above; Ann. Chim. t. v. p. 266, 7; see also Crawford, p. 147.

Dr. Ellis's hypothesis, respecting the origin of the carbon which is employed in respiration, differs essentially from the opinion that is generally adopted on this subject, in supposing that the carbon does not proceed immediately from the blood, but that it is an excretion, produced by the action of the exhalent vessels of the lungs. But, I conceive this hypothesis to be defective, inasmuch as it does not sufficiently explain the object of the elaborate apparatus, by which the air and the blood are brought into such close and extensive proximity; nor does it show the connexion between the chemical change which the air experiences in the lungs, and the conversion of the blood from the venous to the arterial state. It appears moreover to neglect the analogy which we have between the action of the air on the blood out of the body, and what takes place in the lungs; the change appears to be the same, in each case, both upon the air and the blood, and hence we naturally infer that it is brought about by the same kind of agency[1].

As an objection to the hypothesis which supposes the union of oxygen and carbon to be brought about in the lungs, various facts were adduced to show, that the change from the arterial to the venous state can take place, by the action of the constituents of the blood upon each other, while it remains in the great trunks, in a situation where it is incapable of receiving any addition of extraneous matter. It has been observed in surgical operations, that after a tourniquet has been applied to an arterial trunk, the blood which first flows when we remove the instrument, is perceived to be of the venous colour, and it was remarked by Hunter, that extravasated blood is always purple, even in cases where there is every reason to suppose that it may have proceeded from an artery. That this was actually the case he proved by puncturing the femoral artery of a dog, when upon examining the blood that was effused in the adjoining cellular substance, he found that it was of the purple colour, and as far as could be judged by its external characters, was converted into the venous state, although it had been carefully preserved from the contact of any extraneous body. A more direct experiment was then tried; a portion of the carotid of a dog was included between two ligatures, and upon piercing this part of the vessel after some hours, it was found to contain blood which had acquired the complete venous appearance[1].

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[1] This point is well stated by Crawford, allowance being necessarily made for the discoveries and consequent changes of our hypotheses which have taken place since the date of his publication; On Animal Heat, p. 147, 8. Some late experiments of Fodera's on transudation seem very much to favour the idea of the possibility of the air acting upon the blood through the intervention of the vessels; see Magendie's Journ. t. iii. p. 35 et seq., and the still later experiments of Drs. Faust and Mitchell, which have been referred to above, remove any doubt which might still attach to the subject.

It was partly from certain facts of this description, and partly from the difficulty which was supposed to exist in accounting for the equable diffusion of heat over the system[2], that La Grange formed his hypothesis, which Hassenfratz illustrated by various arguments and direct experiments[3]. According to La Grange, the oxygen is absorbed in the lungs, and enters into a loose combination with the blood, to which it imparts the scarlet colour; during the course of the circulation a more intimate union takes place between the oxygen and the carbon, in consequence of which the blood becomes venalized. The difference, therefore, between arterial and venous blood depends not so much upon the nature and proportion of its constituents, as upon the mode of their combination, and the action which they exercise on each other[4]. From the time when the blood enters the left auricle of the heart, until it leaves the right ventricle, it undergoes the complete change from the arterial to the venous state, yet it may be presumed that the proportion of oxygen and carbon is not altered, in so far as respects this specific change[5].

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[1] On the Blood, p. 65..7.

[2] Hassenfratz, Ann. Chim. t. ix. p. 265, 6, observes, that according to Crawford's hypothesis, "les poumons sont le foyer où se dégage toute la chaleur que la sang abandonne dans l'economie animale;" and again, "M. de la Grange reflechissant que si toute la chaleur qui se distribue dans l'economie animale se dégageoit dans les poumons, &c." It must excite some surprise that these expressions should have been employed on a subject so generally known as Crawford's doctrine of animal heat, which had been many years before the public; and still more so, because Hassenfratz himself, in the beginning of his paper, p. 263, expressly notices the experiments on the different capacities of arterial and venous blood as what were generally recognized. See Dr. Dalton's observations on this point in Manchester Mem. v. ii. 2d. ser. p. 20.

[3] Ann. Chim. t. ix. p. 269.

[4] The experiment of Priestley, in which arterial blood assumed the venous hue by being placed in vacuo, On Air, v. iii. p. 364, has been regarded as a proof that the change from the arterial to the venous state must depend upon the action of the constituents of the blood on each other.

[5] A modification of La Grange's hypothesis was proposed by Mr. Allen, in his lectures on the animal œconomy, formerly delivered at Edinburgh, according to which a part only of the oxygen, necessary to form the carbonic acid, is united to it in the course of the circulation, so as to produce an oxide of carbon; when this arrives at the lungs, it attracts from the air the remaining quantity of oxygen, and is converted into carbonic acid. For an account of Mr. Allen's doctrines on this and some other points connected with it, the valuable thesis of Professor Delarive may be consulted. It is supposed that

In pursuance of this idea, Hassenfratz proposed to observe what would be the effect of placing blood in contact with oxygen, or substances supposed to contain it, and also to notice the spontaneous changes which arterial blood undergoes when cut off from all communication with oxygen. The experiments performed with a view to the first of these objects cannot be regarded as entitled to much attention; indeed they principally consist in comparing the effects produced by exposing portions of blood to liquid chlorine, and to muriatic acid. The other set of experiments are, however, more deserving of our consideration, both as affording more direct results, and as of so simple a nature as to be little liable to mistake or inaccuracy. Hassenfratz filled a number of tubes with arterial blood, and sealed them hermetically, when he uniformly found that the blood, after some time, lost its scarlet colour, and acquired the complete venous aspect.

Upon the whole the experiments and reasoning of Hassenfratz are not without their value, although few, if any of them, are of that unequivocal nature, as to afford any very direct or decisive proof of the truth of the hypothesis. It must be acknowledged that the mere change of colour which the blood undergoes, when it is extravasated in the cellular texture, enclosed in sealed tubes, and still less the effect of chlorine upon it, can be considered as bearing but an imperfect analogy to what takes place while it is circulating in the vessels. And we may farther remark, that even should we consider the observations and experiments of Hunter and Hassenfratz as proving that the change from the arterial to the venous state may be effected, without any addition ab extra, it does not necessarily follow that the reverse operation can take place, nor indeed

the serum contains a quantity of pure soda, which is incompatible with the presence of carbonic acid in the blood. The hypothesis of Richerand is very similar to that of Mr. Allen; Elem. of Phys. § 76. p. 208. Blumenbach's idea of the nature of the change which the blood experiences is not essentially different from that of La Grange, except that he ascribes the change to carbon only, and not to the compound of carbon and hydrogen; he supposes that the "oxygenized blood" acquires carbon in the small vessels; Instit. § 167. Sir E. Home suggests, as an argument in favour of the opinion that oxygen is actually absorbed by the blood, that if this were not the case, the fœtal blood could not be aerated by being brought into proximity with that of the mother; Phil. Trans. for 1810, p. 217. The suggestion may be regarded as favourable to the hypothesis, but it might be said that the maternal blood, in this case, merely abstracts carbon from the blood of the fœtus. In the same connexion the experiment of Hewson may be mentioned, in which he confined a quantity of blood in the jugular vein between ligatures, and upon admitting air to it, observed that each bubble of air, as it came in contact with the venous blood, converted it to the arterial hue; Enquiries, v. i. p. 8. ex. 3. We have an experiment related by Fourcroy, which has been supposed to be favourable to the hypothesis of the absorption of oxygen by the blood; a portion of air was confined in a jar over blood, when the air was found to have its volume diminished and its oxygenous part removed; Ann. Chim. t. vii. p. 148, 9; but as this experiment was performed in the infancy of the pneumatic chemistry, we may suspect there is some inaccuracy in the statement.

have we any evidence that it ever has been accomplished, except by the intervention of oxygen. It is more by other considerations, connected with the changes induced upon the air, or with the part which its constituents perform, either separately or conjointly, when placed in contact with the blood, that we must form our opinion upon this controverted subject.

And indeed the merits of the question may be rested almost exclusively upon the single point, whether the oxygen which is consumed be exactly replaced by an equal bulk of carbonic acid, the nitrogen remaining altogether passive, or whether there be a surplus quantity of oxygen absorbed by the blood, as well as a reciprocal absorption and exhalation of nitrogen. After duly balancing the facts and arguments that have been advanced on each side of the question, I have been induced to adopt the latter of these opinions, and as a certain degree of absorption of both oxygen and nitrogen appears to take place in the lungs, there is no difficulty in supposing that this is the case with respect to the whole of what is employed in the system; and we shall probably find it to be more consonant to the other operations of the animal œconomy to conceive of the union between the oxygen and the carbon being brought about during the course of the circulation, than by a momentary contact in the lungs alone[1].

But this opinion does not rest entirely upon our knowledge of the changes which are induced upon the air by its passage through the lungs; we have some very direct and unexceptionable experiments by Dr. Edwards, which may be regarded as proving both the absorption of oxygen and the exhalation of carbonic acid by the pulmonary vessels. Having shown that the air is diminished by respiration, he proceeds to examine whether the diminution depends upon the absorption of oxygen or of carbonic acid, and he determines in favour of the former, because when a small animal is confined in a large quantity of air, and the process is continued for a sufficient length of time, he found that the rate of absorption was greater at the commencement, than towards the termination of the experiment, while at the former period there must have been an excess of oxygen present, and at the latter an excess of carbonic acid[1].

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[1] We have some observations and experiments of Dr. Davy's, that bear indirectly upon this question, and favour the same opinion. He found that in certain morbid conditions of the chest, the pleuræ appeared to have the power of absorbing, and probably of exhaling air from their surfaces, and this seemed likewise to be the case with air artificially introduced between the pleuræ in their healthy state. Hence he justly infers, that mucous membranes generally possess the property of absorbing and exhaling air, and that these operations are mutually going forward in the natural process of respiration; Phil. Trans. for 1823, p. 496 et seq. An inference of the same kind has been drawn from the chemical constitution of the air in the swimming bladder of fishes, which must apparently be regarded as the product of the containing membrane, proceeding from exhalation, and probably modified by absorption. It has been found by different experimentalists that the composition of this air differs from that of the atmosphere. It sometimes contains less oxygen, as was found to be the case by Priestley; On Air, v. ii. p. 462, 3; but, as it appears, it frequently contains a larger proportion. Biot established this very satisfactorily, by a series of experiments related in Mém. d'Arcueil, t. i. p. 252 et seq.; from which we learn, that the proportion of oxygen increases with the depth of the water in which the fish usually resides, varying from a very minute quantity to 87 per cent. Biot's experiments have been fully confirmed by Configliachi; Ann. Phil. v. v. p. 40. Humboldt and Provençal likewise found that the composition of the air in the swimming bladder of river fish was not uniform in its composition; Mém. d'Arcueil, t. ii. p. 400 et seq.

Dr. Edwards's experiments in proof of the exhalation of carbonic acid by the lungs are no less ingenious and decisive than those related above. Spallanzani had stated, that when certain animals of the lower orders are confined in gases that contain no oxygen, still the production of carbonic acid is not interrupted; proceeding upon this statement, Dr. Edwards confined frogs in pure hydrogen, in which, by observing the necessary precautions, they are capable of existing for a considerable length of time, while we observe that the action of the lungs is not suspended. The result of this experiment was that carbonic acid was produced, and in such quantity as to show that it could not have been derived from the residual gas in the lungs, being in some cases nearly equal to the bulk of the animal. The same results, although in a less degree, were obtained with fishes, and afterwards with snails, the animals on whom Spallanzani's original observations had been made[1]. He also extended his experiments to the mammalia, by taking advantage of a property which he had found to exist in certain species of newly-born animals, of being able to exist, for a short time, without the access of oxygen to their lungs. Kittens of two or three days old were immersed in hydrogen; they remained in this situation for nearly twenty minutes, without being deprived of life, when it was found that they had expired a quantity of carbonic acid greater than could possibly have been contained in their lungs at the commencement of the experiment[2].

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[1] De l'Influence, &c. p. 411, 2. The same inference follows from an experiment on the respiration of birds, which was performed by MM. Allen and Pepys. When a pigeon was made to respire an atmosphere of oxygen and hydrogen, there was a loss of oxygen, while a quantity of hydrogen appeared to have been absorbed, which was replaced by an equal bulk of azote; Phil. Trans. for 1829, p. 286. This experiment may be considered as the more valuable, because it seems to oppose the conclusion which these gentlemen deduced from their former experiment, while it directly confirms that of Dr. Edwards; see Dr. Hodgkin's Trans. p. 486. The hypothesis of Dr. Stevens may be considered as resolving itself into the same general conclusion, although it is not so considered by the author. He supposes that the bright colour which the blood assumes in the lungs is owing not to the absorption of oxygen, but to the removal of carbonic acid from it, in consequence of the attraction which this gas possesses for oxygen, a fact which he conceives that he has demonstrated by independent experiments. He supposes that when the carbonic acid leaves the blood in the lungs, a portion of oxygen takes its place; but notwithstanding the interesting nature of the experiments, it appears to me that the hypothesis does not satisfactorily explain why this absorption takes place, the attraction being supposed to be exercised between the oxygen and the carbonic acid, not between the oxygen and the blood; See Proceedings of the Roy. Soc. for 1834, 5, p. 334, 5. Collard de Martigni performed an experiment like that of Edwards, except that he immersed the animals in azote instead of hydrogen; in like manner he found carbonic acid to be expired; Magendie's Journ. t. x. p. 111 et seq. He informs us that venous blood actually gives out a considerably larger quantity of carbonic acid than arterial blood. Dr. Alison remarks that the experiments of Edwards and Dulong, as well as those of Allen and Pepys, tend to prove that the oxygen which disappears in respiration is more than what is sufficient to form the carbonic acid that is generated; Cyclop. of Anat. v. i. p. 258.

A question still remains to be considered, whether, when the air enters the pulmonary vesicles, it is absorbed in its whole substance, that proportion of each of its constituents which is necessary for the wants of the system being retained, while the excess of each is rejected, or whether the quantity only be absorbed which is afterwards employed, consisting of a large proportion of oxygen and a small proportion of nitrogen. Sir H. Davy thinks that the whole of the air is absorbed, and that the surplus quantity of each of the constituents is afterwards discharged; he remarks, that air has the power of acting upon blood through a stratum of serum, and he conceives it probable, that in this case the whole mass must be absorbed before it can arrive at the red particles, upon which its action is specifically exercised[1]. But, although this view of the subject appears to be the most probable, yet we must not consider it as resting on any very decisive evidence[2].

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[1] Upon these experiments the author remarks, " il est indubitable, qu'elles produisent de l'acide carbonique, en respirant un gaz dépourvu d'oxigène ; que cet acide carbonique n'est pas dû à une quantité de ce gaz contenu dans la cavité des organes respiratoires avant l'expérience, ou à l'oxygene qu'ils peuvent renfermer ; par conséquent qu'il n'est pas formé de toutes pièces dans l'acte de la respiration, par la combinaison de l'oxigène de l'air inspiré avec le carbone du sang, mais qu'il est le produit de l'exhalation." p. 451.

[2] The conclusion from these experiments is ; " que l'acide carbonique expiré est une exhalation qui provient en tout ou en partie de l'acide carbonique contenu dans la masse du sang." p. 465. The experiments related in the text are contained in Dr. Edwards's work, par. 4. c. 16. § 4. p. 437..465. We have a direct experiment of Legallois in favour of the absorption of carbonic acid during respiration. He placed an animal in a quantity of air which contained a considerable proportion of carbonic acid, and, upon removing the animal, he found an actual diminution in the quantity of carbonic acid ; Ann. de Chim. et Phys. t. iv. p. 115. As an indirect argument in favour of the opinion maintained in the text, we may adduce the conclusion which Sir H. Davy formed from his experiments on the respiration of nitrous oxide and hydrogen ; " that a certain portion of the carbonic acid produced in respiration is evolved from the blood ;" Researches, p. 447. We have likewise some farther observations in p. 188. I must not omit to mention an experiment which has been performed by Magendie and by Orfila, in which when phosphorated oil was injected into the cellular texture or the blood vessels, the phosphorus has been expired in combination with oxygen ; see Mém. by Magendie on Trans. p. 19, 0 ; and Orfila's Toxicologie, t. i. p. 531 et seq. In this case it has been supposed by Dr. Prout more probable that the union of the phosphorus and oxygen should take place in the pulmonary vesicles than in the course of the circulation ; see Ann. Phil. v. xiii. p. 278 ; but the effect is of so peculiar a nature that it seems scarcely possible to reason from it to what takes place under ordinary circumstances. See the remarks of Dr. Alison, Physiol. p. 192, 3.

I have already made some observations on the supposed discharge of hydrogen from the lungs. The experiments and arguments that were employed by Lavoisier, to prove that the water contained in the expired air is generated by the union of oxygen and hydrogen, appear to be totally inadequate to the purpose, and accordingly the hypothesis itself, although at one time so very generally adopted, is at present, I conceive, entirely abandoned[3].

As the blood is a very compound fluid, composed of various substances that are loosely combined together, and possess different chemical properties, it has been a subject of inquiry, upon which of its constituents does the air more particularly act. According to the hypothesis which supposes the lungs to be the seat of the operation, the inquiry will be, from what part does the oxygen procure the carbon, and according to the other hypothesis, by what part is the oxygen attracted. Of the two substances into which the blood separates by its spontaneous coagulation, the crassamentum and the serum, the latter appears to be similar, in its chemical relations, to many other parts of the body, and has not been found to possess any specific or peculiar chemical properties[4], whereas the former, when employed separately, has the power of acting upon the air in the same manner with the entire mass of blood. Hence therefore we infer that the crassamentum is the great agent in bringing about the change which is effected by respiration. The crassamentum itself is, however, composed of fibrine and red particles, and as the former of these has precisely the same chemical properties with the muscular fibre, which does not appear to possess any relations peculiar to itself, we naturally regard the red globules as that part of the crassamentum on which the air more particularly acts[1]. Their organization is peculiar to themselves, they are the only part of the blood which is known to possess any specific chemical characters; we have reason to suppose that they are easily decomposed, and are more readily acted upon than either the serum or the fibrine, and it is principally by their change of colour that we are enabled to form our judgment respecting the action of the air upon the blood. The nature of this action is, however, obscure, and we know nothing more than that they appear to have a strong attraction for oxygen, for, although it has been shown that they contain a small quantity of iron, there appears no foundation for the opinion, which at one time prevailed, that the iron is the part by which the oxygen is attracted[2].

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[1] Researches, p. 447.

[2] Sir H. Davy's experiments on the respiration of nitrous oxide have been adduced in favour of this opinion, because they have been thought to prove that nitrogen was generated by this process, which it has been supposed could only have taken place by the decomposition of the nitrous oxide after it had been previously absorbed by the blood; Researches, p. 412 et seq.; and an argument was drawn from this in favour of the absorption of atmospheric air by the blood. There are, however, several points in these experiments, with respect to the capacity of the lungs in their different states of distention, as well as the relation which they bear to the quantity of air inspired, which require to be re-considered, before we can admit the conclusion that is deduced from them. See note 50 of the Essay on Respiration.

[3] I may observe, that upon either hypothesis concerning the mode in which the oxygen unites with the carbon, the water was equally supposed to be generated by the union of oxygen and hydrogen, although they differ in the one being a rapid union effected in the lungs, the other a more slow process carried on during the course of the circulation.

[4] Berzelius observes, that "serum absorbs very little oxygen;" Med. Chir. *Tr. v. iii. p.* 232.

Some other circumstances have been pointed out, in which arterial differs from venous blood; it has been stated, for example, that it contains less water and crassamentum. But, even, if we admit the facts, which are perhaps not very completely established, this difference might be attributed rather to the effects of secretion and transudation, than to what is to be regarded as the proper action of the lungs. The consideration of these differences between the two states of the blood will therefore be better understood, when we have considered the nature of the secretions, as well as of the substance from which the blood itself is produced, and have compared, as far as is in our power, the chemical relation which these bodies bear to each other. I shall conclude this section by recapitulating the changes which, according to the present state of our knowledge, the blood appears to undergo by respiration, after premising that our information upon this subject is still in a very imperfect state, and that, in most cases, we arrive at our conclusions, rather by indirect inferences, than by any direct experiments that can be made upon the blood itself.

1. The blood, when it leaves the right side of the heart, is of

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[1] Young's Medical Literature, p. 503. The curious discovery of Messrs. Dumas and Prevost, that the temperature of an animal is in exact proportion to the quantity of red globules which exist in its blood, may afford an indirect proof of this opinion; Ann. Chim. et Phys. t. xxiii. p. 64 et seq. See also Dr. Prout, Ann. Phil. v. xiii. p. 270.

[2] The opinion that the iron in the blood is the constituent on which the air more specifically acts, was generally adopted by the physiologists of the last century; see Haller, El. Phys. vi. 3. 18; and at one time appeared to be proved by the experiments of Fourcroy and Vauquelin, who pointed out the state of combination in which it exists, and the nature of the change which was effected upon it by the air; Fourcroy's System by Nicholson, v. ix. p. 207..0; but, notwithstanding the high authority of these chemists, there appears to have been some inaccuracy in their statement; see the experiments and reasoning of Wells; Phil. Trans. for 1797, p. 427 et seq.; also my remarks on the iron in the blood, in p. 283 et seq. I have already made some remarks on the part which the salts of the blood are supposed to act in this process, in conformity with the discovery of Dr. Stevens.

a purple colour; during its passage through the lungs it is converted into a bright scarlet, and again acquires the purple colour when it arrives at the venous part of the circulation. 2. This change from purple to scarlet is effected by the oxygen of the atmospheric air, which is received into the vesicles of the lungs. 3. The same change of colour may be produced upon the crassamentum of the blood out of the vessels, by exposing it to atmospheric air, or still more to oxygen, while, on the contrary, scarlet blood is rendered purple by exposure to hydrogen, nitrogen, or carbonic acid. 4. The blood, in passing through the lungs, discharges a quantity of carbon, which is expired in combination with oxygen, under the form of carbonic acid gas. 5. A quantity of aqueous vapour is discharged from the lungs, but this is rather to be considered as the result of secretion or transudation, than as a proper effect of respiration. 6. The blood, in passing through the lungs, absorbs a portion of oxygen, and this appears to be more than what is necessary for the formation of the carbonic acid which is discharged. 7. It is probable that the blood, as it passes through the lungs, both absorbs and exhales nitrogen, the proportion which these operations bear to each other being very variable, and depending upon certain states of the system, or upon the operation of external agents. 8. It appears upon the whole, probable, that the atmospheric air is absorbed by the blood in its whole substance, and that certain proportions of each of its ingredients are discharged or retained according to the demands of the system. 9. We have no proof that hydrogen is discharged from the blood[1].

SECT. 5. *On the Respiration of the different Gases.*

It may be presumed that no gaseous body, except the compound of oxygen and nitrogen which constitutes the atmosphere, is adapted to the permanent support of life. Of the other gases, there are some which are, properly speaking, unrespirable, which, on account of the irritation they produce in the upper part of the trachea, it is impossible to take into the lungs; but there are others, which may be received into the pulmonary cavities, although their employment is followed sooner or later by some derangement of the system, or even by the extinction of life. The gases upon which experiments of this kind have been made are oxygen, nitrous oxide, nitrogen, hydrogen, and carburetted hydrogen.

The first account which we have of the effect of oxygen, when respired in its unmixed state, is given us by Priestley, who almost immediately upon his discovery of this substance, perceived its remarkable capacity of supporting life, and tried the effect of it upon his own person: he informs us that he felt an agreeable lightness in his chest[1]; but nothing particular appears to have resulted from the trial, and it may be fairly questioned, whether the sensation which he described is not to be attributed rather to a mental, than to a physical impression. Some individuals who respired oxygen, conceived that it even produced exhilarating effects, while others describe it as giving rise to pain and uneasiness in the thorax. But it can scarcely be doubted, that much of what was described depended upon the imagination, while something was probably owing to the impurity of the gas that was employed, or to the unusual efforts which were made to take it into the lungs. Priestley was also the first who tried the effect of the respiration of oxygen upon animals that were immersed in it; but his experiments went no farther than to prove the superior power which it possesses of supporting life. He remarks indeed that after an animal had expired in a portion of oxygen, a second animal was able to live for some time in the same air, and it has been inferred from this circumstance, that there must have been something noxious in the gas, which the powers of the constitution were unable to resist for more than a certain length of time. He himself supposed that the death of the mice, the animals which he employed in his experiments, was owing to cold, in consequence of passing through the water with which the gas was confined; he accordingly found that by keeping up the temperature, he was able considerably to prolong the life of the animal, and he remarks, that this experiment completely convinced him that there was nothing in the nature of the gas itself which prevented the mice from living in it[2]. But another and a more efficacious cause may be assigned for the death of the first animal, that the carbonic acid which was generated, and of which a small portion only would be absorbed by the water, must have acted more powerfully upon the system exhausted by having been for some time exposed to its influence, than upon a fresh and vigorous animal, who would be able for a short period to bear the effect with impunity. Exactly the same occurrence has been observed by Morozzo and others[3] to take place in the respiration of a limited quantity of atmospheric air, and, on the contrary, it has been found by Lavoisier and others, that when the carbonic acid was removed by potash, as fast as it was produced, this effect did not take place [1].

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[1] I have not entered upon the question respecting the change of its capacity for heat, which the blood has been supposed to experience in its passage through the lungs, because it will fall under our notice, with more propriety, in the next chapter.

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[1] On Air, v. ii. p. 162.

[2] On Air, v. ii. p. 165.

[3] Jurine, Enc. Méth. "Médecine," t. i. p. 496; Chaptal's Chem. v. i. p. 131, 2; the experiments of Morozzo on this subject appear to have been made with sufficient accuracy; Journ. de Phys. t. xxv. p. 102 et seq.; in one set of experiments a bird lived six hours and a half in a certain quantity of oxygen, and a second lived two hours and five minutes in the same gas; in another set of experiments, the first lived five hours and twenty-three minutes, and others lived in succession in the same gas, as far as the tenth, which lived twenty-one minutes in the same air that had proved fatal to the nine birds that had been previously immersed in it.

The next account that we have of the respiration of oxygen is by Lavoisier. In his first experiments on this subject he examined the state of the internal organs of an animal, after having been for some time confined in this gas, and he conceived that their appearance indicated that there had been an increased action of the sanguiferous system produced, or something which indicated an approach to the inflammatory state [2]. We may, however, presume that these appearances must have been the consequence of some accidental cause, for the same philosopher, in his subsequent experiments, which were performed with a more perfect apparatus, and with every appearance of great accuracy, and where the respiration was continued for a much greater length of time, informs us that neither the circulation nor the temperature were affected by it; and in short that no perceptible change was produced by it upon the animal [3]. This conclusion is the more worthy of our attention and the more to be confided in, as it not only indicates a change in Lavoisier's opinion, but is unfavourable to the analogy which he wished to establish, between the effects of respiration and combustion.

We have some experiments on the respiration of oxygen by Higgins; he informs us that the pulse was quickened, and that a sensation of warmth was experienced at the chest [4]; but it may be reasonably inferred that these effects were as much owing to the mechanical method in which the gas was inspired, as to any specific operation produced by the nature of the gas. Dumas relates a series of experiments which he performed on this subject, that were attended with very different effects. He had formed an opinion that the lungs possessed a great degree of irritability, and in order to put it to the test, entered upon the investigation, expecting that the irritability would be rendered more peculiarly obvious by the action of oxygen upon them. A dog was accordingly confined in this gas, and the apparatus was so contrived that the air, as it became vitiated, might be withdrawn, and a fresh portion substituted in its place; the process lasted for ten hours, and was resumed after a certain interval, and again resumed for the same length of time for several successive days. The animal now began to be much affected, and various symptoms of disease connected with the chest became manifest, and, upon examining the part after death, the lungs were found considerably affected, and even ulcerated, so as to exhibit the symptoms of incipient phthisis[1].

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[1] We are informed by Dr. Edwards, contrary perhaps to the opinion which is generally adopted, that when warm-blooded animals are confined in a limited quantity of air, they always deoxidate it to the same degree, and that a second animal introduced into the same air expires immediately; De l'Influence &c., p. 184. But although the air is ultimately all reduced to the same standard, the effect is produced in very different intervals of time, depending upon various circumstances connected with the constitution of the individuals.

[2] Mém. Soc. Roy. Méd. pour 1782, 3, p. 576.

[3] Mém. Acad. Sc. pour 1789, p. 573.

[4] *Minutes* of a Society &c. p. 144..6, p. 152.

The next account that we have of the effects of the respiration of oxygen is by Beddoes. He performed a series of experiments upon rabbits, in which the attention was particularly directed to the state of the internal organs. He had formed a previous hypothesis, that by the long continued respiration of pure oxygen, a greater quantity of it was absorbed by the blood, than under ordinary circumstances, and that the whole system was, in this way, capable of becoming, as he terms it, oxygenated. The appearances which he found in the animal after death were accordingly such as seemed very strongly to confirm the hypothesis; the lungs were florid, the pleura exhibited marks of inflammatory action, the heart retained its irritability longer than usual; the blood coagulated more rapidly, and indeed the system was so completely saturated with oxygen, that the animals were less easily destroyed by immersion in hydrogen gas or even in water[2]. These results are in themselves very remarkable, and must appear the more so when contrasted with the opposite statements of Lavoisier[3]; and it is to be further borne in mind, that the experiments of Beddoes were continued for a considerably less space of time than those of Lavoisier, and likewise that he expressly informs us, that notwithstanding these very singular effects which were produced on the system, the oxygen in which the animals had been confined "seemed to have suffered little diminution either in quantity or quality."[1] I think that it will not be deemed an uncandid or unfair conclusion to suppose that he was misled by his preconceived hypothesis, and that his zeal in the prosecution of a favourite project caused him to form an erroneous estimate of the appearances which presented themselves to him, or to overlook some circumstances which would have afforded a more natural explanation of them.

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[1] Physiol. t. iii. p. 59 et seq.; the experiments, as the author informs us, were made in the year 1791. Richerand also says, p. 211, that an animal, if long immersed in oxygen, has its circulation quickened, the respiration rendered more frequent, with marks of general excitement. Yet, it is remarkable, he informs us, that notwithstanding these effects, an animal confined in oxygen consumes no more of it than when in atmospheric air; ibid.

[2] On Factitious Airs, part 1. p. 13 et seq., p. 38.

[3] Beddoes quotes the authority of Lavoisier and Priestley in favour of his opinion; on Fact. Airs, part 1. p. 13; and Observations on Calculus, &c. p. 136..8. We have already seen what is the case with respect to Lavoisier; as to Priestley, the only passage in his works, that I conceive can be brought forwards in this connexion, is one in which it is stated as a mere conjecture, that "as a candle burns out much faster in dephlogisticated than in common air, so we might, as it may be said, live too fast, and the animal powers be too soon exhausted in this pure kind of air;" On Air, v. ii. p. 168; but this, although a plausible conjecture at the time when it was formed, can have no weight against the direct experiments that have been since made by Lavoisier and others. The results which he obtained and his opinions upon them were so explicit, that it is surprising Dr. Beddoes should have ventured to adduce them in support of his doctrine of the oxygenation of the blood. I shall quote Lavoisier's remarks at some length. "On sait que la combustion, toutes choses égales d'ailleurs, est d'autant plus rapide, que l'air dans lequel s'opère, est plus pur. Ainsi, par example, il se consomme dans un temps donné beaucoup plus de charbon ou de tout autre combustible, dans l'air vital, que dans l'air de l'atmosphére. On avoit toujours pensé, qu'il en etoit de même de la respiration; qu'il devoit s'accélérer dans l'air vital, et qu'alors il devoit se dégager soit dans le poumon, soit dans le cours de la circulation, une plus grande quantité de calorique. Mais l'expérience a detruit toutes ses opinions qui n'etoient fondés que sur l'analogie. Soit que les animaux respirent dans l'air vital pur, soit qu'ils respirent ce même air, melangé avec une proportion plus ou moins consi-

Sir H. Davy has given us the results of some experiments on the respiration of oxygen, and he is induced to infer from them that the long continued employment of this gas would ultimately destroy life; but the grounds on which he formed this opinion are not very explicitly stated, and, upon examining the nature of his results, it would appear that the injury done to the system could not arise from the excessive absorption of oxygen, because both when he performed the experiment upon his own person and upon mice, the quantity of oxygen consumed was less than from the use of common air[2]. Messrs. Allen and Pepys, among their other experiments, tried the effect of the respiration of oxygen; between 3 and 4000 cubic inches of the gas were respired during a period of from 7 to 9 minutes, and the result

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derable de gaz azote, la quantité d'air vital qu'ils consomment, est toujours la même, à de très lègeres differences près. Il nous est arrivé plusieurs fois, de tenir un cochon d'Inde pendant plusieurs jours, soit dans l'air vital pur, soit dans une melange de quinze parties de gaz azote et d'une d'air vital, en entretenant constamment les mêmes proportions; l'animal dans les deux cas est demeuré dans son état naturel; sa respiration et sa circulation ne paroissoient pas sensiblement, ni accelerées, ni retardées; sa chaleur etoit égale, et il avoit seulement, lorsque la proportion de gaz azote devenoit trop forte, un peu plus de disposition à l'assoupissement." Mém. Acad. Sc. pour 1789, p. 573. Beddoes's experiments on the respiration of oxygen were performed for the purpose of establishing his opinion, that the morbid state of the lungs in phthisis might be relieved by the respiration of an air less oxygenated than that of the atmosphere. It is remarkable that an opinion of an exactly opposite nature was, about the same time, broached in France, according to which a more oxygenated air was recommended in these complaints, upon the principle, that, as the structure of the lungs was injured, they would require an atmosphere from which they might more readily obtain their due proportion of oxygen. The results of the experiments, according to the reports which we have of them, appear to have been equally favourable to each hypothesis; we may fairly conclude, that both plans would be equally unavailing; see Fourcroy, Ann. Chim. t. iv. p. 83 et seq.; Jurine, Enc. Méth. art. "Médecine," t. i. p. 500; Chaptal's Chem. v. i. p. 138.

[1] On Fact. Airs, part i. p. 13.

[2] Researches, p. 439..444. We may presume that this remarkable result was not the effect of inaccuracy or inadvertence, because he expresses his *surprise* at the circumstance.

was that a glow of heat was felt over the body, attended by a gentle perspiration. It appeared that, during the respiration of oxygen, more carbonic acid was formed than under ordinary circumstances, and also that a portion of oxygen was consumed more than sufficient for the production of the carbonic acid, while it appeared that a corresponding quantity of nitrogen was disengaged from the blood. The proportion of carbonic acid formed during the respiration of oxygen appears to have been 54201·6 cubic inches, while that produced under ordinary circumstances was no more than 39534 cubic inches[1].

The only remaining experiments on this subject with which I am acquainted are those of Mr. Broughton. He operated upon various kinds of animals, and he found that they always expired before the air was completely deoxidized or carbonated, that the blood was still florid, and that symptoms of congestion or excitement were produced[2]. The results are favourable to the opinion, which appears to have been generally adopted, both in this country and in France, that the respiration of oxygen has a tendency to increase the action of the heart and arteries, and even produce an inflammatory state of the system; but, upon reviewing the whole of the evidence, I confess that it appears to me scarcely sufficient to warrant the conclusion[3]. With respect to the earlier experiments, it may be fairly conjectured that the gas upon which they operated was not pure, from the mode in which it was procured[4]; and the experiments of Messrs. Allen and Pepys, which, in this respect are unexceptionable, labour under the disadvantage, which has been already pointed out, that the respiration was not performed in the natural mode, as was the case in those of Lavoisier, where the contrary result was obtained.

Of the gases, which although not capable of supporting life are still respirable, the one which is the least injurious to the system appears to be nitrous oxide. Priestley, who discovered it, supposed it to be highly noxious to animals[1], and the associated Dutch chemists, who afterwards examined its properties, coincided with him in this opinion[2]. Sir H. Davy, however, found that this gas may be respired for a short interval without proving fatal[3], and, at the same time, made the curious discovery that the employment of it produces a powerful excitement of the nervous system, in many respects resembling that from alcohol, but differing from it in its not being succeeded by a state of exhaustion[4]. In addition to his own experience, we have a number of very interesting details of its effects upon other individuals[5]; and the experiment has been now so frequently repeated, that although in certain constitutions the peculiar excitement cannot be perceived, and, in some instances, there seems to be even a sedative effect produced[6], yet no doubt can remain of the general truth of the fact. Sir H. Davy has rendered it probable, that nitrous oxide, when it is taken into the lungs, is absorbed by the blood; he conceives also that it is decomposed by it, an opinion, however, which is scarcely sanctioned by the experiments.

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[1] Phil. Trans. for 1808, p. 265, and p. 277; and for 1809, p. 415 et seq., and p. 427. They found the diminution in the volume of the air to be greater than from the respiration of atmospheric air; in the latter case they assume the average diminution to be about 1-166th, whereas, in the respiration of oxygen, the diminution in one case was 1-44th; Phil. Trans. for 1808, p. 277.

[2] Inst. Journ. No. 13. Ap. 1830. See also Dr. Hodgkin's remarks in his appendix to his translation of Edwards's work, p. 486, 7; and those of Dr. Alison, Art. "Asphyxia," in the Cyc. of Anat. and Phys. v. i. p. 257.

[3] Magendie, whose opinion may be justly esteemed as a specimen of what is regarded as of the highest authority in France, remarks upon this subject, that pure oxygen is fatal to life, and even when mixed in different proportion from that in which it exists in the atmosphere, that it sooner or later causes the death of the animals that are confined in it; Physiol. t. ii. p. 295. As he refers to no particular authorities, we may regard this as the current doctrine among his countrymen. Dr. Prout also supports the same doctrine, Ann. Phil. v. xiii. p. 266, but without stating the grounds of his opinion.

[4] We have some useful observations on this point in Cavallo on Factitious Airs, c. 9.

Hydrogen has been frequently respired[7], and the general conclusion which we are led to form is that it possesses no positively noxious effects, and only acts by excluding oxygen, a conclusion which appears necessarily to follow from the experiments of Lavoisier[8], Sir H. Davy[9], and Messrs. Allen and Pepys[10]. A contrary opinion has indeed been maintained by Priestley[11], and some other chemists; but it may be presumed that the gases upon which they operated were not sufficiently pure, as their experiments were performed in the earlier periods of the pneumatic chemistry, and we learn from Sir H. Davy[12], that a considerable difference of effect is produced according to the method in which the gas is procured.

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[1] On Air, v. ii. p. 55.

[2] Journ. Phys. t. xliii. p. 329, 332.

[3] Researches, p. 333..360, p. 425 et seq.

[4] Ibid. p. 456 et seq.

[5] Ibid. p. 497 et seq.

[6] Thenard, Chem. t. iii. p. 674, 5, gives an account of its effects upon Vauquelin. I may add that I experienced the same feelings in my own person; the first inspiration of the oxide produced a sensation like that of fainting, which quickly proceeded to insensibility. Sir H. Davy informs us that the appearances which were exhibited by the lungs of animals that had expired in nitrous oxide were similar to what Beddoes had found in the lungs of animals that had been confined for some time in oxygen; Researches, p. 356; a circumstance which, I conceive, affords a pretty strong presumption that the oxygen employed was not pure; this may perhaps assist us in explaining the extraordinary effects which were supposed to be produced by breathing this gas.

[7] Scheele, on Air and Fire, p. 160; Fontana, in Phil. Trans. for 1779, p. 337; and Journ. Phys. t. xv. p. 99 et seq.; Pilatre de Rozier, Journ. Phys. t. xxviii. p. 425.

[8] Mém. Acad. Scien. pour 1789, p. 574.

[9] Researches, p. 465, 6.

[10] Phil. Trans. for 1809, p. 421, 427; see also Beddoes on Fact. Airs, part i. p. 30 et seq.; p. 42.

[11] *On* Air, v. i. p. 229.

[12] Loco citat.

Pure nitrogen, like hydrogen, appears to act merely by the exclusion of oxygen, an opinion which might naturally be formed respecting a substance that enters so largely into the composition of the atmosphere, for it would seem scarcely possible that any thing which had a positively noxious influence should be at all times received into the lungs in such large quantity. Higgins indeed states that animals die sooner from immersion in nitrogen than from mere interruption to respiration[1], but he does not inform us upon what facts this opinion was founded. Sir H. Davy also appears to have experienced a greater sense of suffocation from respiring nitrogen than hydrogen, but the gas which he employed contained a portion of carbonic acid[2], and he expressly states that immersion in nitrogen or hydrogen proves fatal merely by the exclusion of oxygen, in the same manner with submersion in water[3]. And this view of the subject, if it stood in need of farther support, would appear to be confirmed by the view which we have taken of the action of the air upon the blood, an essential part of which consists in the absorption of nitrogen.

The only remaining gas which is capable of being received into the lungs is carburetted hydrogen. If it be respired in an undiluted state, it seems to act as a direct sedative, producing instant death; and if it be employed in small proportion only, diffused through atmospheric air, it induces vertigo, loss of perception, and other symptoms which indicate the extinction of the vital powers[4]. It acts more rapidly than those gases which merely exclude oxygen, or than the mechanical causes which prevent the admission of this gas into the lungs; and it must therefore be considered as possessing a directly injurious effect upon the animal œconomy. Physiologists are not agreed respecting the mode in which this gas operates; the opinion at one time prevalent, that it acts chemically by abstracting oxygen from the blood, seems to be supported only by a kind of loose analogy, and it is, upon the whole, more probable, that it operates immediately upon the vital powers of the system, destroying either the contractility of the muscles or the sensibility of the nerves, or perhaps both of them, by a direct agency.

All the remaining gases are strictly unrespirable; it is obvious that this must be the case with the irritating acid and alkaline gases, but it may appear remarkable that this should be the case with carbonic acid; although however it must at all times exist in considerable proportion in the air which is contained in the lungs, we learn from the experiments of Pilatre de Rozier[1] and Sir H. Davy[2] that, in an undiluted state, it cannot be taken into the trachea, even by the most powerful voluntary efforts. Sir H. Davy found that air was still unrespirable when it contained three-fifths of its volume of carbonic acid, but that when the proportion was diminished to 3 parts in 10, it might be received into the lungs; the effect which it produced, after being breathed for a minute, was a slight giddiness and a tendency to sleep[3].

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[1] Minutes of a Society, p. 133.

[2] Researches, p. 466.

[3] Ibid. p. 335.

[4] Ibid. p. 467 et seq.

Dumas has given us a detail of some experiments which he performed on the respiration of dogs in carbonic acid; his expression would lead us to conclude that he employed pure carbonic acid, but this could not be the case, as the animals lived in the air for a considerable length of time; after death their lungs were found to be in a state which seemed to have been produced by inflammation, but it is very difficult to draw any accurate conclusion from the narrative[4].

SECT. 6. *The remote Effects of Respiration on the Living System.*

Having now given an account of the direct effects of respiration, I shall next proceed to consider its remote effects upon the living system.

This inquiry must be considered as in fact identical with an investigation into the uses of respiration, for, according to the conception which we are led to entertain of the structure and powers of the living body, we conceive that every action which it performs must produce some useful purpose in its œconomy, and be essential to the existence and well-being of the whole. But although no object to which the human mind can be directed is so interesting and delightful as tracing out the final causes of the phenomena which we observe, it has been found by experience that it is not the most appropriate method of arriving at a correct knowledge of the facts themselves. On this account I have thought it more desirable, in a work like the present, to ascertain, in the first instance, as far as the present state of our knowledge will admit of it, the exact nature of the operations of the animal œconomy, and either altogether to leave the application to be made by the reader, or at least to consider this only as a secondary object of our attention.

The remote effects of respiration will naturally arrange themselves under two heads, those which more immediately affect the vital functions, and those the operation of which is more of a mechanical nature.

Among the remote effects which have been ascribed by the old writers to the function of respiration, there are some, which are so obviously founded upon incorrect principles, that it will not be necessary to enter upon the examination of them, or to do more than merely to allude to them, as forming a part of the history of science. The following, however, are more deserving of our attention, either as being supported by direct experiment, or as having received the sanction of some of the most eminent modern physiologists. The effect of respiration in producing heat, in preserving the contractibility of the muscles, in preventing the decomposition of the body, in promoting the process of sanguification, in the formation of the voice and the various sounds emitted from the larynx, and in the mechanical operations depending upon the motion of the thorax, or upon its connexion with the contiguous viscera[1].

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[1] Journ. Phys. t. xxviii. p. 422 et seq.

[2] Researches, p. 472.

[3] *Researches*, p. 473.

[4] Physiol. t. iii. p. 62.

The first of these effects, the production of animal heat, on account of the great extent of the inquiry into which it must necessarily lead us, and still more, from the peculiar, and as it were, specific nature of the operation, I shall consider in the following chapter, as a distinct function. In all warm-blooded animals, where an uninterrupted continuance of the respiration is essential to life, we find that the first effect which ensues from a deficiency of the supply of unrespired air, is the cessation of the contraction of the heart. If we examine the heart when it is in this state, we shall find that its capillary arteries are filled with blood which exhibits the purple venous aspect, and by observing the coincidence between the contractility of the muscular fibres and the nature of the blood which is sent to their minute vessels, we seem to be warranted in the conclusion, that a regular supply of arterialized blood is essential to the support of their contractility, and that the deficiency of this species of blood is the immediate cause of the change which they experience when the respiration is impeded. Goodwyn's observations and experiments were directed to this object, and they fully substantiate his hypothesis, so far as the general question is concerned, respecting the nature of "the connexion of life with respiration."[2] But the mode of accounting for this change will necessarily depend on the opinion which we adopt respecting the direct effect of respiration upon the blood. If we suppose that this function acts merely in abstracting a portion of carbon from it during its passage through the lungs, we must conclude that the presence of this superabundant carbon in the blood prevents it from preserving the muscles in their contractile state, or if we think it more probable that the oxygen is absorbed in the lungs, we may conceive that the want of contractility is owing to the combined influence of the absence of the ordinary proportion of oxygen and the excess of that of carbon. And whichever supposition we may adopt, it is very possible that some other change may have taken place in the blood, either in its chemical or its mechanical constitution, which may render it no longer fit for the continuance of its appropriate functions, although of the nature of such change we are entirely ignorant. The production of animal heat, should it appear that this is one of the effects of respiration, is essentially connected with the discharge of a portion of carbon, and it will thus follow that these two processes ultimately depend upon the same operation, and that this is to be resolved into a change in the chemical, and possibly also a consequent change in the mechanical nature of the blood.

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[1] Sœmmering enumerates the following uses of respiration; Corp. Hum. Fab. t. vi. § 72. 1. To promote the circulation by a mechanical action; 2. to mix together the components of the blood; 3. to condense the blood by discharging a portion of aqueous vapour; 4. to promote the secretions and excretions by pressing upon the viscera; 5. to assist in chylification; 6. to enable us to exercise the sense of smell; 7. to enable the infant to suck; 8. to enable the lungs to inhale; 9. it is doubtful whether the body acquires electricity by respiration; 10. by respiration the blood is purified and prevented from putrifying; 11. it maintains the temperature of the body; 12. assists in sanguification; 13. is necessary for the voice and speech. Probably some of the above uses may be thought very problematical; but in addition to them we have the various indirect mechanical purposes which is served by the respiration, which will be enumerated hereafter.

[2] See the 5th Sect. of his Essay; also Young's Lectures, v. i. p. 739; where the author remarks, that "the muscles are furnished by the blood with a store of that unknown principle, by which they are rendered capable of contracting." Spallanzani goes farther, and states that the oxygen which the blood absorbs, unites with the muscular fibres of the heart and endows

The share which the nerves have in respiration, or the connexion which subsists between the nervous system and the lungs, has long been a subject of controversy, and has given rise to many elaborate dissertations, as well as to numerous experiments[1]. This question being intimately connected with the effects that result from dividing the par vagum, it may be proper to introduce into this place an account of the facts that have been ascertained, and of the opinions which have been formed respecting this operation.

These nerves[2], from the peculiarity in their anatomical rela-

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it with its contractility; Mémoires, p. 327; but his hypothesis is deduced from very insufficient premises.

[1] I have already had occasion to refer to the opinion of Flourens, concerning the connexion between the spinal cord and the function of respiration. He remarks that the part of the cord, the division of which affects this function, is situated higher in fish than birds, corresponding to the origin of the nerves of the gills and the ribs respectively; Ann. Sc. Nat. t. xiii.

[2] There has been some difference among anatomists in the nomenclature which they have employed with respect to these nerves. The term "8th pair" is considered by many as synonymous with "par vagum;" Boyer, Anat. t. iii. p. 350, 1; while others, as it would appear with more accuracy, regard the par vagum, (or, as they have been named by the French physiologists, the pneumo-gastric nerves,) as only the principal branch of the 8th pair. See the synoptical table in Bell's Anat. v. iii. p. 113, 4. also the remarks of Dr. Elliotson, Med. Chir. Tr. v. xix. p. 226. For a description of the part it may be sufficient to refer to Winslow's Anat. Sect. 6. 104..142 (he terms them nervi sympathetici medii); Bell's Anat. v. iii. p. 153..9; Sœmmering, Corp. Hum. fab. t. iv. § 259; et De Bas. Enceph. in Ludwig, t. ii. § 84..6; Bichat, Anat. Des. t. iii. art. 3. § 2. p. 209..222; Desmoulins, Anat. des Syst. Nerv. p. 438; and Cloquet's Anat. by Knox, p. 471..6; and for a delineation of it to Vicq d'Azyr, pl. 17, 18; Sœmmering's plate of the base of the brain, for

tions, have been, at all times, an object of great interest to physiologists. The other cranial nerves are primarily destined to the organs of sense, or to some of their appendages, whereas these nerves are carried to a considerable distance from the head, and are distributed over certain viscera in the thorax and abdomen. This singular destination indicated something peculiar in the functions of the parts to which they are appropriated, while, at the same time, the form and situation of the nerves rendered them particularly favourable for investigating the uses which they serve, as it was easy to deprive the organs of the nervous influence, without the risk of injuring the parts, or affecting them in any other manner. We accordingly find that the division of the par vagum is among the oldest of the physiological experiments that are upon record.

In consequence of the connexion which these nerves have with the recurrents, the effects of their division have been often confounded together, and it was not until comparatively of late years, that we were aware of the difference between their functions, or attempted to separate them from each other. We now know, that by dividing the recurrent nerves, the action of the glottis is deranged, and the voice destroyed or materially impaired, and this was supposed by the ancients to be the chief effect of dividing the par vagum. It was found, however, that other functions were injured, particularly the circulation, the respiration, and the digestion, and that death was, sooner or later, the consequence of the operation [1]. The earlier among the modern physiologists appear, for the most part, to have directed their attention to the effects that were produced upon the heart, and, for a long time, the principal subject of discussion was how far the division of the nerves suspended or destroyed the action of this organ. But, although many experiments were performed, and their effects described, the observations were not made with that degree of accuracy, which can enable us to draw any correct deductions from them, nor indeed were the observers themselves aware of the circumstances which it was neces-

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its origin; C. Bell's Dissect. pt. 1, pl. 8, and to his engravings of the nerves, pl. 2 and 3.

[1] We shall find a very considerable degree of irregularity in the results of the experiments that have been performed on the division of the par vagum. In most cases the death of the animal, or the destruction of some important function, being the evident and direct consequence of the operation, while, in some instances, little more appears to have ensued from it, than what might be referred to the pain and irritation produced by the operation. These anomalies are probably, in a great measure, to be referred to the curious discovery of Dr. Philip, to which I have already alluded, that when a nerve is divided, and the ends remain in opposition, or even when they are separated from each other by a small interval only, the nervous influence continues to be transmitted along it with little or no interruption. Dr. Alison remarks, that the section of the cord above the phrenic nerves causes death directly by asphyxia while the division of the par vagum causes death indirectly, by producing disease of the lungs; Cyclop. of Anat. v. i. p. 258, 9.

sary to attend to, in order to attain a full insight into the subject [1].

One of the earliest among the moderns, whose ideas respecting the nervous system assumed a more matured form, was Willis. He divided the par vagum, in order to obtain a test of the truth of his doctrine, that the involuntary motions of the body proceed more immediately from the cerebellum, and having found, in conformity with his preconceived opinion, that the circulation was considerably affected, he did not particularly attend to the effects of the operation upon the other organs [2].

Haller's experiments led him to suppose, that the stomach and the lungs were the organs that were more immediately affected by the division of the par vagum, but he has not explained how the effect is produced, what relation the parts bear to each other, or how they act in causing the death of the animal [3]. With respect to the circulation, it seemed to be the general opinion, that the derangement produced in this function was neither so considerable nor so uniform as it should have been, had the heart been the part primarily affected. And, with respect to the stomach, although perhaps no organ seemed to sustain more injury, yet life was destroyed more rapidly than it would have been, had the digestion alone been the function that was suspended.

The lungs were therefore concluded to be the organ, the derangement of which was the immediate cause of death, an opinion that seemed to be confirmed by some experiments of Bichat's [4], and now the discussion took place respecting the

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[1] We have a very interesting historical detail given us by Legallois of all the experiments which have been performed on the par vagum, from the time of Rufus (who appears to have been the first anatomist who tried the effect of dividing or compressing these nerves) to the date of his own publication in 1812. I have thought it sufficient to notice those only which were made for the purpose of establishing some new principle, or which lead to some important conclusion; see his work "Sur le Principe de la Vie," p. 164 et seq.; see also Mr. Broughton's sketch of these experiments, prefixed to his paper in the Quart. Journ. v. x. p. 292. We have also a list of the authors, with references, in Breschet's paper in "Archives de Médecine," Aug. 1823.

[2] Cerebri Anat. cap. 24. p. 127. It may be interesting to recite the names of the physiologists who successively occupied themselves with this inquiry, for the purpose of showing the great interest which was attached to it. The following list is taken from Legallois, p. 170: Chirac, Bohn, Duverney, Vieussens, Schrader, Valsalva, Morgagni, Baglivi, Courten, Berger, Ens, Senac, Heuermann, Haller, Brunn, and Molinelli; it appears that, in these cases, the circulation was the function that was more particularly attended to, and that, in a great measure, with reference to Willis's hypothesis. To these may be added Riolanus, Plempius, Lower, and Boyle, who preceded, or were contemporary with, Willis; Haller refers to his relative Brunn, as having performed many experiments on the par vagum. Haighton's experiments on these nerves will be noticed in a subsequent section.

[3] Haller's experiments are contained in his work "Sur la Nature sensible et irritable des Parties du Corps animal;" No. 181, 182, 185, 186, 188. His opinion respecting the effect of the operation is stated in El. Phys. iv. 5. 2.

[4] *Sur la Vie*, &c. p. 2. Art. 10. § 1 p. 221..224.

s in which the division of the par vagum could act upon the
s, so as to prevent them from performing their functions.
umber of experiments were accordingly made to elucidate
point, which, in consequence of the improved state of phy-
gical science, and of the greater dexterity and precision of
perators, were attended with results, that are much more
esting and satisfactory than those of the older anatomists.
are chiefly indebted to the ingenuity and diligence of the
ch physiologists for the information which we possess upon
topic, and more particularly to the investigations of Dupuy-
Dumas, Blainville, Provençal, and Legallois [1].

e points that were more particularly attended to were to
tain what is the exact state of the lungs, whether they re-
the air as usual into their cavities, and whether the same
ical change was induced upon the air as in ordinary re-
tion. A preliminary step of the investigation, the import-
of which seems to have been first duly appreciated by
llois, was to distinguish accurately between the effects
h ensued from dividing the par vagum and the recurrent
s [2]; this he accomplished by opening the trachea below
lottis, so as to ensure a free passage for the air into the
chia [3]. The result of the investigation appears to be, that
essels of the lungs are loaded with blood, and the bronchial
clogged up with a serous or mucous effusion; that the air
course only partially admitted into the vesicles, and that
periences a less degree of change in its chemical compo-
than under ordinary circumstances. This last point,
h bears most directly upon the subject under consideration,
s to be established by the experiments of Provençal [4], and
njunction with the other observations, sanctions the con-
n that is drawn by Legallois. He remarks that the divi-
of the par vagum affects the respiration in three ways, it
ishes the quantity of air admitted by the glottis, it retards
irculation of the blood through the pulmonary vessels, and
he vesicles with a fluid which prevents the admission of
ir into them. Hence we learn, that in these cases, the
l is destroyed by a process which is similar to suffocation

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[1] e Legallois' work, p. 177 et seq. Blainville, although he observed
certain degree of derangement took place in the lungs, appears to have
ered it as not an essential effect of the operation, and ascribed the
of the animal to injury sustained by the functions of the stomach; ubi
p. 181. Sœmmering, Corp. Hum. fab. t. iv. p. 237, remarks that the
ession of these nerves by a ligature produces difficulty of breathing, deaf-
omiting, and that it prevents the food from being digested.

[2] perhaps rather the laryngeal nerves, as we are informed by Dr. Hast-
hat, in his experiments, he found little or no dyspnœa by dividing the
nt nerves, but that it took place immediately upon dividing the laryn-
rves; Philip's Inq. p. 121, note.

[3] r le Principe de la Vie, p. 188 et seq.; p. 205 et seq.

[4] i supra, p. 182; Mém. de l'Institut de France, pour 1809, histoire,
par Cuvier.

or drowning except that it is less complete, and consequently less rapid in its progress[1].

Of the three circumstances mentioned above, we may conclude that the third is the essential cause of the effect which ensues. The first depends merely upon the anatomical relation which subsists between the nerves of the glottis and the main branch of the eighth pair, and may accordingly be obviated, by procuring a free admission for the air into the lungs, while the second is probably to be regarded, rather as an effect, than as a primary cause, arising from the contractility of the heart being partially impaired by the defective action of the air upon the blood. It now therefore remains to inquire, in what way the interruption of the nervous influence can produce the effusion into the vesicles; but upon this point it does not appear that we have any direct facts, which can enable us to form a decisive conclusion; and as it involves the question, respecting the influence which the nervous system has over the function of secretion, I shall reserve the farther consideration of it for the chapter which treats upon that subject. So far, therefore, as the experiments on the division of the par vagum respects the general question of the influence of the nerves over the respiratory organs, it leads us to the conclusion, that this influence is exercised in an indirect way only, depending upon the intervention of certain circumstances which affect the respiration in consequence of their not allowing the air and the blood to be brought within the sphere of their mutual action[2].

It still, however, remains for us to examine whether in any part of the process of respiration we can perceive a more direct connexion between the nervous system and the lungs, so as to indicate a necessary dependence of the one upon the other, and for this purpose, we must trace out the successive steps of the operation, and inquire into the relation of each of them to the other parts of the system.

The two great actions which constitute the process of respiration are the contraction of the heart, by which the blood is propelled through the pulmonary vessels, and the contraction of the diaphragm, by which the air is admitted into the vesicles. With respect to the first of these, I have already endea-

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[1] Sur le Principe de la Vie, p. 208 et seq.

[2] This is nearly the same view of the subject which is taken by Magendie; Physiol. t. ii. p. 298 et seq. The numerous experiments that have been lately performed in this country and in France on the division of the par vagum, more immediately refer to the effect of the operation upon the functions of the stomach, and will therefore fall under our consideration in a subsequent chapter; I may, however, remark that they agree with those that have been referred to above respecting the state of the lungs. The experiments of Sir B. Brodie afford a good illustration of the point in question, by exhibiting the difference of effect which results from the division of the par vagum, according as it is made in the neck of the animal, or near the termination of the nerves in the stomach; Phil. Trans. for 1814, p. 103..5.

d to show, that it is produced by the distending force of
lood operating upon a contractile organ, which is entirely
ntary, and, in its ordinary action, is altogether uncon-
d with the nervous power. The diaphragm, on the con-
, being a voluntary muscle, its contractions proceed upon
erent principle. But as the power of volition is conceived
applied to this part on certain occasions only, where it is
sary to produce some extraordinary effect, it still remains
s to inquire in what way its ordinary contractions are pro-
l; whether by a stimulus acting directly upon a contractile
as in the case of the heart, or whether the stimulating
is always transmitted to it through the medium of the
s.

ere are many circumstances, which characterize the con-
on of the diaphragm, that, at first view, might induce us
nclude that it is produced in the first of these ways, and
it ought to be classed among what are termed the vital
ns. It commences immediately at birth, and continues
after to exercise its functions, without interruption, in a
ant and regular manner, unlike the generality of the ac-
depending on the nervous influence, which are excited on
in occasions only, when the specific stimulus is applied. But
ithstanding this analogy, we shall find, upon a more accu-
examination of the phenomena, that we cannot refer the
action of the diaphragm to the first class of functions. In
irst place, we do not perceive that there is any stimulus
diately applied to the part, so as to produce the effect.
as far as we can form any judgment of the cause which
es the contraction, it would appear to be one which must
hrough the nerves, for although we are not very well able
ace the connexion between the cause and the effect in this
yet it would appear that the uneasy sensation which we
rience, when the lungs do not receive their due supply of
air, is the immediate cause of the contraction of the
hragm [1].

is view of the subject is confirmed by referring to the ana-
cal structure of the parts, and especially to the origin and
e of the nerves which are transmitted to the diaphragm.
their situation they are less exposed to injury than many
r parts of the nervous system, but it appears, that when-
these nerves are affected by any cause, either morbid or
lental, the action of the diaphragm is proportionally im-

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may here recur to the inquiry, which was instituted in a preceding part
chapter, into the cause of the alternation of inspiration and expiration;
om the above remarks, it would appear to be ultimately referable to the
y sensation experienced in the nerves of the diaphragm, by which this
is stimulated to contract. Adelon conceives that this uneasy sensation
logous to that experienced by the stomach from the want of food;
ol. t. iii. p. 152; the resemblance in the two cases is, however, rather
al than real.

paired. We have seen with respect to the heart, that if the structure of the organ itself be sound, and its vessels properly supplied with arterial blood, so as to maintain the contractility of its fibres, its action may be kept up for an indefinite length of time as long as the blood continues to be poured into its cavities, although its communication with the brain be entirely destroyed. But this is not the case with the diaphragm, for if its nerves be injured or divided, it appears that its contraction is completely destroyed, and its functions suspended, unless it be excited by an artificial stimulus[1]. Hence we learn, that we are to consider the muscular power, which is exercised in respiration, as being intermediate in its nature between the action of the muscles which are entirely voluntary, such as those of locomotion, and of those which, like the heart, are completely involuntary. The final conclusion which we must draw from these observations will be, that there is a necessary connexion between the nervous system and the function of respiration, and that, in this respect, it differs from the circulation, which is essentially independent of the nerves, although occasionally subject to their influence[2].

The celebrated experiment of Vesalius[3], but of which Hooke appears to have been the first to show the importance, and to deduce from it the just inference, where, by artificially introducing air into the lungs of an animal that had been apparently destroyed by interrupting the respiration, the blood is enabled to undergo its appropriate changes from the venous to the arterial state, while, in proportion as this change is effected, the contractility of the heart is restored, seems to be decisive, in pointing out the connexion between the heart and the lungs, but it does not give us any insight into the nature of the connexion between the lungs and the nervous system. In the case of the heart the connexion is of an indirect kind, and takes place by means of a series of intervening operations, which may be clearly referred to distinct sources, although they become sooner or later necessarily connected together and inseparably united. But when, on the contrary, life is extinguished by a blow on the head, by an injury of the spine, or by any of those causes which do not act upon the blood in the first instance, the immediate effect, as far as the respiration is concerned, appears to be the destruction of the contractile power of the diaphragm, because the nerves which give this organ its power of mechanically contributing to the reception of the air into the lungs, are no longer able to transmit to it their specific influence.

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[1] This was remarkably illustrated by the experiments of Legallois, in which upon successively removing the different portions of the brain, he always found the action of the diaphragm to be destroyed, when he arrived at the origin of the nerves which supply this part.

[2] The observations of Dr. Philip on this point are very judicious, although perhaps there may be some objection to the phraseology which he employs. He clearly points out the cause of the difficulty which Legallois experienced in explaining the phenomena, and reconciling them to his system. According to Dr. Philip's view of the subject, the lungs are supplied with perception, or as he terms it, sensorial power, by means of the par vagum, so that it is through their intervention that the uneasy feeling is conveyed to the sensorium, which ensues from the defect of fresh air in the vesicles; Inquiry, p. 207, 268; also Quart. Journ. v. xiv. p. 98 et seq. The anatomical structure of the parts, and the experiments of Legallois, to which he refers, render this opinion probable; yet there is still considerable difficulty in conceiving how the state of the air in the pulmonary cavities can act upon the nerves, and we have yet to inquire into the nature of the connexion between the 8th pair and the phrenic nerves. The student may peruse with advantage Bichat's remarks, " Sur la Vie," &c. p. 2. Art. 10. § 2. p. 228 et seq., the object of which is to show, that it is by an indirect effect that the lungs cease to act in consequence of a cessation of the functions of the brain; also Art. 11. § 2, where he endeavours to prove that the lungs are the intermediate organs, which cause the death of the heart to succeed to that of the brain. With respect to the effect of dividing the par vagum, it is remarkable that he draws a conclusion from his experiments which is directly the reverse of what appears to be the natural deduction from them, ubi supra, p. 224. There is frequently an obscurity in this author, from his peculiar phraseology, and from his hypothesis of the two lives; but the valuable information which he affords us is well worth the trouble of developing his meaning from the metaphysical language with which it is embarrassed. Sir B. Brodie adopts a conclusion essentially the same with the one stated above, that when the function of the brain is destroyed respiration ceases, although circulation is still maintained; Phil. Trans. for 1811, p. 36.

[3] See note in p. 332.

The conclusion, that one of the remote effects of respiration is to produce that change in the blood, which may enable it to preserve the muscles in their contractile state, affords an easy explanation of a circumstance connected with the action of the heart, which has given rise to much discussion; in the ordinary course of the circulation the right ventricle is always filled with venous blood; this is then transmitted through the lungs, and is brought to the left ventricle in the arterialized state. As the blood is supposed to be equally the cause of contraction in both the ventricles, it has been asked, how the two sides of the heart can be acted upon in the same manner by blood of such different qualities, or more particularly, how the venous blood can enable the right ventricle to contract, when, in other cases, arterial blood appears to be necessary for this purpose. The speculative physiologists have given us many answers to this inquiry. Some of them have been satisfied with referring it to the action of the vital principle, others have assumed certain specific properties, in the right side of the heart, by which it was enabled to be acted upon by venous blood, an hypothesis which was adopted even by Goodwyn[1]; others have supposed that there was a kind of sympathy between the ventricles, and others again have imagined there to be something in the mechanical structure of the heart, by which the contraction of one of its parts is necessarily succeeded by that of the remaining part[1].

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[1] Connexion of Life, &c. p. 82, 4. See also a posthumous essay by Dr. Goodwyn, in Ed. Med. Journ. v. xxxiv., where he controverts the doctrine of Bichat. This would appear to be the opinion of Sœmmering, who says, "Sanguis nempe venosus, aeri non admissus, licet ventriculum cordis pulmonalem stimulare queat, tamen ventriculo cordis aortici incitando impar est;" Corp. Hum. fab. t. vi. p. 76. And upon the same principle Dr. Philip observes that the two sides of the heart are "fitted to obey dif-

To a certain extent the latter opinion must be considered as not without some foundation. Although there is a good deal of intricacy in the mechanism of the structure of the heart, and in the form and disposition of its muscular fibres, yet, as far as the present question is concerned, it may probably be regarded as composing one muscle, and will therefore possess that simultaneous action in its various parts, which belongs to the muscles generally, in consequence of which, when any one part of them is stimulated, the whole is thrown into contraction. But although this principle may operate to a certain extent, it cannot be regarded as the correct or proper solution of the proposed inquiry. The blood which is poured into the ventricles of the heart, and causes their contraction, acts, not by any specific properties which it possesses, but merely by its mechanical bulk, while the blood which imparts contractility to the heart is contained in the coronary arteries, which, like the other muscular arteries, proceed from the great trunks in their vicinity, and are distributed through the whole substance of the muscles. Provided the blood in these vessels be properly arterialized, it is immaterial what kind of blood enters the interior cavities of the heart; we may venture to assert, that if it were possible to carry on the circulation through the other parts of the body, it would be sufficient for the action of the heart, if its ventricles were filled with any fluid of the proper temperature and consistence, and in the requisite proportion[2].

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ferent stimuli;" Phil. Trans. for 1815, p. 81. See also Nicholls's Pathology, p. 71, where the author supposes that the contraction of the ventricle may be affected by the state of the blood which is poured into it.

[1] Haller formed a singular opinion on this subject, that the venous blood was the proper stimulus to the heart. He found that when the blood was made to pass through the right side of the heart, after it had ceased to pass through the left side, the contraction of the right side remained longer than that of the left; Sur les Part. sens. et irrit. mém. 2. ex. 515..523. t. i. p. 362..7.

[2] This point is well treated by Bichat; Sur la Vie, &c. Art. 6. § 2; but, I believe, this manner of viewing the subject is not original in him, as has been intimated by some of the French writers; See Cuvier, Leç. d'Anat. Comp. t. iv. p. 300. I recollect this doctrine being explicitly taught by Mr. Allen, in his admirable lectures on the animal œconomy, which I had the good fortune to attend in the years 1796 and 1797. Mr. Coleman controverts Goodwyn's opinion respecting the state of the two sides of the heart very satisfactorily; he supports his argument by the peculiarities of the fœtal circulation; Dissert. p. 40 et seq. This may also be regarded as the legitimate deduction from Sir B. Brodie's experiments, in which the action of the heart continued after the destruction of the nervous system, until, in consequence of the respiration being suspended, the circulation also ceased;

These remarks upon the effect of respiration in promoting the contractility of the muscular fibre, will enable us to understand the immediate cause of death from drowning, suffocation, or any other of those accidents which prove fatal by preventing the access of fresh air to the lungs. When the action of the pulmonary organs was conceived to be altogether of a mechanical nature, the cessation of the motion of the heart in drowning or suffocation was ascribed to a mechanical obstacle impeding the passage of the blood through the lungs, and the heart was supposed to be at rest, because the blood was not duly transmitted to its cavities. We are, however, now assured that no obstacle of the nature formerly contemplated exists; that the first, and indeed the essential effect of submersion is to cut off the supply of oxygen from the blood, so that it can no longer undergo its appropriate change; it is therefore carried into the coronary arteries in the venous state, and hence the heart loses its contractility. The various organs of the body, all of which require for their support a regular supply of arterial blood, are consequently unable to perform their functions, and among others, the brain and nerves lose their sensibility, so that all the powers, both physical and vital, are suspended, and in a short time irrecoverably destroyed. We may consider ourselves as indebted to Goodwyn for the first consistent hypothesis upon this subject; for although his ideas on some of the subordinate points are not altogether correct, he is fundamentally right in his view of the connexion which subsists between life and the

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Phil. Trans. for 1811, p. 36. Here the immediate effect depended upon the want of the appropriate change in the blood by the air rendering the muscular fibres of the heart no longer contractile. This subject has been amply discussed by Dr. Alison, in his Physiol. p. 193 et seq., and still later in an elaborate article, "Asphyxia," in the Cyclop. of Anat. He controverts the doctrine of Bichat, principally from the alleged fact, that the introduction of air into the lungs can restore the respiration, after the circulation has entirely ceased; and also from the result of the experiments of Drs. Williams and Kay, which have shown, that the blood ceases to be returned to the heart while it still retains its contractility; the experiments of Dr. Kay more particularly prove this with respect to the left side of the heart; see his treatise on Asphyxia; and Ed. Med. Journ. v. xxix. p. 37 et seq. It further appears, that the obstruction is principally situated in the capillaries of the lungs, and hence, upon a review of the whole question, Dr. Alison concludes, that the effect essentially depends, not upon the heart, nor, as some physiologists have supposed, upon the brain, but upon a certain vital action of the capillaries of the lungs, which tends to attract the blood towards them, and which is stimulated into action by the oxygen of the atmosphere. In the last number of the Ed. Med. Journ., v. xlv. p. 103 et seq., Dr. Alison has published an account of a series of experiments which he performed in order to illustrate his doctrine. Dr. Kay's treatise referred to above deserves an attentive perusal. Dr. Williams's Essay "on the cause and effect of an obstruction of the blood in the lungs," in the Ed. Med. Journ. v. xix. p. 524 et seq., contains some valuable and original observations, which have not met with the attention due to their merit. Dr. Roget's art. "Asphyxia," in the Cyclop. of Med., contains many judicious remarks. See also Adelon, Physiol. t. iv. p. 206 et seq.

function of respiration[1]. The brain and nerves it appears are not directly concerned in the process, and only suffer together with the rest of the system; a consideration which, although it would seem to be a very obvious consequence of the admitted facts, has not been always borne in mind, and has tended to throw an air of mystery over a subject, which is in itself sufficiently intelligible.

During a certain period of time the powers of life are merely suspended, but the parts not being irrecoverably injured[2], if the proper means be resorted to, the actions of the system may be restored. These means essentially consist in enabling the blood to undergo its specific change in the lungs, by introducing into them a quantity of air, containing the requisite proportion of oxygen. But the various functions of the body are so connected together, that the suspension of any one of them is necessarily attended with a derangement of the whole machine; and we accordingly find, that after a very short interval, the mere introduction of fresh air into the lungs is not sufficient to restore its action, because the diaphragm, upon the contraction of which this action essentially depends, has now lost its contractility, in consequence of the arteries being filled with venous blood. We have therefore to direct our attention immediately to the state of the circulation, as well as of the respiration, and we attempt to induce the contraction of the heart by stimulants applied to it, or to any other part of the system, which may indirectly affect the action either of the muscles or of the nerves [1].

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[1] See his Essay, sect. 5. He clearly established the fact, respecting which the opinions of physiologists had been previously much divided, See Morgagni, Epist. No. 19. § 30, that the introduction of water into the cells of the lungs is not a necessary effect of submersion; Essay, p. 10, 14; a fact which is equally important in a theoretical and practical point of view. Goodwyn's experiments upon this point were confirmed by those of Mr. Kite and Mr. Coleman; see Essay on Submersion, p. 3, 4; and on Suspended Respiration, p. 82, 3; also Paris's Med. Jur. v. ii. p. 35 et seq. On speculating upon the state of the system which is produced by drowning, many physiologists have supposed that death is immediately caused by a derangement of the nervous functions, analogous to, or rather identical with apoplexy. The lungs, it is said, are in a state of forced expiration, and are completely loaded with blood, sent into them by the right side of the heart, which is retained there in consequence of the left ventricle not being able to propel its contents. This mechanical congestion of the blood will be communicated to the whole of the sanguiferous system, but it is supposed that its effects will be more peculiarly experienced in the brain, on account of the nature and situation of the vessels of that part. This doctrine is maintained by Mr. Kite, and although I conceive it to be incorrect, it is stated and enforced with considerable ability; Essay, p. 46, 66, 75. Mr. Coleman's observations on the above opinion may be read with advantage, and may be regarded as containing a complete refutation of it; Essay, p. 135..144; but I may remark, that he appears to lay too much stress upon the supposed collapse of the lungs; p. 148..1. We shall find many ingenious observations on the state of the functions, and on their relation to each other, when death is caused by the exclusion of air from the lungs, in Dr. Edwards's chapter on Asphyxia, p. 263 et seq. Many of the systematic nosologists of the last century considered the affection produced by submersion as a species of apoplexy; this was the case with Cullen, Nosol. t. ii. p. 190.

Since writing the above note, I have met with an account of a series of experiments, performed by Professor Meyer, on the presence of water in the lungs of drowned persons. We are informed that he always found more or less fluid in the lungs after drowning, and that it possessed the colour and other sensible properties of the medium in which the animal had been immersed. He employed water to which the hydrocyanate of potash had been added, and tested the fluid that was found in the lungs with the muriate of iron. It appears that he performed 15 experiments, in which animals were drowned under different circumstances, and always with the same results; Med. Repos. v. iii. new ser. p. 436. I may remark that Haller, as the result of his inquiry, thought that a small portion of the fluid in which an animal is drowned is occasionally found in the lungs, but that fluids do not readily pass into them; El. Phys. xviii. 3. 22, 25.

[2] See Hunter, in Phil. Trans. for 1776, p. 412 et seq. and the same paper in an improved form, in his "Observations on the Animal Œconomy," p. 129..141. The older writers were disposed to admit of a much longer continuance of the state of suspension than the moderns; See Boerhaave, Prælect. § 203; Parr's Dict. Art. "Submersion;" also the long and desultory article "Noyés," by Vaidy, in Dict. Scien. Méd. t. xxxvi. p. 393 et seq.

There is likewise considerable difference of opinion respecting the period during which the respiration can be suspended, without the interruption of any of the functions. Here, as in the former case, we have every reason to suppose, that the narratives which we have of the length of time that divers can remain under water, are much exaggerated. Boyle, referring to the stories of divers remaining under water for three or four hours, informs us, that a person, who was in the habit of diving, for the purpose of recovering goods from sunk vessels, confessed that he could not remain longer than two minutes under water; Works, v. i. p. 111. Perceval, in his account of Ceylon, p. 64, 5, states that the pearl divers generally remain under water for about two minutes, but that in some cases, they remain as long as four or even five, and in one instance as long as six minutes. See also the remarks of Dr. Alison, art. "Asphyxia", in Cyc. of Anat. and Phys. v. i. p. 259. Halley observes, that few persons can live longer under water than half a minute, but adds that a professed diver may, by long practice, acquire the power of remaining near two minutes; Phil. Trans. v. xxix. p. 493. Cavallo is disposed to limit the time to a minute and a half; on Airs, p. 26, 7. Dr. Edwards, who has particularly directed his attention to the subject, informs us, that some of the best divers in the swimming school at Paris can remain under water three minutes; De l'Influence &c. p. 269. Dr. Paris, Med. Jur. v. ii. p. 34, resting his opinion on Sir B. Brodie's authority, conceives it very doubtful, if the heart ever continues to pulsate for so long as five minutes after the respiration has ceased; generally the interval is much shorter; in drowning he seems to limit it to a few moments; p. 37. The stories that are related respecting divers, he regards as entirely fabulous; note in p. 34. It is supposed, however, by Roesler, from a series of valuable and elaborate experiments, made expressly to elucidate this point, that the period assigned by Sir B. Brodie is too limited; see Ed. Med. Jour. v. xxiii. p. 207 et seq. For many extraordinary narratives, see Elliotson's Physiol. p. 225, note.

[1] The remarks and directions of Hunter, in the papers referred to above, are for the most part very correct and judicious; but he has fallen into one error of importance, in recommending the application of stimulating vapours to the interior of the lungs; Observ. on Anim. Œcon. p. 136. Fortunately for the patient, the natural actions of the organs are commonly sufficient to exclude the vapours, for if they were admitted in any considerable quantity suffocation would be the consequence. The only use of stimulating vapours, is to excite the nerves of the nose, which, by their connexion with the respiratory nerves generally, may eventually stimulate the diaphragm to contrac-

Of these stimulants the most effectual is caloric, either as applied to the surface of the body generally, by placing it in a warm medium, or by a topical application of it to the region of the stomach, or to any other part more particularly sensible to its influence. With the same intention we apply frictions, and occasionally more powerful stimulants, such as the electric or galvanic shock, transmitted through the heart or the diaphragm, which, if judiciously used, would seem to be indicated, by the power which they are known to possess of exciting the contraction of a muscle, when it can no longer be acted upon by any other means. It is almost unnecessary to add, that in the above remarks, I have proposed only to state the general principles upon which we ought to proceed in attempting to restore the powers of life, when they have been suspended in consequence of the want of the due supply of oxygen to the lungs; a variety of minute, although very important considerations, will naturally require our attention, depending upon the peculiar circumstances of each individual case, which must be left to the judgment of the practitioner[1].

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tion. The directions published by the Humane Society, in their latest advertisements, appear generally judicious; some of the means formerly employed were at least of very dubious utility. See a good abstract of the same in Rees's Cyclopædia. Art. "Drowning." Sir J. Gibney's proposal to employ steam as a speedy and effectual method of restoring the heat of the body, may, I conceive, be frequently employed with advantage; On Vapour Baths, p. 135, 6. It has been justly remarked, that there are certain cases of suspended animation, as in suffocation from carbonic acid, and in fainting, where the contraction of the heart and diaphragm are restored by applying cold to the surface of the body, and when warmth would be unfavourable, thus producing an exception to the general rule of treatment. But the cases when cold is to be applied are those in which the heat of the body has not been abstracted by a cold medium; if the temperature be much reduced, it will be necessary to proceed in the usual manner, with the application of warmth. I may remark, that in the recovery from apparent drowning, the contractility of the muscles, as exhibited by the action of the heart and diaphragm, is re-established for a considerable time before the restoration of the sensibility, thus illustrating the general position of the independence of the former of these powers upon the latter. An observation, the converse of the above, tends to illustrate the same position, that when suffocation takes place, from any cause, volition ceases for a considerable time before life is irrecoverably extinguished. See Edwards, de l'Influence &c. p. 269. See also the elaborate art. "Submersion", by Orfila, in Dict. de Méd. t. xx. p. 2. et seq., and the art. by Dr. Roget referred to, for the treatment of asphyxia from submersion.

[1] So little conception had Boerhaave of the real objects of respiration, and consequently of the cause of death by submersion, that he conceived an animal might be rendered amphibious, by frequently plunging it while young into water, and thus prevent the closing of the foramen ovale and the ductus arteriosus. Why we cannot live without respiration he says, "nobis ignotum est, quod forsan nobis, vestroque ævo, aliquid patescet." Prælect. not. ad § 691. t. v. par. 2. p. 186. Boerhaave's proposal might be pardoned at the period when it was advanced, but we can scarcely extend our apology to Beddoes, who, notwithstanding all the discoveries that had been made since the time of Boerhaave, seriously advances the opinion, that "by frequent immersion in water, the association between the movements of the heart and

The opinion that one of the remote effects of respiration is to prevent the decomposition of the blood, and eventually that of the body at large, may be considered as having originated from the hypothesis of Crawford[1], respecting the source of the inflammable matter employed in the production of animal heat. It is generally admitted, that all the matter which enters into the composition of a living organized body, is subject to perpetual change, that after having performed its appropriate functions, it becomes, in some way or other, altered in its nature, so as to be no longer suitable for the purpose, and that it is then discharged from the system. Now it has been supposed that it is by the exchange of old for new particles that the body is preserved from decomposition, and that when this process is suspended by death, so that the effete matter can no longer be carried off, a complete decomposition ensues. The blood is the medium by which this mutual interchange is effected, the veins are the channels by which the matter is carried off, and the lungs are the organ by which it is finally discharged. Of all the constituents of the body, the blood, and more especially its red globules, appears to be the part which is the most subject to decomposition, and it is accordingly on this, that the air is contrived more immediately to act in the process of respiration; it may be farther observed, that the first step in the spontaneous decomposition of animal matter consists in the loss of a portion of its carbon, which unites with the oxygen of the atmosphere, and forms carbonic acid, as is the case with the air in the lungs[2].

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ngs might perhaps be dissolved; and an animal be inured to live commodiously, for any time, under water;" On Fact. Airs, part 1. p. 41. This opinion had been previously maintained by Buffon; and, although it is altogether without foundation, yet, in endeavouring to prove it by experiment, he discovered a curious fact, which has been since confirmed by Legallois, and Dr. Edwards, that a newly-born animal can live without air for a much longer space of time, than an adult of the same species; see Edwards, De l'Influence &c. part 3. chap. 4. p. 165..174. Buffon made his experiments upon dogs. Legallois and Edwards used rabbits, cats, and other animals of various kinds, and found that they could bear submersion in water for nearly half an hour without injury; it was perceived, however, that this faculty was soon diminished, and that, after some days, it was entirely lost. Dr. Edwards also found that it was only certain species of animals which possessed it, and he made the very important observation, that it belongs to those only which have but little power of generating heat when newly born, a circumstance which seemed exactly to correspond with this capacity of remaining for a certain space of time without air. Sir A. Carlisle observes, that animals which hibernate are less easily drowned than others, and gives us the results of some experiments on hedge-hogs in proof of his position; Phil. Trans. for 1805, p. 19.

[1] The same opinion had indeed been previously suggested by Priestley, see note 6, p. 339; and even the older physiologists, as may be found in the references to Haller, El. Phys. viii. 5. 20, entertained some imperfect ideas of the same nature, but it was not brought into a consistent form until the publication of Crawford's work.

[2] Spallanzani found that the bodies of the different classes of animals, worms, insects, fishes, oviparous quadrupeds, birds, and the mammalia, all deoxidate

Of these stimulants the most effectual is caloric, either as applied to the surface of the body generally, by placing it in a warm medium, or by a topical application of it to the region of the stomach, or to any other part more particularly sensible to its influence. With the same intention we apply frictions, and occasionally more powerful stimulants, such as the electric or galvanic shock, transmitted through the heart or the diaphragm, which, if judiciously used, would seem to be indicated, by the power which they are known to possess of exciting the contraction of a muscle, when it can no longer be acted upon by any other means. It is almost unnecessary to add, that in the above remarks, I have proposed only to state the general principles upon which we ought to proceed in attempting to restore the powers of life, when they have been suspended in consequence of the want of the due supply of oxygen to the lungs; a variety of minute, although very important considerations, will naturally require our attention, depending upon the peculiar circumstances of each individual case, which must be left to the judgment of the practitioner[1].

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i, p. 339; and even the older physiologists, as may be found in the
nces to Haller, El. Phys. viii. 5. 20, entertained some imperfect ideas
same nature, but it was not brought into a consistent form until the pub-
n of Crawford's work.

[2] allanzani found that the bodies of the different classes of animals, worms,
s, fishes, oviparous quadrupeds, birds, and the mammalia, all deoxidate

Hence it would appear to be not an unfair conclusion, that the cause which more immediately operates in preventing the decomposition of the body, as far at least as the chemical nature of the substances is concerned, consists in the abstraction of a part of the carbon of the blood, and that if these particles were not removed from it in proportion as they are deposited, they would produce a tendency to decomposition, which would terminate in complete disorganization.

These remarks may assist us in forming some judgment respecting the value of an hypothesis, which has been very generally adopted, that the power which the living body possesses of resisting the tendency to decomposition, is to be ascribed to the operation of what has been termed the vital principle. If we examine the body immediately after death, its structure and composition, as far as we can perceive, is precisely similar to what they were previous to dissolution, yet it soon begins to exhibit a series of chemical changes, which will eventually proceed to its complete destruction, while, if life had continued, it would have retained its form and composition for an indefinite length of time. This difference has been said to be owing to the presence or absence of the vital principle, an agent which is supposed to keep every part of the system in its perfect state, and to regulate all its functions, while conversely, the continuance of this perfection and regularity has been assumed as an evidence of the existence of this principle.

There are two senses in which the term principle has been correctly applied in natural philosophy; first, when we wish to designate a material agent, which produces some specific effect, as, according to the doctrine of Lavoisier, oxygen is said to be the acidifying principle, and one of the constituents of oak bark is styled the tanning principle: or secondly, we may correctly employ the term principle to signify the cause of a number of phenomena, which essentially resemble each other, and which may be all referred to one or more general laws, as the principle of gravitation or the principle of chemical attraction. We may then inquire how far the term principle can be properly applied to the cause of the phenomena of life.

I feel little hesitation in saying, that it cannot be used with propriety in the first sense, to designate any material agent, notwithstanding the high authority of those physiologists who maintain the existence of a "materia vitæ," and go so far as to describe its visible and tangible properties; or of those who

the air after death, some of them as much as during life; and this appears to have been the case before any visible marks of decomposition could be observed; Mém. sur le Respiration, p. 63, 70, 74, 302, 316. He found the same change to be produced in the air by torpid animals, although the respiration seemed to be entirely suspended; p. 77, 108, 185. The observation, that the decomposition of animal matter produces carbonic acid, appears to have been first made by Priestley; On Air, v. iii. (1st ser.) p. 340, 1.

identify the cause of the characteristic properties of life with electricity or any analogous agent[1]. Nor shall we find the term principle more appropriate when employed in the second sense, to express the supposed cause of a series of phenomena, which may be all referred to one or more general laws; for, according to the explanation which has been given of it by those who have expressed themselves in the most intelligible manner, the vital principle has been employed to express all those actions which could not be referred to any other general principle[2]. Besides the laws of mechanics and of chemistry, we observe in the living body various phenomena which essentially differ from these, and which we must therefore ascribe to

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[1] I shall defer the objections which, I think, may be deduced against these, as well as against the other modifications of the material hypothesis, to a subsequent part of my work. At present, I shall only remark, that in supporting the doctrine of immaterialism, I disclaim all intention of throwing out any imputation or censure against either the principles or talents of those whom I oppose. Such a proceeding I should regard as highly illiberal, and therefore unworthy of one who professes to feel an interest in the advancement of knowledge.

[2] In order to show that I have not misrepresented the doctrines that are maintained by many of the modern physiologists, upon the subject of the vital principle, in addition to the works that have been already alluded to, I shall subjoin the following references, with a brief abstract of the opinions of the several authors: Barthez, in the introduction to his "Nouveaux Elémens," t. i. p. 15. note 2, speaks of the vital principle, as what is proved to exist by its effects, but of the nature of which we are ignorant, and adds, that it is to be regarded as like the unknown quantities in algebra. See Thomson's remarks on Barthez, in his Life of Cullen, p. 446..451. Dumas, El. Phys. t. 1. p. 61. (1re ed.) in the same manner, likens the vital principle to the letters x, y, z, as employed in algebra to designate unknown quantities; see also the introduction to his second ed., where he farther explains his hypothesis, and vindicates his claims to originality against Barthez. Blumenbach, Instit. Physiol. § 30, observes that the vital powers are those which "are not referable to any qualities merely physical, chemical, or mechanical;" a remark which is strictly correct; but as we have already had occasion to observe, he speaks of the "vital energy" as an individual agent, and classes together under the title of "vita propria," actions which have no bond of union, except their being unlike every other. Dr. Park, Inquiry, p. 113, says, "what the vital principle is I shall not attempt to define; but it certainly does not consist in the functions which depend upon it. It is the cause and not the effect." Plenk, Hydrologia, p. 15, does not hesitate to consider the vital principle as one of the elements of which the body is composed. Virey, in conformity with the method that has been adverted to, supposes that those animals that have the power of being multiplied by division, or of repairing lost parts, do it by means of "a vital intelligent force;" Histoire des Mœurs &c. t. i. p. 485. The writer of the valuable article "Anatomy," in Dr. Brewster's Encyc. v. i. p. 473, 4, employs the vital principle to explain every action that cannot be otherwise accounted for. Dr. Fleming, after enumerating the actions that are peculiar to organized bodies, refers them all to the operation of the vital principle, which he speaks of as an individual agent, yet he designates it only by the negative property of being different from all mechanical or chemical powers; Phil. of Zoology, v. i. chap. 2. The term vital principle is frequently employed by Dr. Philip; and although he uses it in a more guarded sense, it is liable to the objections which have been stated above. In his essay in the Quart. Journ., which may be quoted as a matured digest of his physiological doctrines, he says that the vital principle "bestows on bodies certain properties;" v. xiii. p. 97. If by this expression is meant that animate possesses essen-

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tially different properties from inanimate matter, no one can deny the position; but if it is intended to convey the idea, that the vital principle is something which can be added to or removed from bodies, without affecting their other properties, or to designate a series of phenomena which essentially resemble each other, we are going beyond the limits of correct induction, and are employing a form of speech which has given rise to much misconception and obscurity. Dr. Copland, in like manner, says, that the vital principle controls the changes and forms of matter, and that the laws and affinities of matter are entirely subject to it; Trans. of Richerand, p. 532. Dr. Philip supposes that arterial blood contains the vital principle; Quart. Journ. v. xii. p. 20, and v. xiii. p. 112. The only correct meaning of this phrase I apprehend to be, that the blood exhibits those properties which are characteristic of life, viz. that it is contractile and sensitive; for that the blood is connected with the vital actions of the system is a position to which no one can object. Dumas, who is a zealous defender of the life of the blood, El. Phys. t. i. p. 454 et seq., extends this property to the chyle also; he speaks of it as "vivant par elle-même," "vivant de sa propre vie;" t. ii. p. 45, 6; and I conceive that we cannot resist the conclusion if we admit the premises. It may be said that the contest merely regards a difference of expression; but in answer to this I reply, that when physiologists state, that certain effects are produced by the vital principle, if the words have any meaning, they must be intended to explain the mode in which the effect is performed, whereas they only tell us that the effect in question is the result of vitality, a proposition of the truth of which no one can doubt, but which affords us no insight into the nature of the operation. Waving, however, any objection that there may be against the term vital principle, and employing it as synonymous with life or vitality, it will be found that it has been often used in a vague and inappropriate manner. It neither bestows upon a living body its specific properties, nor is it the consequence of these properties, or the result of organization; but it is by the existence of these properties that life is indicated, and in which life consists. There are certain circumstances in which the living differs from the dead animal; whether these circumstances may be all resolved into the two general principles of contractility and sensibility, is a point for farther inquiry. We have a valuable paper by Ferriar "on the Vital Principle," in which we have an account of the origin of the doctrine and of its successive development; Manchester Mem. v. i. p. 216 et seq.; he observes, "it is evident that we gain nothing by admitting the supposition, as no distinct account is given of the nature or production of this principle," &c. p. 240. Sir Ev. Home makes an observation which cannot be too strongly impressed upon the mind of the physiologist; "it seems," he says, "to be a rule of the animal œconomy that the laws of life should not be employed when the mechanical or chemical laws of matter will answer the purpose;" Lect. on Comp. Anat. v. i. p. 477. We have many valuable remarks on this subject in Barclay's work on life and organization. Dr. Milligan's observations on the term vital principle, and others of a similar nature, are, I conceive, correct and appropriate; Trans. of Magendie, p. 533, 4. See Dr. Elliotson's remarks on the doctrines of life; Physiol. p. 21 et seq.; many of them are just, but I cannot assent to all his conclusions; also Blandin's notes on Bichat, "Consid. Gén." passim. I may also refer to the 2d chap. of the 4th book of Rudolphi's Physiol., p. 213 et seq. of the transl. by How. This work contains many judicious and philosophical observations, but frequently mixed up, as I conceive, with a degree of mysticism and metaphysical subtilty. Rudolphi's work is rendered valuable to the student from the number of references which it contains. Many of Dr. Willis's remarks on this subject appear to me correct and appropriate; see the Art. "Animal," in the Cyc. of Anat. The some other cause; but we find that these phenomena differ essentially among themselves, so that if we make this want of resemblance the bond of union, we proceed upon the fundamentally erroneous plan of generalizing specific differences, or associating phenomena, not because they resemble each other, but because they cannot be reduced under any other class. We may then conclude, that when it is asserted that the blood resists decomposition in consequence of the operation of the vital principle, if the phrase have any definite meaning, it is saying no more than that the blood is not decomposed because it is contained in the vessels of the living body, an assertion which no one will be disposed to deny, but which unfortunately does not throw any light upon the subject of our investigation.

I conceive that the present state of our knowledge does not admit of our giving a satisfactory answer to this question, but as far as we are able to understand it, I think it is very evident, that it depends upon no single cause or principle, but upon the conjoined operation of many actions, which together constitute life, or by the operation of which the living differs from the dead animal[1]. The regular supply of fresh materials, as furnished by the digestive organs, the removal of various secretions and excretions, and lastly, the abstraction by the lungs of the superfluous carbon and water, effects which depend upon the united agency of both chemical, mechanical, and vital actions, are among the various causes which probably all contribute to the ultimate object[2].

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most complete view of the subject is contained in Dr. Prichard's Essay on the Doctrine of a Vital Principle, a work replete with learning, judgment, and candour.

[1] Béclard's remark perhaps comprises the most simple and intelligible view of the subject: "On appelle la vie l'ensemble des phénomènes propres aux corps organisés;" Elém. d'Anat. p. 4.

[2] The doctrines of Hunter on the subject of vitality have had so extensive an influence upon the opinions of the English physiologists, that it becomes a question of no small interest to ascertain them with accuracy. But even the most devoted admirers of Hunter admit, that this eminent physiologist was not fortunate in the explanation of his principles, and that, in justice to his memory, when speaking of his theories, we should not take his literal expressions, but the general scope and tenor of his doctrines. Nor shall we find our difficulties removed by the expositions that have been given of them by his commentators. I cannot but think that Mr. Abernethy has attributed sentiments to Hunter, which are not fairly to be ascribed to him. Identifying, as it appears, his own ideas with those of Hunter, Mr. Abernethy expressly states his opinion that "irritability is the effect of some subtile, mobile, invisible substance superadded to the evident structure of muscles, or other form of vegetable and animal matter, as magnetism is to iron, and as electricity is to various substances with which it may be connected;" Inq. into Hunter's Theory, p. 39. He afterwards develops at some length the reasons which induce him to regard electricity as the immediate cause of the phenomena of life; p. 38..44. Although not exactly a believer in the existence of the animal spirits, he thinks that the nerves contain "a subtile and mobile substance;" p. 69; and he finally concludes that "if the vital principle of Mr. Hunter be not electricity, it is something of a similar nature;" p. 88. Nothing, it will be observed, can be more explicit than Mr.

The next of the remote effects of respiration which were enumerated above, is the share which it has been supposed to have in completing the process of assimilation. Arterial is said to differ from venous blood in containing a larger proportion of crassamentum, and as we conceive that the crassamentum is immediately produced from the chyle, which enters the vessels just before the blood is exposed to the action of the air, it has been supposed by Cuvier[1], that the conversion of chyle into fibrine is one important office which is served by the lungs. Nor is it improbable that there may be a foundation for this opinion, yet it appears, upon the whole, more analogous to the usual operations of the animal œconomy, to ascribe the effect rather to secretion than to respiration; and, as to the greater proportion of it in arterial blood, even were the fact completely established, it ought perhaps to be referred more to the abstraction of a portion of the water and serum in the lungs, and to a deposition of crassamentum by the capillary arteries that are distributed over the muscles, than to the production of it in the pulmonary vessels. At the same time we may admit, that the removal of carbon from the blood, during its passage through the lungs, will tend to bring it into that condition which fits it for the purpose of repairing the necessary waste of the body, and maintaining the various functions in their perfect state[2].

The hypothesis that was adopted by the older physiologists, and which was embraced by Boerhaave[3], that the blood in its passage through the lungs, receives its peculiar organization, and especially, that the red globules are generated by the action of the air upon it, while it circulates through the pulmonary

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Abernethy's declaration, that life consists in an independent material agent, superadded to the visible corporeal frame, yet I conceive it would be difficult to prove that such was the conviction of Hunter, although he might occasionally indulge in some speculations of this nature. In Duncan's Med. Com. v. ii. p. 198..2, we have a brief, but correct summary of Hunter's doctrines, as far as respects the vitality of the blood.

[1] Cuvier argues, that as respiration separates carbon and hydrogen from the blood, it will leave in it a greater proportion of nitrogen, and as respiration maintains the contractility of the system, it is probable that it does it by leaving a greater proportion of that body in which alone contractility resides; Leçons, t. i. p. 91, 2. See also Young's Lect. v. i. p. 789; Thomson's Chem. v. v. p. 629, 0; and Prout, Ann. Phil. v. xiii. p. 278. Van Sweiten, in his commentary to the 97th Aphorism, observes that the air may have some effect in assimilation and sanguification; t. i. p. 136, 7. When the old physiologists speak of a pabulum vitæ existing in the air, they probably attached no very determinate meaning to the expression, but they do not appear to have intended to designate by it a nutritive substance.

[2] A new hypothesis of respiration has been lately advanced by the Prof. Gmelin, Tiedemann, and Mitscherlich. They conceive that oxygen unites in the lungs with carbon and hydrogen, producing carbonic acid, water, and also acetic acid; this latter decomposes the carbonate of soda, which exists in the blood, and disengages carbonic acid. The acetic acid is disengaged by means of the urine and perspiration, when the soda obtains carbonic acid from the constituents of the blood; Brit. and For. Med. Rev. v. i. p. 590.

[3] *Prælect.* notæ ad § 200. t. ii. p. 93; § 210. p. 115, 6.

vessels, is entirely without proof, and appears to have been formed principally because it was difficult to assign any other effect which the air could produce upon the blood.

There is a singular state of the system, to which certain animals are incident, which is closely connected with the respiration; the apparent suspension of the greatest part of their functions by cold, constituting what has been termed torpidity or hybernation. It appears not to be confined to any peculiar anatomical structure[1], nor to any one of the great classes of animals, but seems rather to exist in all cases where the situation or circumstances of the individual render it necessary for them to pass a portion of the year in the torpid state, thus affording us an insight rather into the final, than the efficient cause. It has been generally supposed to bear a close analogy to sleep[2], and, although I apprehend we shall find that the idea has been carried too far, yet, to a certain extent, it appears to exist; and, consequently, the states of sleep and torpidity may tend mutually to illustrate each other.

Although in the torpid state all the powers and functions are more or less affected, yet it would appear that the respiration is that in which the change is first experienced. In proportion as the animal becomes torpid, the action of the lungs is diminished, until it very nearly, if not altogether ceases[3]; the circulation, as well as the functions of digestion[1], secretion, and absorption are, in like manner, nearly suspended[2]; and the temperature is reduced almost to that of the surrounding medium[3]. Various opinions have been formed respecting the cause of hybernation, for although there seemed to be a necessary connexion between the application of a diminished temperature and the torpidity of the functions, yet we were not able to explain why certain animals only experienced this effect from cold, or how they were able to bear this suspension of all their functions, without the body being decomposed, or its powers being irrecoverably destroyed.

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[1] Edwards, De l'Influence &c. p. 471, 2; also p. 148, where he gives a list of the animals that become torpid in the climate of France. Sir A. Carlisle has indeed described a peculiar conformation in the great veins of the hybernating mammalia; but this does not appear to be found in the other classes of animals, nor is it very obvious what purpose it serves; Phil. Trans. for 1805, p. 17; see also Fleming's Phil. of Zoology, v. ii. p. 45 et seq. Cuvier informs us that some hybernating animals have certain fatty appendages, connected with the abdominal viscera, which probably serve to retain the heat of the internal parts; but he remarks that this structure is by no means general; Leç. d'Anat. comp. t. iv. p. 91, 2. Otto, of Berlin, informs us, that certain peculiarities in the sanguiferous system of hybernating animals, which had been advanced by Mangili and Saissy, do not exist; Inst. Journ. No. 3. p. 585, 6.

[2] Elliotson's notes to Blumenbach's Physiol. p. 182; Reeve on Torpidity, p. 136.

[3] Spallanzani, Mém. p. 77, 107; Fleming's Zool. v. ii. p. 53..6; Reeve on Torpidity, § 3. p. 21 et seq.; Edwards, De l'Influence &c. p. 149; art. "Hybernation," in Brewster. Dr. Ellis has observed this to be the case with snails; the authors who have described the state of torpidity do not quite agree respecting the fact, whether the respiration be entirely suspended when the torpor is complete. On the one hand, we might suppose it was the case, as Spallanzani informs us, Mém. p. 68. 109; Rapports, t. ii. p. 207, that torpid animals are not affected by being immersed in carbonic acid, or other noxious gases. Yet, on the other hand, he says, that they slightly deoxidate the air, an effect which he supposes may be produced by the skin; p. 77. I should, however, conceive that the action of the skin could not continue in the living animal, after that of the lungs was suspended. With respect to some of the lower tribes, he states, that caterpillars, when perfectly torpid, do not affect the air in any perceptible degree; Rapports, t. i. p. 30, 1. Fish, it would appear, do not become perfectly torpid, but, in great degrees of cold, have all their functions weakened; and produce a proportionably *less effect* upon the air; ibid. t. i. p. 457 et seq.

An explanation of the first part of the difficulty appears to be afforded us by the experiments of De Saissy, who found that hybernating animals possess the power of producing heat in a less degree than other animals with warm blood[4], so that

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The accounts that are collected by Dr. Fleming seem to indicate, that in most cases the lungs are not entirely passive, and this may be inferred from the remarks of Reeve. The same was likewise the result of the experiments of Mangili; Ann. de Mus. t. ix. p. 106 et seq. But it is not improbable that when the animals are disturbed for the purpose of experiment, a little degree of action may be induced in the pulmonary organs, which did not previously exist there. The results of Spallanzani's experiments on the respiration of various animals of the lower orders, in many of which the effect of temperature was particularly attended to, may be found in the "Rapports," t. i. p. 186, 7; 249, 0; 468..1. See also some observations on this subject by Dr. Hall; Phil. Trans. for 1832, p. 321 et seq.

[1] We are informed by Dr. Kirby, that hybernating animals, even if they be kept warm during their ordinary period of repose, are not inclined to take food; thus indicating, that the torpid state of the digestive organs is rather a contemporaneous occurrence, than an effect of that of the circulation and respiration; see the art. "Herpetology," in Brewster, v. xi. p. 16.

[2] Haller, El. Phys. xix. 2. 7. We have a curious account by Major General Davies, of the jumping mouse of Canada; during its state of hybernation, which lasts for between seven and eight months, it lies closely rolled up, and completely enveloped in a ball of clay, and in this state lies buried some inches below the surface of the ground, so that during this long period, all means of obtaining nutrition must be effectually precluded; Lin. Trans. v. iv. p. 156 et seq. In tab. 8. fig. 6. we have a representation of the perfectly globular form which the animal assumes. There was an opinion current among the Romans, that dormice became fatter during the state of hybernation; see Martial, lib. xiii. ep. 59; but it appears from Barrington, that this is contrary to the fact; we also learn, from the same authority, that the circulation is not entirely suspended during the hybernation of these animals; Miscellanies, p. 167, 8.

[3] Hunter on the Anim. Œcon. p 111. et seq.; Carlisle, Phil. Trans. for 1805, p. 17; Fleming's Zool. vol. ii. p. 50 et seq.; Reeve on Torpid. p. 12. Flourens informs us, that in the lethargy produced by cold, the respiration is totally suspended, and the circulation nearly so, and that the temperature falls nearly to the freezing point of water; Brewster's Journ. v. ii. p. 111 et seq. It would appear that respiration is the function which is the first affected and suffers in the greatest degree. We have some experiments of Dr. M. Edwards which bear upon this point; the conclusion which he deduces from them is, that whatever diminishes the energy of any of the vital functions immediately affects the respiration; as sleep, defective nourishment, fatigue, and cold; art. "Respiration," in Dict. Class. d'Hist. Nat.

[4] *Edwards*, De l'Influence &c. par. 3. ch. 2. p. 151 et seq.

when the atmospheric temperature falls below a certain standard, which is uniform for each species, their animal heat declines to a degree which is unable to support their contractility, and of course all the functions that depend upon it. What peculiarity it is in these animals which enables them to maintain their vital powers in the dormant state, seems still very difficult to explain; but this subject will be considered with more advantage when we have proceeded farther in our examination of the various functions.

There is another curious inquiry connected with the function of respiration, how is the change in the blood effected in the fœtus? The fœtal heart is supplied with blood, which exhibits the arterial properties [1], and gives the muscular fibres sufficient contractility to maintain the circulation, before the air can have access to the lungs; whence then does the blood, in this case, acquire its oxygen, or by what means does it discharge its superfluous quantity of carbon? This point has been long the subject of discussion among physiologists, and notwithstanding some important observations that have been made respecting it by the moderns, we shall find that it still requires farther investigation. The lungs of the fœtus are in an imperfect, or rather in a partially developed state, and must be regarded as one of those organs, the object of which is prospective. I have already had occasion to describe the peculiarity of the fœtal circulation, which essentially consists in a small portion only of the blood being transmitted through the lungs; we shall also find that the blood does not experience that change in them which is effected after birth, while, at the same time, we observe this same change to take place in a different organ. The fœtus is connected with the mother by a large cellular mass, spread over the internal surface of the uterus, and which consists essentially of two parts, termed, from their connexions, the fœtal and the maternal placenta [2], each of them being attached to the circulation of the fœtus and the mother respectively, but which, so far as we are entitled to judge from various experiments that have been performed expressly to decide the question, have no direct vascular connexion with each other[1]. We find, however, that the blood which is sent from the fœtus along the umbilical cord to the placenta in the venous state is returned in the arterial state, so that we are justified in concluding that this organ supplies the place of the lungs, or produces the chemical change in the blood which fits it for the support of life.

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[1] Although this is the opinion which is, I believe, generally entertained, yet it is necessary to observe that there are some physiologists of great eminence, who do not admit that any difference can be observed in the different parts of the fœtal blood, corresponding to the arterial and venous states. See Bichat, Sur la Vie &c. p. 190, 1, and Anat. gén. t. ii. p. 344; Cuvier, Leçons, t. iv. p. 298; Berzelius, Anim. Chem. p. 41; Young's Med. Lit. p. 505; Magendie, Physiologie, t. ii. p. 438; and Adelon, Physiol. t. iv. p. 405. I cannot but feel surprise at such an opinion, as in some cases where I have had an opportunity of examining the fœtus immediately after its extraction from the uterus, the different colours of the blood in the funis appeared quite obvious, thus agreeing with the observations of Dr. Jeffray; De Placenta, p. 41. We are informed by Dr. Milligan, that the experiments and observations of Dr. Jeffray have been lately confirmed by Dr. Campbell; Trans. of Magendie, p. 598, 9. See also the remarks of Dr. Holland in his Treatise on the Physiology of the Fœtus, and especially his 4th chapter, on its functions. I may also refer to Tiedemann, Tab. Arter. No. 38, where the different colours mark the different states of the blood in the umbilical cord.

[2] What I have stated in the text is the view of the subject which was supposed to be established by the investigations of the Hunters. The late re-

It is no doubt difficult to conceive how this can be accomplished without a direct vascular communication, and we can only account for it by supposing that the minute vessels of the placenta, like those of the lungs, are capable of absorbing and exhaling through their coats the substances necessary for effecting the change, the arteries of the maternal placenta exhaling oxygen and absorbing carbon, while those of the fœtal placenta perform the reverse operation[2]. It must farther be remarked,

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searches of Dr. R. Lee seem, however, to have proved, that there is no actual division of the placenta into a maternal and a fœtal portion, the whole of it being composed "solely of a congeries of the umbilical vessels." He is also led to conclude, "that there is no communication between the uterus and the placenta by large arteries and veins;" Phil. Trans. for 1832, p. 57 et seq.

[1] Haller, El. Phys. xxix. 3. 28; Blumenbach's Inst. Physiol. § 575. p. 320; Magendie, Physiol. t. ii. p. 443; Monro's (tertius) Elem. v. ii. p. 608; See also Darwin's Zoonomia, v. i. sect. 38; Jeffray, de Placenta, p. 32; Murat, Art. "Fœtus," in Dict. Sc. Méd., where the reader may find a more copious than select list of references. See also the elaborate, but somewhat diffuse art. "Oeuf", by Desormeaux, in Dict. de Méd. t. xv; he believes in the existence of a direct vascular connexion between the mother and the fœtus, founding his belief on the experiments of Dr. Williams; p. 309, 0. I may also refer to the art. "Fœtus", by Dugés, in Dict. Méd. Chir. prat. t. viii. p. 290 et seq., where we have many important physiological observations, although the author professes to treat exclusively on its diseases. Prevost and Dumas announce the curious discovery, that the red globules in the blood of the fœtus differ in their form and volume from those of the adult, the former being double the size of the latter; this fact, if it be confirmed, must decide the question respecting the vascular communication in the negative; Ann. Chim. et Phys. t. xxix. p. 108; and Ann. Sc. Nat. t. iv. p. 499. The instances, which are not unfrequently met with of extra-uterine fœtuses, appear to furnish a strong argument against the existence of a direct vascular communication. The fœtal placenta, in these cases, attaches to any part of the abdominal viscera to which it is contiguous, and in this state the fœtus grows and is nourished, although it is obvious that it can have no vascular connexion with the organs to which it is attached. An example of this kind is detailed by Dr. Baillie, with that perspicuity and correctness which are so characteristic of the author; Works, by Wardrop, v. i. p. 226 et seq. See also a case of this description by Lallemand, Observ. Pathol. p. 17 et seq. Such cases afford us very interesting examples of the powers of the system to adapt themselves to extraordinary circumstances, in consequence of their assuming vicarious functions.

[2] Mayow appears to have been the first who entertained a correct opinion respecting the use of the placenta, as an organ supplementary to the lungs; he also extended his views to the chick in ovo; Tract. p. 131 et seq. He had *not*, however, a very clear conception of the manner in which the nitro-aerial

that a small quantity of effect is all that we require in the case of the fœtus, some of its functions being not yet called into action, and the remainder acting only in a very limited degree, the mother still supplying the wants of the fœtus and superseding many of those causes of expenditure which exist in the animal after birth [1].

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particles were obtained by the blood of the fœtus, or by the fluids of the egg; p. 313, 318, 322. Ray states his opinion on this point very clearly; he says that the fœtus "receives air..from the maternal blood by the placenta uterina ..," an opinion which he informs us he obtained from Dr. Ed. Hulse; Wisdom of God &c. p. 73. This doctrine, after that period, seems to have been almost forgotten or neglected, until near the end of the last century. Before that time the placenta was regarded as an organ of nutrition, and the great subject of controversy was, whether the fœtus was nourished entirely by this organ, or by the placenta in conjunction with the mouth. A good view of the state of opinions in the earlier part of the last century may be found in two papers in the 1st and 2nd vol. of the Edinburgh Medical Essays, by Gibson, vol. i. p. 171 et seq., who maintained that the fœtus is nourished, partly by the placenta and partly by the liquor amnii, and by Monro, primus, vol. ii. p. 121 et seq., who argued that the placenta is the only organ concerned. Neither of these writers had any idea of the placenta being an organ supplementary to the lungs, nor do their contemporaries in general seem to have been aware of the necessity of any such organ. I may remark that Mayow supposed the placenta to perform the office of nutrition, as well as to produce the appropriate change in the blood; p. 319, 322. Some physiologists who conceived it necessary that the blood should be purged or purified, as they expressed it, during the course of the circulation, supposed, from the great size of the liver, that it performed this office. Haller discusses the question concerning the share which the placenta has in the nutrition of the fœtus; El. Phys. xxix. 3. 10, 1; he dismisses the inquiry respecting any farther use which the placenta may serve in a very few words, and although he refers to Mayow, he does not attach any importance to his doctrine, § 37. Sir E. Home gives us an interesting account of the mode in which the blood of the fœtus, in the various classes of animals, is enabled to undergo its appropriate change, either by being exposed to the action of the atmosphere, as in the case of the eggs of birds, of water containing a portion of air dissolved in it, as in various species of aquatic animals, or of the arterial blood of the mother, as in the mammalia; Phil. Trans. for 1810, p. 213..7. Breschet informs us, that he has discovered a peculiar green matter in the placenta of certain animals, which he conceives to bear some resemblance to bile, and hence supposes that there is a connexion between the functions of the placenta and the liver; Ann. Sc. Nat: t. xix. p. 379 et seq.

[1] A very clear description of the modern doctrine on this subject is given by Dr. Jeffray, in his Thesis de Placenta; a work which contains a judicious summary of the opinions that had been previously entertained upon the subject, together with many important original observations. We have some valuable remarks in Bichat, Anat. gén. t. i. p. 348, and Sur la Vie &c., p. 82 et seq., mixed up, however, with much incorrect hypothesis, depending upon the metaphysical ideas which he entertained respecting the relation between the vital functions. Mayow was quite aware of the degree in which the functions of the mother superseded those of the fœtus, p. 322; he limits the actions of the fœtus almost exclusively to the power of supporting its muscular contractility, and I may remark, that if an arterialized state of the blood be necessary for muscular contraction, it is essential that the fœtal blood should experience an equivalent change, to maintain the action of its heart. We have some judicious observations by Mr. Coleman on the state of the fœtal circulation, as connected with its other functions, Dissert. p. 46 et seq.; see also Legallois, Sur la Vie, p. 248, 9, on the fœtal functions. Dr. Edwards

The state of the chick during incubation, although differing so considerably in its anatomical structure and arrangements of

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found the temperature of a seven months' child to be only 89½°; De l'Influence &c. p. 236. Dr. Williams, of Liverpool, has lately instituted a series of experiments, the object of which is to prove the existence of a direct communication between the sanguiferous vessels of the mother and the fœtus. He operated upon dogs, and the plan which he pursued was to open a pregnant animal, immediately after it had been deprived of life, while the capillaries might still be supposed to retain their contractility, and to inject oil of turpentine coloured with alkanet root into the descending aorta. When the blood of the mother was supposed to be sufficiently impregnated with the oil, one of the pups was removed from the uterus, and its vessels being opened; a portion of the oil was found to have entered into them. It was detected either by suffering the blood of the fœtus to drop upon paper, to which it imparted a greasy stain, or the vessel was opened under water, in which case small globules of oil were observed floating upon the surface. Being in Liverpool in the autumn of 1824, in company with Dr. Roget, we were present at one of these experiments. This, however, as Dr. Williams candidly admits, was not successful, owing, in a great measure, as he supposed, to the size of the animal upon which he operated, being too large in proportion to the syringe and the quantity of injection which was employed. We also suggested that the result was liable to deception, in consequence of the peculiarly adhesive nature of the oil, which would cause it to adhere to the apparatus or the fingers of the operators, and might thus be accidentally smeared over the surface of the pup, or in some way interfere with the result. Dr. Williams has since endeavoured to obviate this objection by using rape oil, and by afterwards carefully washing the animal in an alkaline solution. An experiment is related, in which these precautions were employed, yet where the oil was still detected in the blood of the fœtus. The experiment is one which leads to such important conclusions, that I shall offer no apology to the author for making my remarks upon it without reserve. In the first place, the oil does not seem capable of penetrating into the vessels of the fœtus, unless it be employed in considerable quantity, and injected with considerable force; is there not therefore some reason to suspect that there may have been a rupture of the delicate cellular texture which is supposed to separate the maternal from the fœtal vessels? 2d. Notwithstanding the care that was taken to wash off the oil, I conceive that it must be very difficult entirely to remove this cause of inaccuracy; it would therefore be desirable to employ some other substance, that is not liable to this objection, which might be dissolved or suspended in the blood of the mother and afterwards detected in that of the fœtus, by means of an appropriate chemical reagent. 3d. It seems to be agreed by all anatomists, and is admitted by Dr. Williams himself, that mercury cannot be made to pass from the mother to the fœtus, without an obvious extravasation taking place, a circumstance, which is at least a presumption against the existence of any natural passage, through which the oil could pass from one system of vessels to the other. 4th. It is known that when we draw off a large proportion of the blood of the mother, the quantity of blood in the fœtus does not appear to be diminished. 5th. The nature of the fœtal circulation, both as to the quantity of the blood, the rapidity of its motion, the number of its red particles, and more especially their different form and volume, seem to indicate that there can be no direct channel of communication between the two sets of vessels, so as to constitute them parts of the same circulating system. Should future experiments confirm those of Dr. Williams, the degree of effect would rather indicate some peculiar connexion, essential indeed to the existence of the fœtus, but different from the ordinary circulation of the blood, as it takes place in the other parts of the sanguiferous system. Raspail, in his remarks on the "formation of the placenta," *§ 621* et seq., remarks that there is no vascular connexion, but that the parts, physiologically considered, bears a close resemblance to the fœtus in utero. We have here an organ analogous to the placenta, in the form of a fine net-work of vessels, distributed on the external surface of the contents of the egg, which receive the blood from the embryo in the venous and return it in the arterialized state, the shell being provided with a number of pores, which permit the air to act upon the blood, and thus enable it to undergo its appropriate change. Hence we find that a free access of air is as necessary to the evolution of the chick as to the existence of an animal with lungs, so that if the egg be completely smeared over with varnish, the chick is as effectually destroyed, as the animal after birth would be by submersion or suffocation[1].

There is a subject connected with the effect of respiration on the living system, which must be noticed in this place, both as in itself sufficiently curious to demand our attention, and likewise because it has been supposed to throw light upon the theory of respiration, or upon the mode in which it affects the vital functions. I refer to the peculiar sensations which are experienced at great elevations. These sensations have been supposed to be connected with the action of the lungs, both because a change in the density of the air is the only circumstance to which they can, with any probability, be assigned, and because the respiration appears to be generally affected by any change of this kind to which the lungs are subjected. The effect produced by ascending high mountains was distinctly noticed by Boyle; he ascribes it to the rarefaction of the air, but he does not satisfactorily explain how rarefied air should produce the feelings which are experienced[2]. Haller[3], with his usual diligence, has collected accounts from various travellers who have ascended high mountains, but the result of their testimony seems to be adverse to the supposition that any peculiar or specific effect is produced in these situations from the state of

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certain fibrils and projections from the fœtal parts are inserted into corresponding cavities in the maternal part, and that these fibrils absorb nutrition and act on the blood.

[1] Blumenbach's Compar. Physiol. by Lawrence, p. 483; Paris on the Physiology of the Egg, Ann. Phil. v. ii. N. S. p. 2 et seq. As was remarked above, we are indebted to Mayow for the first clear conception of this subject, although on some minor points his opinion is probably not correct. Sir Ev. Home has given us a series of very interesting engravings, exhibiting the progressive changes which the egg undergoes during incubation; Phil. Trans. for 1822, p. 339 et seq. I must not omit to mention, that in two elaborate articles in Rees's Cyclopœdia, "Egg" and "Incubation," the doctrine maintained in the text, respecting the action of the air upon the blood of the chick in ovo, is controverted in all its parts.

[2] Works, i. 105, 6; iii. 374, 5. The individuals to whom he refers experienced affections of the stomach, as well as of the lungs. It is also noticed by Mead, who ascribes it to the extreme rarity of the air, so that enough of it cannot be taken into the lungs to inflate them; Works, v. i. p. 181.

[3] El. Phys. viii. 3. 7.

the air. He also informs us that this was the case with himself, in his expeditions among the Alps; he farther observes, that in many parts of Switzerland, there are individuals permanently residing at very considerable elevations, without experiencing any inconvenience, and it may be added that this still more remarkably takes place in some parts of the E. Indies and S. America[1]. It is also stated that no effects of an analogous kind have ever been noticed with respect to the different species of animals that are found in these regions. Haller is disposed to ascribe the peculiar sensations which have been occasionally felt upon ascending high mountains, rather to excessive fatigue or exhaustion than to any thing specifically depending upon the state of the air, an opinion which had been previously formed by Bouguier, from his experience of what occurred to himself on the Andes[2].

Saussure, however, who has given us a very minute account of his own sensations on the Alps, has formed a different opinion on this subject, and may at least lead us to doubt the correctness of Haller's conclusion. When he was at the height of above 8000 feet above the level of the ocean, he always experienced an extreme degree of fatigue and loss of muscular power, and this differed from ordinary fatigue in its coming on more rapidly, being quite irresistible, in being attended with violent palpitations of the heart and beating of the arteries,and it was remarkably distinguished by the very short space of time in which all the unpleasant sensations were removed, so that in two or three minutes he seemed to be perfectly recovered from a state of complete exhaustion, while, almost immediately upon resuming his exertions, the exhaustion, together with the total loss of muscular power, was again experienced. Saussure also mentions, as one of the specific effects of these situations, the great tendency to drowsiness, which is more than proportionate to the previous fatigue, and he remarks that sleep seized them immediately upon their being at rest, notwithstanding all the inconveniences of their situation, and the various circumstances, unfavourable to sleep, with which they were surrounded. He informs us that the affection exists in different degrees in different individuals, and that it is less observable in those who have been long habituated to these situations, although they are not exempt from it[1].

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[1] According to Lieut. Gerard, Marang, a large town on the Sutlej, is 8,500 feet above the level of the ocean, and Skipké 9,000 feet, Geol. Trans. v. i. N. S. p. 128, 9; the village of Misang 10,165 feet, Edin. Phil. Journ. v. x. p. 302, and Nako 11,550 feet; he farther states that fields are cultivated at an elevation of 13,000 feet; Brewster's Journ. of Science, v. i. p. 41 et seq. The height of the city of Quito is said to be above 9,000 feet; Jameson's Miner. v. iii. p. 333. Mr. Pentland has ascertained that there are habitations, in certain parts of S. America, at an elevation of 15,721 feet; the mines of Potosi at 16,000 feet; Ed. Rev. v. l. p. 366. The Post-house at Auromarco in Peru is 15,000 feet, and there are towns of considerable size above 12,000; See Jameson's Journ. No. 16. and No. 18. p. 391..4 and 345. Dr. Gerard ascended to the height of 20,000 feet among the Himalayas; he visited a village at an elevation of 14,700 feet, found corn fields at 14,900 feet, and saw goats feeding at 16, or 17,000 feet; Jameson, No. 15. p. 191, 2.

[2] Saussure, Voy. dans les Alpes, t. vii. p. 339; Bouguier, in his abridged narrative of the expedition to Pinchincha, undertaken by Condamine, himself, and others, ascribes the effect which some of the party experienced to fatigue rather than to any peculiar state of the respiration; but the symptoms which he relates do not justify this opinion; Mém. Acad. pour 1744, p. 261. These travellers spent three weeks on the summit of the mountain, the height of which is above 16,000 feet; it appears that they were less affected after remaining for some time in this highly rarefied atmosphere. In the history prefixed to the volume of the Mém. Acad. Scien. for the year 1705, it stated that Cassini and Maraldi experienced no affection of the breathing at an elevation where the atmosphere possessed scarcely more than half of its ordinary weight, p. 15. In the 29th volume of the Phil. Trans. p. 317 et seq., we have an account by Mr. Eden, of his ascending the Peak of Teneriffe, an height of above 12,000 feet; the narrative is written in a simple and unaffected style, and it appears from his remarks, that the respiration was not affected; he particularly states that "the report is false about the difficulty of breathing upon the top of this place;" p. 324. See the account of a late ascent up Caucasus, to a height of 14,800 feet; in Brewster's Journ. v. ii. p. 134; also the remarks of Dr. Dunglison, of Maryland, as stated in the British and Foreign Medical Review, p. 345.

The accounts that are given us by the late travellers among the Himalaya mountains and the Andes are not very uniform, with respect to the effects of these great elevations upon the respiration or the other functions of the body. Mr. Moorcroft, in his journey to the lake Manasarovara, unfortunately does not notice the heights of the country through which he passed, but they must necessarily have been very considerable[2]. He informs us that when he was at the village of Niti, his breathing was quickened, and that he was obliged to stop frequently in consequence of the increased action of the heart[3]. He experienced the same sensations in other places, and it is remarkable that his breathing was often oppressed while he was lying down, and especially, just before falling asleep[4], a circumstance in which it differs materially from what is described by Saussure. The difficulty of breathing was also felt by Capt. Webb, when he visited Niti, and he farther adds that horses are liable to it as well as men[5]. Lieut. Gerard, who appears to have ascended to greater heights among the Himalayas than any other individual, mentions that he suffered excessive debility and severe head-ache, but the respiration does not appear to have been much affected, although he was at elevations of 15, 16, 18, and even of 19,000 feet; indeed in the last case, he expressly states that he attained that great height "without much difficulty."[6] He, as well as the other travellers among these mountains, informs us that the natives attribute these peculiar effects to the poisonous exhalation or effluvia from certain plants which grow in these regions. Dr. Govan, on the other hand, who crossed the Himalayas at an elevation of considerably more than 15,000 feet, felt nothing peculiar in his respiration or other functions, nor was any one of a train of 40 natives who accompanied him in any way affected; yet he is aware that the sensations are frequently experienced and even at a much less elevation[1]. Mr. Caldcleugh also, who twice crossed the Andes, at a height of between 12 and 13,000 feet, seems to have experienced nothing more than what might be reasonably ascribed to the fatigue of the journey, nor did any thing particular occur to his guides and attendants[2].

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[1] Voyages dans les Alpes, t. ii. § 559..561; t. v. § 1280; t. vii. § 2021.

[2] Asiatic Researches, v. xii. p. 375 et seq.

[3] Ibid. p. 397.

[4] Ibid. p. 399. 407, 8, 9. 412, 4.

[5] Quart. Journ. v. ix. p. 65.

[6] Geol. Trans. v. i. N. S. p. 124 et seq.; Edin. Phil. Journ. v. x. p. 295 et seq.; Brewster's Journ. of Science, v. i. p. 41 et seq.

Upon the whole I think it highly probable that a part of what Saussure experienced depended upon something more than mere fatigue, and that certain specific effects are produced on the system by the rarity of the air. He is himself disposed to account for these effects on mechanical principles, depending on the diminished pressure to which the body must be subjected under these circumstances[3], while others have thought that the want of a due proportion of oxygen would afford a more easy explanation of them. I think that we are scarcely in possession of any facts which will enable us to decide upon the merits of the first of these hypotheses; as for the second, I conceive that it cannot be maintained, because the effects which have been found to follow from the respiration of air which is deficient in oxygen are different from those described by Saussure[4].

[1] Brewster's Journ. of Science, v. ii. p. 282.

[2] Travels in S. America, v. i. p. 309; v. ii. p. 110.

[3] See also Sauvages, Œuvres Diverses, t. ii. p. 164, referring to the description of Bouguier, who had previously ascribed it to the same cause.

[4] Dr. Edwards ascribes part at least of the effect which is produced upon the breathing by great elevations to the increased evaporation which will take place from the skin and lungs; De l'Influence &c. p. 493 et seq. The rarity of the atmosphere in these situations would, no doubt, tend to promote the evaporation, but, on the other hand, it must be checked by the low temperature; and although, in certain states of the atmosphere, the air of high mountains appears to be peculiarly dry, it is frequently in the contrary state. Dr. Edwards observes, that uneasy sensations are occasionally felt when the air of an apartment is rendered dry by the mode in which it is warmed. He is correct in the observation; but, it may be remarked, that the feelings which take place in these cases do not seem, in any respect, to resemble those that are experienced on high mountains; p. 498, 9. Mr. Fellows and Mr. Auldjo, in their interesting accounts of their ascent to the summit of Mont Blanc, describe the effect on the respiration nearly in the same way with Saussure; but it may be presumed, that a part at least of the inconvenience which they experienced may be attributed to their extreme exhaustion, especially in the case of Mr. Auldjo. It has been conjectured, that the great cold which exists at these elevations may contribute to the uneasiness and derangement of the functions, by enduring a degree of torpidity; I conceive that some of the narratives referred to above appear to sanction this opinion. The latest account which we have of the ascent of Mont Blanc is that of Dr. Barry; he began to feel the exhaustion at the elevation of 14,700 feet; it was quickly removed by rest; it was not experienced by the guides.

We have now considered the various remote effects of respiration, which are more immediately connected with, or dependent upon, a chemical change in the nature of the substances concerned. I must next proceed to give some account of those that are more of a mechanical nature. I have already had occasion to remark upon this subject in the first section of this chapter, when I endeavoured to show that the earlier physiologists, in consequence of their limited knowledge respecting the nature of the air, were disposed to regard the effects of respiration as almost exclusively of a mechanical nature, so that they were generally much exaggerated, while many of them were probably altogether imaginary.

There is, however, one very important function, which is necessarily connected with the lungs, and which may be classed among the remote effects of their action, the formation of the voice[1]. The superior extremity of the trachea is furnished with a number of cartilages of a peculiar form, which constitute the larynx, in the upper part of which is a chink or cleft, called the glottis[2]. The cartilages which compose the larynx are connected to each other by an apparatus of muscles, so constructed, that the aperture may have its form and dimensions very considerably varied; and, if the air be forcibly propelled from the lungs through the glottis, according to the form and size of the aperture, the different vocal sounds will be produced. The muscles of the glottis are under the control of the will, and we are enabled, by our voluntary efforts, operating through the medium of these muscles, to produce all the vocal or musical tones of which the voice is susceptible[3].

We have an interesting narrative of an ascent up Chimborazo by Boussingault, in Ann. Chim. t. lviii. The party attained a height of about 19,700 feet; they experienced a great degree of the exhaustion, so as to be under the necessity of stopping after every two or three steps, in order to recruit. They took up the mules to a little above the height of Mont Blanc, when the respiration of these animals seemed to be considerably affected. They thought that they experienced more of the peculiar effect when they were passing over snow than over the bare rock, and they found that the air contained in the snow had a less proportion of oxygen than that of the atmosphere, a circumstance which had been likewise noticed by Saussure.

[1] So little advanced was our knowledge of the nature of respiration, even in the middle of the last century, that Haller supposed the formation of the voice to be one of the principal uses of this function; El. Phys. viii. 5. 23.

[2] The most complete and original description of the larynx is that given by Mr. Willis, in the Cambridge Phil. Trans. v. iv. p. 323 et seq., with accompanying plates.

[3] An accurate delineation of the muscles of the larynx, and the mode in which they are connected together, may be seen in Albinus's description of the muscles; Tab. 11. fig. 44..48; Tab. 12. fig. 1..7; also in Cloquet, Anat. de l'Homme, t. ii. pl. 112, 3. A good deal of discussion formerly took place respecting the effect of tying or cutting the nerves that are distributed over these muscles; but it is now generally admitted, that when the nervous communication is entirely intercepted, the voice is destroyed. A part at least of the uncertainty which attached to the subject depends upon the circumstance, that it was thought necessary to *operate* upon the recurrent nerves alone,

Speech depends upon another series of actions, connected with the muscles of the tongue and lips, which, although they are distinct from those that are concerned in the formation of the voice, are, like them, connected with the respiration, as articulate sounds necessarily depend upon the emission of air from the lungs[1]. Besides the cartilages and muscles that

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whereas these are not the only nerves that supply these parts. Haighton's experiments led him to conclude, "that the recurrent branches of the par vagum supply parts which are essentially necessary to the formation of the voice; while the laryngeal branches of it seem only to affect its modulation or tone." Mem. Med. Soc. v. iii. p. 435, 6. Vid. supra, p. 389. Probably some of the uncertainty which has prevailed respecting the use of these nerves may be attributed to the curious fact, which was first established by Haighton, that a divided nerve possesses the power of uniting and having its original properties restored; this he proved both with respect to the nerves which are concerned in the voice, ibid. p. 436..8, and the main trunk of the par vagum; Phil. Trans. for 1795, p. 190 et seq.; see Blumenbach, Inst. Physiol. § 156, p. 89, also note A; Legallois, Sur la Vie, p. 187 et seq.; Magendie, El. Physiol. t. i. p. 206, also p. 208 et seq. For our first accurate opinions respecting the anatomy and actions of the organs of the voice we appear to be indebted to Fabricius, De Larynge, &c., Op. p. 268..317. Martine's paper in Edin. Med. Essays, v. ii. p. 114, may be read for the opinions which prevailed on this subject about the beginning of the last century. See Haller, El. Physiol. ix. 1. 1..29, for an account of the larynx; also Monro's (tert.) Outlines, v. ii. p. 411 et seq., and Elemen. v. ii. p. 73 et seq.; see also Cloquet's Man. p. 240..250, and pl. 121..4. For the comparative anatomy and physiology of the parts it will be sufficient to refer to Blumenbach, ch. 15. p. 278 et seq.; and to Cuvier, Leç. 28. t. iv. p. 445 et seq. We are indebted to Savart for some curious experiments on the nature of vocal sounds, and the manner in which their various modifications are effected. The experiments consisted in producing a succession of musical tones, by causing a current of air to pass through a tube with different degrees of velocity, the velocity being correctly measured and compared with the tones that were thus produced. He also found that the tones were affected by the size of the aperture, by the nature of the edge of the orifice through which the current passes, and by the substance of which the tube is composed. All these circumstances he applies to the organs of the human voice, and shows how the different parts act in the formation of its different sounds and tones; Magendie's Journ. t. v. p. 367 et seq. The instrument employed by Savart, which is called the syren, was invented by Cagniard de la Tour. See Mayo's Physiol. p. 341 et seq., where we have many interesting remarks on the mechanism of the vocal organs and the mode of their action. See also the elaborate disquisition of Mr. Willis, on the vowel sounds, in the Cambridge Phil. Trans. v. iii. p. 231 et seq. Bennati has lately published some observations on the mechanism of the voice during singing; his most important conclusion is, that it is not merely the muscles of the larynx which modulate the sounds, but those also of the os hyoides and the other neighbouring parts; Ann. Sc. Nat. t. xxiii. p. 32 et seq. and Magendie's Journ. t. x. p. 179 et seq., with Cuvier's report of the same, p. 233 et seq.

[1] Sir C. Bell, in his account of the organs concerned in the production of vocal and articulate sounds, observes, that the change in the shape of the mouth and pharynx are important agents in the process. He enumerates the succession of actions which must be performed before a word can be uttered; they consist in the compression of the thorax, the adjustment of the glottis, the elevation or depression of the larynx, and the contraction of the pharynx; Phil. Trans. for 1832, p. 299 et seq. We have an interesting case of what may be considered as the separation of voice from the speech in Magendie's Journ. t. ix. p. 119 et seq.; it occurred in a galley-slave at Toulon, who

compose the larynx, there are several ligaments, which serve to connect the various parts, and which, from their supposed use, have been termed vocal cords. It was a question that has been much agitated, and especially among the French physiologists of the last century[1], whether the musical tones of the voice depend upon the size of the aperture, or upon the degree of tension of these ligaments; whether the larynx was more analogous to a wind or a stringed instrument. Although it may not be very easy to give a decisive proof of either of these hypotheses, yet if we are to adopt one in exclusion to the other, I conceive that it is more probable that the ligaments serve to

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attempted to destroy himself by cutting the throat; he survived, but with a large permanent opening into the larynx, through which the respiration was carried on.

[1] The principal writers in this controversy were Dodart and Ferrein; the former endeavoured to prove that the tones of the voice are regulated entirely by the opening of the glottis; Mém. Acad. pour 1700, p. 244 et seq., et pour 1707, p. 66 et seq.; his first paper contains an account of the opinions of the ancients and the earlier of the moderns. Ferrein, on the contrary, Mém. Acad. pour 1741, p. 409 et seq., compares the larynx to a violin, p. 416, or a harpsichord, p. 422, and conceives that the voice is produced by the vibrations of the lips or ligaments of the glottis; he goes so far as to compare the air to the bow acting upon these parts; p. 416. See Haller, El. Phys. ix. 3. 1..17. Blumenbach supposes the larynx to be analogous to the flute, Inst. Physiol. by Elliotson, sect. 9. p. 87 et seq. Dr. Young adopts an opinion more like that of Ferrein, that the ligaments of the glottis act like strings; Lect. v. i. p. 400, 1; see also Phil. Trans. for 1800, p. 141, 2. Nearly the same doctrine is maintained by Sœmmering, with respect to the vibration of the ligaments, yet he conceives that the larynx resembles a pipe or flute; Corp. Hum. fab. t. vi. p. 93. § 94. We have a very elaborate description of the vocal organs and their mode of action, by Magendie; Physiol. t. i. p. 196 et seq.; both from the form and structure of the parts, and from experiments made upon the dead subject, he is disposed to refer the modifications of the voice principally to the vibrations of the ligaments, and supposes that they are analogous to the reed in the hàutbois; p. 207, 8. This hypothesis would, however, appear not to be original in Magendie, as it is directly controverted by Dodart; Mém. Acad. pour 1700, p. 246. We have some judicious remarks on Magendie's hypothesis in the Ed. Med. Jour. v. xv. p. 576. See also Good's Study of Med. v. i. p. 429 et seq. for the account of the vocal organs and the formation of the voice. I may refer to this author for a concise, but as it appears to me, satisfactory account of the curious art of ventriloquism; p. 435. Some of the older writers had supposed that the individuals who were possessed of this faculty, had an additional organ lower down than ordinary in the trachea, from which the name was taken. Mr. Gough, and others, ascribed it to echoes proceeding from the walls of the apartment, which deceived the audience, and were mistaken by them for the direct impulse of the voice. Dr. Good conceives it to be an imitative act produced by a peculiarly delicate modification of the glottis, while at the same time the sound is generally emitted through the nostrils instead of the mouth. On this subject, and on all those connected with the voice and speech, I may refer to the judicious summary of opinions that is given by Adelon, Physiol. t. ii. p. 204 et seq. We have a good view of the vocal cords, or Thyro-Arytenoid ligaments, as they have been termed from their anatomical relations, in Cloquet, pl. 122, fig. 4; see also Mayo, p. 344..7.

regulate the size and form of the aperture, than that they are themselves instruments of sound. A most curious part of the mechanism of the voice consists in the extreme delicacy with which we are able to modify its tones, and the power which we possess of imitating the tones of others. The same observation applies to the organs of speech, but as in this case the parts are exposed to view, we can more easily conceive how the process of imitation is conducted than when an internal organ is concerned, where the operation is entirely concealed from our sight. But this point will be considered more fully in a subsequent part of the work.

The great diversity of articulate sounds, and the celerity with which we are able to accomplish the necessary muscular contractions, have been frequently commented upon by physiologists, and it may be asserted, that as the gift of speech is one of those powers which eminently distinguishes the human species from all other animals, so there is none in which both the mechanism by which it is produced, and the acquired perceptions with which it is associated, afford a more worthy subject of our admiration[1].

There are a variety of actions, partaking more or less of a mechanical nature, in which the lungs and chest are essentially concerned, depending upon some variation in their bulk, the extent or velocity of their action, or the manner in which they affect the contiguous parts. Some of these are, to a certain degree, instinctive, being directly subservient to some useful purpose in the animal œconomy, while they are more or less independent of the will, such as sneezing and coughing. There are others, on the contrary, which are entirely under the control of the will, depending upon the contraction of the diaphragm or the muscles of the chest, which we call into action and regulate at pleasure, like other voluntary actions, such as sucking and straining. Some of these actions may be regarded as modifications of the voice, being characterized by distinctive sounds, essentially connected with their final cause, as laughing and weeping. The mechanical actions connected with respiration, which are enumerated by Haller[2] and Sœmmering[3] are the following; sighing, yawning, sucking, panting, straining, coughing, sneezing, laughing, weeping, hiccup, and vomiting[1].

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[1] In Blumenbach's Comparative Anatomy, ch. 15, we shall find the cause why no animal, except man, is capable of uttering articulate sounds; see also Camper's description of the larynx of the orang outang, Phil. Trans. for 1779, p. 139 et seq., from which we learn why this animal is in the same predicament. We have a minute account given us by Haller, El. Phys. ix. 4. 2...8. of the different sounds, and an analysis of the particular muscular contractions by which they are each of them produced. Also by Sœmmering, Corp. Hum. fab. t. vi. p. 103, sect. 113 et seq.; by Blumenbach, Physiol. sect. 9. § 160 et seq., p. 91 et seq.; and by Dr. Young, Lect. v. ii. p. 276 et seq. We have a minute account of the mode in which the various vocal sounds are produced, by Dr. Gordon, art. "Deaf and Dumb," in Brewster's Encyc. See also Mayo's Physiol. p. 354 et seq.

[2] El. Phys. viii. 4. 30..40.

[3] Corp. Hum. fab. t. vi. p. 79. § 80..90.

Sighing consists in a full and protracted inspiration, by which the cavity of the chest is considerably augmented; its final cause appears to be to promote the passage of the blood through the pulmonary vessels and to enable the air to act more fully upon it. Yawning also consists in a full, slow, and long inspiration, but it differs from sighing in being followed by a slow and full expiration; it is also attended by an involuntary opening of the jaws, by which the air has a more free admission to all parts of the chest. In sucking we apply the lips closely to the vessel containing the fluid, and by making an inspiration, we increase the capacity of the chest; the air in the mouth and fauces thus becomes rarefied, and the pressure of the atmosphere causes a portion of the fluid to enter the mouth. Panting consists in a succession of alternate quick and short inspirations and expirations, and thus produces a frequent renewal of the air in the lungs, in cases where the circulation is unusually rapid, or where, from some obstruction in the chest, we require a more than ordinary supply of fresh air. In the act of straining, we commence by a full inspiration, and retain the air in the chest, while, at the same time, we contract the abdominal muscles. By this means we not only compress the viscera, and expel their contents, but the flow of the blood is retarded, and it has a tendency to accumulate in the venous part of the circulation. The act of straining enables us to exercise the greatest degree of muscular power, because the trunk becomes firmly fixed and serves as the point in which the actions of all the muscles are centred. Coughing is produced by a quick and forcible contraction of the diaphragm, by which a large quantity of air is received into the chest; this, by a powerful and rapid contraction of the abdominal muscles, is propelled through the trachea with considerable force, and in this way dislodges mucus, or any other extraneous substance which irritates the part. When the irritation is considerable, it is involuntary, although, in other cases, it is under the control of the will. Sneezing, in many respects, resembles coughing, but it differs from it in being more violent and in being involuntary. The irritation is applied to a more sensible part, the inspiration with which it commences is more deep, and the succeeding expirations are more violent, and are directed through the cavities of the nose. The final cause of sneezing is obviously for the purpose of removing any irritation from these passages, and by means of the interesting observations of Sir C. Bell, to which I have so frequently referred, we are able to trace the nervous communications which connect the mucous membrane of the nose, with the muscles that are concerned in respiration[1], but there is still some difficulty in explaining the physical causes of coughing and sneezing as distinguished from each other. Laughing is produced by an inspiration succeeded by a succession of short imperfect expirations. Although it may be produced by certain bodily sensations, yet for the most part, it depends upon a mental emotion; the theory of laughter, or the connexion which there is between the action and the causes which excite it, is somewhat obscure. The action of weeping is very similar to that of laughing, although its causes, both corporeal and mental, are so dissimilar. It consists in an inspiration, which is succeeded by a succession of imperfect expirations. Both laughter and weeping are supposed by many physiologists to be confined to the human species; but we may observe approaches to them in some of the animals which, in other respects, exhibit the most intelligence and sagacity. Hiccup is a quick, involuntary, convulsive contraction of the diaphragm, occurring at intervals, and produced by irritation of the cardiac extremity of the stomach, the gullet, or other neighbouring part. The only remaining action which is connected with the respiratory organs is vomiting, but as this involves some physiological considerations, which are connected with the functions of the digestive organs, it will be more properly considered in a subsequent part of the work.

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[1] Of this list Blumenbach omits sucking, panting, straining, and vomiting; Physiol. sect. 9. § 162. p. 92, 3. He only enumerates those which he regards as modifications of the voice. See also Sprengel, Instit. Med. t. i. sect. 4. § 220..5. p. 490..7.

SECT. 7. *Of Transpiration.*

As the functions of the skin, and the action which it exercises over the animal œconomy, are generally supposed to bear a considerable analogy to those of the lungs, it may be convenient to introduce in this place an account of the cutaneous transpiration. It seems to have been a very early opinion among physiologists, that besides the visible matter of perspiration, a species of invisible vapour is likewise discharged from the surface of the body, and that this discharge is connected with some of the most important operations of the system[2]. The first person who endeavoured to ascertain the amount of this vapour, or, indeed, who may be said to have adduced any very unequivocal proofs of its existence, was Sanctorius[3]. He devoted his almost undivided attention to this subject for the greatest part of his life, and although we shall probably be inclined to think, that the information which he obtained from his researches was, by no means, proportionate to the labour which he bestowed upon them, yet so pre-eminent was he above his contemporaries, as an inquirer into the operations of the living body, by the mode of experiment, that he obtained the highest degree of celebrity.

The method which Sanctorius adopted to measure the quantity of the insensible perspiration, as he termed it, was to notice accurately the food that was received into the body, and all the discharges that proceeded from it; the former of these quantities was found, in all cases, very considerably to exceed the latter, and this excess was supposed to be transpired from the skin, in the state of invisible vapour. By comparing the weight of the body under all the circumstances to which it is exposed, or by which its functions are modified, as well in health as in disease, a due allowance being always made for the proportion of the ingesta to the egesta, he endeavoured to ascertain the amount of the insensible perspiration during these different states, and he deduced from them a series of aphorisms, which were supposed to contain the general deductions from his almost innumerable experiments[1]. It is, however, not a little remarkable, and certainly much to be regretted, that he no where gives us any exact numerical account of his results, and from his having, in most instances, blended his theories with his experiments, and admitted a certain share of hypothesis into his conclusions, there are comparatively but few of them on which we can place much confidence, or at least, which we can adopt without making many exceptions and allowances.

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[1] See particularly the observation in Phil. Trans. for 1822, p. 287 et seq.

[2] *Haller*, El. Phys. xii. 2. 4.

[3] Ibid. xii. 2. 10.

The example of Sanctorius was followed by several other physiologists, who, partly, in consequence of their being aware of his deficiencies, and partly, from the improvements which took place in the mode of conducting philosophical inquiries, employed a more satisfactory method both of executing and of detailing their experiments, many of which appear to have been pursued with much assiduity, and recorded with great accuracy[2]. Among those who seem to have been the most successful in their investigations on this subject, we may enumerate Dodart[3], Keill[4], Rye[5], Gorter[6], Lining[7], Robinson[8], Home[9], and Stark[10], all of whom performed experiments, more or less resembling those of Sanctorius, and greatly added to their value by generalizing the results, or reducing them into the form of tables, exhibiting the loss which the body is supposed to sustain by its insensible perspirations in various constitutions, ages, and temperaments, at different periods of the day and seasons of the year, after exercise and repose, after fasting and taking food, sleep and watching, and various other circumstances of an analogous nature. They proceeded upon the plan of Sanctorius, of weighing the body, adding the aliment received, deducting the discharges, and placing the loss which it had sustained to the account of the cutaneous transpiration. Without impeaching the accuracy of the operator, we should expect that experiments performed upon different individuals, and under such a variety of circumstances, must afford very various results, and accordingly, the quantities obtained are so different from each other, that it seems almost impossible to draw from them any satisfactory average. If I were to select any of these more early experiments, as having been apparently conducted in the most judicious manner, and with the greatest accuracy, it would be those of Rye; his general result is, that the excess of the weight of the ingesta over the egesta, that of the body remaining the same, is 57 ounces in 24 hours[1].

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[1] Medicina Statica; this celebrated work appears to have been published in the year 1614, at Venice, "unde," as Haller observes, "innumerabiles editiones prodierunt."

[2] We shall find in Haller, El. Phys. xii. 2. 10, 12, 13, an account of the successive sets of experiments which were performed on this subject, detailed with his usual minuteness and correctness.

[3] In Mém. Acad. t. ii. p. 276. there is a notice of these experiments.

[4] Medicina Statica Britannica, a small treatise appended to his "Tentamina."

[5] Medicina Statica Hibernica, appended to Rogers on epidemic diseases.

[6] Gorter, de Perspir. insens. cap. 2.

[7] Lining's observations were made at Charlestown, and contain the result of a year's experiments performed on his own person; Phil. Trans. for 1743, p. 491 et seq., and for 1745, p. 318 et seq.

[8] "Dissertation on the food and discharges of human bodies;" a learned treatise proceeding upon the principles of the mechanical physiology, containing, however, many important observations both original and collected.

[9] Medical Facts, p. 235..253.

[10] Works, p. 169..182. Stark's experiments, there is every reason to believe, were executed with the most scrupulous accuracy, and afford us many valuable results, but, as they were performed principally with a view to ascertain the comparative effect of different kinds of diet, upon the cutaneous transpiration, they are not adapted to the purpose of comparison with the

But besides other sources of inaccuracy, depending either upon the mode in which the experiments were performed or the false hypothesis with which they were frequently implicated, there was one fundamental error which pervaded the whole, that the action of the lungs was necessarily confounded with that of the skin. Although it was known that a quantity of aqueous vapour is discharged along with the air of expiration, yet it seems to have been, in a great measure, disregarded in all the calculations, and no one attempted to estimate its amount, with any degree of accuracy, before the time of Hales. I have already had occasion to notice the result of Hales's experiments[2], and of the others which were afterwards instituted for the same purpose, which showed that the quantity of water expired from the lungs is so considerable, as to induce Haller to conclude, that in order to compensate for this, and for some other excretions which had been neglected in the calculations, one-half of the estimated quantity of the insensible perspiration should be deducted, which would reduce the average to about 28 ounces in

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other experiments that are referred to above; his general conclusions are stated in p. 178.

[1] He found the greatest loss of weight in the summer months to be 93 ounces, the least 33, giving an average of 63 ounces; the greatest loss in the winter months was 60, the least 42, giving an average of 51, or of 57 for the whole year. Rogers, p. 310. Aphor. 99. Lining, whose experiments were made in a much warmer climate, found the proportion of the summer to the winter perspiration to be in the proportion of 2·06 to 1, in each case taking the average of the same length of time, 30 days; Phil. Trans. for 1743, p. 509.

[2] P. 359.

the 24 hours[1]. This, however, must be regarded as a very vague estimate, that is imperfect in all its essential points; for not only was the mode employed to ascertain the quantity of water expired altogether inadequate to the purpose, but the other effects which the lungs produce upon the air were, at that time, entirely unknown. We have, however, found that one of the most important of these effects consists in the abstraction of a portion of carbon from the blood, which, as well as the aqueous vapour, must, accordingly to the mode in which the experiments were conducted, have been necessarily confounded with the cutaneous transpiration.

After the time of Haller we have no experiments on the cutaneous transpiration, which can be regarded as particularly deserving our attention, until the celebrated ones, to which I have already had occasion to refer[2], that were performed by Lavoisier and Seguin[3]. As these appear to have been executed with great dexterity, and with an apparatus much superior to any which had been hitherto employed in physiological researches, so the conclusions to which they lead are proportionally important, and although they will be found to be not unobjectionable, yet they very materially advanced our knowledge of the animal œconomy, at the same time that they gave a fresh impulse to the progress of inquiry, and opened a new path for future investigations.

The authors commence by observing, that transpiration consists in the evaporation of a quantity of water from the body, part of which proceeds from the pores of the skin, and part from the inner surface of the vesicles of the lungs, forming respectively the cutaneous and the pulmonary transpiration. Hence there are three distinct operations going forwards in the living system, which affect the weight of the body, and which have been generally confounded together in the statical experiments that have been performed upon it, but which it is necessary to distinguish from each other,—the cutaneous transpiration, the pulmonary transpiration, and the respiration. In order to separate these effects from each other, the body was enclosed in a silk bag, varnished with elastic gum, so as to render it air-tight, while a tube was adapted to the mouth of the operator, by which means it was conceived that the effects of transpiration and of respiration would be kept distinct from each other. This, however, would not be accomplished by the arrangements of the apparatus; they would preserve the cutaneous transpiration distinct from the respiration, but this last would not be kept separate from the pulmonary transpiration. In performing the experiments the body was weighed before entering the apparatus, and again just before leaving it, for the purpose of finding what weight is lost by respiration as distinct from transpiration; it was again weighed immediately after leaving the apparatus, by which the total loss of weight was obtained, and the former quantity was subtracted from the latter, in order to discover the loss by transpiration alone. But it is obvious that, upon this plan, the effects of the pulmonary transpiration and of the respiration are confounded together, nor are there any direct experiments by which the quantity of the pulmonary transpiration was attempted to be ascertained.

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[1] El. Phys. xii. 2. 11.

[2] P. 343.

[3] Mém. Acad. Sc. pour 1790, p. 601.

In giving an account of Lavoisier's opinion respecting the change which the blood experiences by respiration, I have mentioned that he conceived the water which is expired from the lungs to be, in part at least, actually generated in that organ by the union of oxygen and hydrogen; and in examining into the nature of the pulmonary transpiration he takes occasion to recur to the same hypothesis. He remarks that a substance which is chiefly composed of hydrogen and carbon, is secreted from the blood, and transudes through the membranes of the lungs into the bronchia: that while it is passing through the exhalent vessels in an attenuated state, it is brought into contact with the oxygen contained in the air of inspiration, with which it unites and is converted into water and carbonic acid. Besides the water which is thus produced in the lungs, a quantity is supposed to transude through the membranes of the vesicles ready formed; they are both of them reduced into vapour by the warmth of the part, and compose the pulmonary transpiration. A calculation is then entered upon for the purpose of determining the quantity of water which is transpired by the lungs, but it proceeds upon data which I conceive are not correct. The authors lay it down as the basis of their reasoning, that the whole of the oxygen which is consumed in respiration is united to hydrogen and carbon which it meets in the lungs, whereas I have endeavoured to show, that the union does not take place in the lungs, that a part of the oxygen is absorbed and appropriated to some other purpose, and that no combination of oxygen and hydrogen takes place. It is obvious that if any part of the above statement be found to be erroneous, it must vitiate the whole of the reasoning, as far at least as the quantity is concerned; so that although the results of the experiments are valuable as matters of fact, we are unable to follow the authors in the conclusions which they deduce from them.

With respect to the estimates, we are informed that the average quantity carried off by the cutaneous transpiration in 24 hours is 30 ounces, while that by respiration (including as it appears the pulmonary transpiration) is 15 ounces. Of these 15 ounces rather more than $\frac{1}{3}$ is supposed to consist of the water which is transuded through the lungs, properly constituting the pulmonary transpiration, the other $\frac{2}{3}$ consisting of the hydrogen and carbon, which is supposed to combine with oxygen and form water and carbonic acid, the quantity of the water being determined by that of the oxygen which is left after subtracting what is required for the formation of the carbonic acid. It appears, however, from the remarks that were made in a former section, that this hypothesis of the generation of water in the lungs is without foundation; so far, therefore, as the existence of this portion of water rests upon that hypothesis, it must be considered as equally unfounded.

There does not appear to be any theoretical objection to the method that was employed to ascertain the amount of the cutaneous perspiration, yet I conceive that many circumstances may occur which would practically interfere with the results, so that I think we can only consider it as an approximation, and that perhaps not a very close one, to the truth. The near coincidence between the quantity which Lavoisier and Seguin obtained, and the average formed by Haller, may appear remarkable, yet, I apprehend, that it is to be regarded as purely incidental, and not as, in any degree, tending to confirm the accuracy of the estimate. In the present state of our information, the only method that we seem to have of estimating the pulmonary transpiration, is to take the total quantity lost by the body, after making the due allowance for the excess of the ingesta above the egesta; from this we must deduct the loss by the cutaneous transpiration, and also the carbon which is emitted from the lungs; what remains may be supposed to be due to the pulmonary transpiration; but it is obvious that we can place very little dependence upon a calculation founded on such uncertain and inadequate data. The points which seem to be ascertained are, that the body loses a certain quantity of weight, besides what can be attributed to any of the visible discharges, and to the carbon and water which is emitted from the lungs, that a certain quantity of vapour is emitted from the skin, and it may be inferred that this is the principal source of the loss which is experienced. Lavoisier and Seguin do not expressly state what is their opinion respecting the origin of the matter of transpiration, but it would appear that they considered it as arising from a fluid which is secreted by the cutaneous vessels from the blood, and is converted into vapour by the caloric which it abstracts from the body.

We have some very interesting observations on transpiration by Dr. Edwards, the principal object of which is to illustrate the effect produced upon this function by the various circumstances to which the body is subjected. He began by a series of experiments on cold-blooded animals, as we have here the advantage of being easily able to obtain the cutaneous transpiration entirely distinct from the pulmonary, in consequence of the length of time which these animals can live without respiring; in this way he not only very unequivocally proved the existence of the transpiration by the skin, but ascertained with more certainty its comparative quantity in the different circumstances referred to above. In this case nothing more was necessary than to weigh the animal before and after the experiment, and to make allowance for the ingesta and the egesta, there being no other organ except the skin, by which any thing is removed which can affect the weight. Some of his most important results are, that the body successively loses less and less by transpiration in equal successive portions of time, depending, as we may presume, upon the absolute quantity of fluid in the vessels; that the transpiration proceeds more rapidly in dry than in moist air, in the extreme states, nearly in the proportion of 10 to 1; that temperature has also a great effect, the transpiration at 68° (20 C.) being twice as much, and at 104° (40 C.) seven times as much as at 32°. He also found that frogs transpire while they are in water, as is proved both by the diminution which they experience while immersed in water, and by the appearance of the water itself, which becomes visibly impregnated with the substance that is excreted from the skin[1].

Dr. Edwards then extended his researches to the warm-blooded animals. He found that in these, like the others, the transpiration became less in proportion to the quantity of fluid which was evaporated from the body; he also observed the same kind of difference between the effect of moist and dry air, and between a high and a low temperature. The experiments were performed on guinea-pigs and on birds; and it appeared that in all the essential particulars these two kinds of animals agreed in the mode of their transpiration as affected by external agents[2]. The transpiration of man was likewise found subject to the same general laws, but probably, in consequence of the greater delicacy of the human frame, and the more complicated nature of his functions, it is more subject to be disturbed by external agents. A circumstance is noticed by Dr. Edwards, which had not been sufficiently attended to by his predecessors, that in endeavouring to ascertain the comparative amount of the transpiration under different circumstances, it is necessary to take intervals of considerable length, in order that the results may not be influenced by the effect of the fluctuations which are always occurring, and which constituted one principal source of the apparent irregularities, that produced so many anomalies in the older experiments. A period of six hours was thought to be necessary, and this he accordingly employed in all his researches. With respect to the different states of the air, its effects upon the cutaneous transpiration were essentially the same on man as on other animals; among other observations he found that the transpiration was more copious during the early than the latter part of the day, that it is greater after taking food, and although most of the vital functions appear to be diminished during sleep, it appeared upon the whole, that the transpiration was increased during this state[3].

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[1] De l'Influence &c. part 1. C. 5, 6.

[2] Ibid. part 3. C. 7.

[3] De l'Influence &c. part 4. C. 11. Where the data are confessedly so *insufficient*, it may appear to little purpose to found any calculation upon

The author next proceeds to investigate the nature and source of the matter which is transpired. He begins by making a distinction between what is carried off from the body by evaporation, and what is removed from it by transudation; the first depending upon a mere physical operation, in which a substance is converted into vapour, by the addition of heat, while transudation is a vital process, of the nature of secretion or excretion. He observes that the terms evaporation and transudation are not synonymous with the insensible and sensible perspiration respectively of the older writers, because a part of what is removed by transpiration is first transuded, and then evaporated. Evaporation may take place from the dead body, while transudation can only take place from the living body; transpiration is, therefore, properly an operation of an intermediate kind, where the fluid is furnished by a vital function, while it is removed from the body by a mere physical process. The older

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them; still it may be desirable to form the best estimate that we have it in our power to deduce from them. The numbers will be as follows:

Ingesta in 24 hours.	oz.
Food, according to Rye	96
Oxygen retained in the system, vide supra, p. 361	4
	100

Egesta in 24 hours.	
Urine, according to Rye, } Rogers, p. 310, 1	40
Alvine discharge, Do. } Rogers, p. 310, 1	6
Various other excretions, see Haller, El. Phys. xii. 2. 11	3
Carbon discharged from the lungs, vide supra, p. 361	11
Water expired, according to Menzies, vide supra, p. 359	6
	66

We are indebted to Dr. Dalton for an account of a series of experiments, which he performed on his own person; they were continued for a considerable length of time, and agree nearly with those of Rye in their results, thus mutually confirming each other. He found the ingesta to amount to 91 oz. daily, and the egesta to 53½, thus leaving 37½ for the discharges from the skin and lungs; Jameson's Journ. No. 27. p. 62 et seq. The ingesta will therefore exceed the visible egesta by about 36 oz., which may therefore be assumed as the average amount of the transpiration in 24 hours. Perhaps a farther addition should be made to this quantity, in order to compensate for the water which is absorbed by the skin; for, as I shall have occasion to observe hereafter, it is probable that an action of this kind takes place, although it appears quite impossible to ascertain its amount. An objection, which is not without considerable weight, has been urged against the supposition of so large a quantity of water being discharged from the skin, that many nations are in the habit of smearing the surface of the body with substances, which it is supposed must prevent any thing from being discharged from it, Haller, El. Phys. xii. 2. 19; and many individuals fall under our observation, in whom, from various accidental circumstances, the skin is frequently so covered with extraneous substances, as apparently to obstruct any discharge from its pores. The only method of obviating this difficulty is to suppose that the pulmonary transpiration is in this case proportionally increased to supply the deficiency. I am not aware, however, that any experiments were ever performed on these individuals, and until this be done, it would be improper to speculate upon the subject.

physiologists were much divided respecting the question, whether the matter of the sensible and the insensible perspiration were originally the same substance, the former being in the fluid state, the latter in the form of vapour. Haller was inclined to suppose that they were essentially different[1], and Dr. Edwards appears to be of the same opinion, although it is not very clear, whether he considers that the whole, or only a part of the matter of the insensible transpiration is not derived from the sensible transpiration[2].

The two operations of evaporation and transudation being considered as, to a certain extent, independent of each other, it became an interesting object to endeavour to ascertain the degree in which they are each of them respectively exercised. For this purpose Dr. Edwards had recourse to cold-blooded animals, in which we can easily suppress the evaporation, by placing them in air saturated with moisture, and which will of course be nearly of the same temperature with themselves; in this case, therefore, we can obtain the loss by transpiration alone. By performing this experiment on frogs at a medium temperature not exceeding 68° (20 C.) the evaporation was found to be to the transpiration as 6 to 1; and as the transudation in this animal is very copious, we may infer that in man the proportional quantity of the evaporation is still more considerable[3]. With respect to the comparative action of the skin and the lungs, it is supposed that what is lost by the lungs must be entirely due to evaporation, as nothing can be removed from them except what is carried off in the state of vapour, mixed with, or dissolved in the air of expiration, so that strictly speaking we have no pulmonary transudation. On this account we may presume that the loss by the skin will be greater than that by the lungs, although it must be expected that the former will be much more variable[4].

The distinction upon which Dr. Edwards so much insists between evaporation and transudation, although one of great importance, had been but little attended to by preceding physiologists. But on some occasions, I conceive it has been carried by him too far, as where it is maintained that the lungs can transpire only by means of evaporation and not by transudation, and that, in this respect, their action differs from that of the surface of the body. I should suppose, on the contrary, that in both cases, the matter to be transpired must leave the mouths of the vessels in the fluid form, and that the fluid is subsequently evaporated by means of the air, so that, except in degree, I do not perceive any essential difference between the operations.

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[1] El. Phys. xii. 2. 9.

[2] De l'Influence &c. § 6. p. 331 et seq.

[3] Ibid. p. 334 et seq.

[4] See the remarks of Adelon, Art. "Transpiration," Dict. de Méd. t. xx. p. 470 et seq.; also of Collard de Martigni, in Magendie's Journ. t. x. *p. 162 et* seq.

There is another function that is nearly allied to the one which we have been now considering, and which has been made the subject of experiment by some of the modern physiologists; the chemical action which the skin has been supposed to exercise upon the air contiguous to it. Shortly after the discovery of the production of carbonic acid by the lungs, an inquiry was instituted whether the same kind of change was effected at the surface of the body; a circumstance which seemed in itself not improbable, the only difference between the relative situation of the air and the blood, in the lungs and in the skin, being the different thickness of the membrane by which they are separated. The first experiments on this subject appear to have been those of Millet, who, while the body was immersed in the warm-bath, observed a number of minute air-bubbles attached to it, some of which he collected, and upon their being examined by Lavoisier, they were found to consist of carbonic acid [1]. Mr. Cruikshank and Mr. Abernethy afterwards analyzed the air in which the hand or foot had been confined for a certain length of time, and detected in it a considerable quantity of carbonic acid, thus obviating an objection that has been urged against the experiments of Millet and some others of a similar kind [2], that the carbonic acid proceeded, not from the skin but from the water in which it had been dissolved, and from which it was mechanically separated merely by the presence of a solid substance to which it could attach itself; and accordingly when the experiment was repeated by Priestley [3], and due care taken to prevent the adhesion of the small air-bubbles to the body, the production of carbonic acid did not take place [4].

A set of very elaborate experiments were performed by Jurine on the effect which the skin produces upon the air, the results of which appeared to prove very decidedly, that oxygen is consumed and carbonic acid generated in the same manner as in the lungs. He not only established the general fact, but he examined the quantity of effect which is produced under the various circumstances to which the body is exposed, either as influenced by external agents, or as connected with the different states of the constitution; and it seemed to be proved that the amount of carbonic acid was in exact proportion to the activity of the circulation, and the other functions dependent upon it[1]. Jurine's experiments were of so simple and direct a nature, that it is not very easy to conceive how he could have fallen into any material error either in their execution, or in the inference which he deduced from them; yet we have, on the other hand, the accounts of other physiologists, who obtained totally opposite results. Priestley was never able to detect the smallest portion of carbonic acid in air that had been kept in contact with the skin[2]; the same was the case with Dr. Klapp[3], and Dr. Gordon[4], both of whom seem to have conducted the process with every requisite attention to accuracy, but could never perceive the least effect to be produced by it. Dr. Ellis, however, informs us, that he was present when the experiment was made by Dr. M'Kenzie, and reports that, in this case, there was an evident production of carbonic acid[5].

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[1] Mém. Acad. Scien. pour 1777, p. 221, 360.

[2] Cruikshank on Insens. Persp. p. 81, 2; Ingenhousz, sur les Veget. sect. 28. t. i. p. 131 et seq.; Abernethy's Essays, part 2. p. 115 et seq.

[3] On Air, v. ii. p. 193, 4.

[4] We have a series of experiments on this subject by Trousset, which seem to have been performed with accuracy, accompanied with remarks on the opinions of his predecessors; his conclusion was that oxygen is consumed and nitrogen only left, without the production of carbonic acid; Ann. Chem. t. xlv. p. 73 et seq.; Nicholson's Journ. v. v. p. 50 et seq. It appears, however, that this result has not been confirmed, and it does not accord with any hypothesis that we can form upon the subject; if the skin possesses the power of attracting oxygen, it is highly probable that it will produce carbonic acid.

So far, therefore, as the result of experiment is concerned, it appears difficult to decide upon the question respecting the chemical action of the skin on the air in the human subject, the authorities on each side being, as we find, so nearly balanced. But in the cold-blooded animals, where from various causes, it is more easy to perform the experiment in an unexceptionable manner, it is admitted by every one that the skin possesses the power of acting upon the air. In many of the lower tribes the lungs are entirely wanting, yet they consume oxygen and generate carbonic acid like the more perfect animals, and in the oviparous quadrupeds, who are furnished with lungs, it appears that the effect produced upon the air by the external surface of the body, is nearly equal to that of the pulmonary cavities[6]. These considerations have been supposed, by some physiologists, to afford a proof of the existence of the same action in the higher orders of animals, but when so much difference of structure exists, the analogy seems to be of very doubtful application.

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[1] Mém. Med. Soc. t. x. p. 53..72, and Encyc. Méth. "Médecine," t. i. p. 510..515; this volume was published in 1787.

[2] On Air, v. v. p. 100..7. (1st ser.) v. ii. p. 192..9.

[3] Ellis's Inquiry, p. 189, 0. and 354, 5.

[4] Ibid. p. 355, 6.

[5] Inquiry, p. 358. Dr. Ellis, after stating the facts that have been brought forwards on each side of the question, endeavours to reconcile the apparent contradiction of evidence, by supposing that oxygen is not absorbed by the skin, nor carbonic acid discharged from it, but that carbon is excreted by the exhalents, which unites with the oxygen of the contiguous air, thus extending to the skin the same action which he conceives to take place in the lungs; Inquiry, p. 358. But it may be remarked upon this hypothesis, that whatever may be the nature of the action of the skin upon the air, the result would be the same as to the change upon the air; and that we are equally unable to explain why Jurine obtained carbonic acid, while Priestley and others could not procure it. Collard de Martigni, in the paper referred to above, states, as the result of his own experiments, that there is an exhalation of gas from the skin, and that it consists of a variable mixture of carbonic acid and azote, p. 164, 5.

[6] Spallanzani, Mém. p. 150 et seq.; Rapports, t. i. passim; Ellis's Inq. § 142. p. 179; § 662. p. 353 et alibi; Edwards, De l'Influence &c. p. i. ch. 1. § 4, and ch. 4; Mém. d'Arcueil, t. ii. p. 393, 4.

While this chapter was in the press, I had the pleasure of reading the Art. "Cilia," by Dr. Sharpey, in the last number of the Cyc. of Anat. It contains a most interesting and luminous view of the successive discoveries that have been made on this curious subject, and must be regarded as introducing into physiology an agent of great and extensive activity. The ciliary motion has been detected in some of the organs of all classes of animals from the Mammalia to the Infusoria. In the animals which possess what has been termed aquatic respiration, its use obviously appears to be to produce a continual change in the water which is contiguous to the surfaces of the respiratory organs. In the higher classes, where it exists in the lining membrane of the air-passages, its immediate effect must be to convey the secretions of the membrane along its surface, but it does not appear what ultimate effect this can have on the respiration.

I have a farther addition to make to this chapter, with relation to a statement which has been recently brought forward by M. Flourens, respecting the existence of a vascular communication between the mother and the fœtus. This he informs us he has demonstrated by actual injections, which have been passed in both directions; he exhibited his preparations to the Roy. Acad. Sc. at one of the late meetings; the account is given in Ann. Sc. Nat. t. v. 2. ser. p. 65..8.

CHAPTER VIII.

OF ANIMAL TEMPERATURE.

ONE of the most remarkable circumstances which distinguishes the living body from dead or inanimate matter, is the power which it possesses of resisting, to a certain extent, the changes of external temperature, or of maintaining a more or less uniform degree of heat, independent of that of the substances with which it is in contact. As animals are, in almost all instances, immersed in a medium that is colder than themselves, we have much more frequent opportunities of observing their power of generating heat than cold; but we shall find that they are capable of producing both these effects, and although it will probably appear that they depend upon essentially different operations, yet being intimately connected with each other, it will be convenient to consider them in the same part of the work.

After noticing the general fact of the power which animals possess of regulating their temperature, it is necessary to observe that the different species differ very materially in the degree in which they possess this power; those that have the greatest number of organized parts, and whose functions are, in other respects, the most perfect and varied, being able to resist the changes of external temperature much more effectually than the lower classes. Hence arises the great division of animals into warm and cold-blooded, the first including the human species, the mammiferous quadrupeds, and birds; the second, the oviparous quadrupeds, fishes, and, with a few exceptions, all the invertebrated animals. The first of these divisions, the warm-blooded, under ordinary circumstances, retain the temperature of their internal parts at a certain standard, which is nearly uniform for each of the three classes. The temperature of birds is the highest, being about 107° or 108°, that of the viviparous quadrupeds is about 100° or 101°, while the human temperature is a little lower, being 97° or 98° [1]. In the cold-blooded animals the temperature is much less uniform, and indeed nearly follows that of the medium, whether air or water, in which they are

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[1] We shall find the statements that are made by different authors of the temperature of warm-blooded animals not to be entirely uniform. This may depend, in some measure, upon the inaccuracy of the instruments, or upon the mode in which they were applied, but we shall also find that the temperature itself is liable to be affected by circumstances of which we were formerly not aware. Martine, who on all subjects respecting temperature may be regarded as one of the most correct of the earlier writers, has given us a number of observations made by himself and others, from which he deduces

the human temperature to be "about 97° or 98°;" the heat of the warm-blooded quadrupeds and the cetaceæ, he fixes at about 4° or 5° above that of man, and the heat of birds about 4° or 5° still higher; Essays, No. 4, Art. 4. p. 143 et seq. Martine's estimate of the human temperature seems to be sanctioned by the most correct of the modern physiologists; it is also generally admitted that the temperature of birds is some degrees higher, although, perhaps, the difference may be less than he imagined. Hunter estimates their temperature at 3° or 4° above the mammalia; Phil. Trans. for 1778, p. 23..5. Richerand says, that their temperature is from 8° to 10° above that of man; Physiol. § 79. p. 215. Blumenbach estimates the human temperature at 96°, and that of birds, he remarks, is considerably higher; Physiol. p. 96; Reeve, that it is from 3° to 6° higher than that of quadrupeds; On Torpidity, sect. 3. p. 61; Dr. Thomson, that it is 103° or 104°; Chemistry, v. iv. p. 630; Dr. Edwards, in one part of his work, p. 136, remarks that their temperature is from 2° to 3° (cent.), and in another passage, p. 158, from 3° to 4° (cent.), greater than that of the mammalia. Mr. Owen informs us that their "ordinary temperature is 103° and 104°, and according to Camper is occasionally as high as 107° Fahr.;" Cyc. of Anat. v. i. p. 265. According to Despretz the temperature of pigeons is 109°, that of man varying from 93° to 99°, and that of dogs being 103°; Edin. Journ. Sc. v. iv. p. 185. See also the remarks of Bourdon; Physiol. p. 900. We meet with a curious observation in Boyle, which, if correct, may bear upon this point, that birds, with the exception of water-fowls, are more easily drowned than other animals; Works, v. iii. p. 368. Becquerel and Breschet, after remarking upon the inadequacy of the common thermometer to ascertain minute degrees of animal temperature, employed for this purpose the thermo-elective apparatus. On applying it to the human muscles, they found the temperature to be 98°, and that of the cellular substance to be 3° lower. The muscles of a dog were between 2 and 3 degrees higher than the human. A carp was 1° higher than the water in which it was immersed. Muscular contraction increases the temperature, while compression of the artery diminishes it; Ann. Chim. t. lix. p. 113 et seq.; Ann. Sc. Nat. t. iii. (2d Ser.) p. 257 et seq. and t. iv. p. 243 et seq. In this latter paper it is stated that the febrile state increases the temperature of the muscles as much as 3° cent. The authors observe that the actual temperature of a paralytic muscle is not lower than the same part in its healthy state.

It is uncertain how far the peculiar structure which is connected with the pulmonary cavities in birds is related to their higher temperature; Hunter, in his essay on the "Air-cells in Birds," judiciously remarks, "I can hardly think that any air which gets beyond the vesiculated lungs themselves is capable of affecting the blood of the animal, as the other cavities into which it enters, whether of the soft parts or of the bones, appear to be very little vascular;" Observ. on the Anim. Œcon. p. 97. This consideration would lead to the idea, that the use of these cells is rather mechanical than chemical, and is in some way connected with the act of flying. Hunter indeed supposes it to be an objection to this opinion, that those cells are equally found in birds that are incapable of flight; see his paper in Phil. Trans. for 1774, p. 205 et seq.; but an objection of this kind might be urged with respect to almost every organ and function with which the body is provided. See the remarks of Mr. Owen; Cyc. of Anat. v. i. p. 344.

With respect to the extreme variations of the human temperature, Dumas informs us that it ranges from 87° to 108°; he fixes the habitual degree at 95° or 96°, Physiol. c. 6. t. iii. p. 126. Magendie notices the general fact of the variation to which the human temperature is subject, as depending upon constitution, temperament, &c.; he agrees with Dr. Edwards in recommend-immersed, being generally one or two degrees above it, in the ordinary state of the atmosphere [1]. The topics which will more

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ing the arm-pit as the most proper situation for applying the thermometer; Physiol. t. ii. p. 403. Dr. Edwards, as well as M. Magendie, observes that there is a little difference between the temperature of different individuals; he informs us that he examined 20 persons, and found it to vary from nearly 95° to 98½°, (35·5 to 37 cent.) the mean of which will be about 97° (36·12 cent.). This refers to the adult, for in infancy the temperature is decidedly less, being upon the average about 94½° (34·75 cent.). Dr. Holland, however, makes the contrary statement, and informs us that the mean temperature of 40 infants, exceeded that of the same number of adults by 1¾°, and 12 children possessed a temperature of 100° to 103½°. See also the observations of Despretz, in Edin. Jour. of Scien. v. iv. p. 185. He also informs us that it varies according to the season of the year, gradually increasing during the spring and declining again during the autumn; he found the difference in birds to be nearly 4° cent.; from 105° to nearly 111° (from 40·8 to 43·77 cent.), p. 489. Douville states that the temperature of the negro is upon an average 2° R. higher than that of the European; Jameson's Journ. No. 27. p. 181. We have a series of observations by Reynaud, on the differences of the human temperature, as affected by age, temperament, race, or climate; Ann. Sc. Nat. t. xx. p. 43 et seq.; he conceives that it depends more on specific peculiarities than on any general principles. It is not very easy to ascertain what is the greatest height to which the human temperature is occasionally raised in fevers or other morbid conditions of the body; as until lately the observations have been seldom made with much accuracy. It would appear from various notices in Currie's Medical Reports, that in the most acute fevers the temperature seldom or ever exceeds 107°. See also Edin. Med. Journ. v. xxii. p. 363. Dr. Edwards relates an observation of M. Prevost's, where a patient in tetanus had the heat elevated 7° cent. above the ordinary standard; p. 490. We have a singular statement made by Magendie, of the high temperature which the blood occasionally acquires in local inflammation, but the authority is not quoted; Physiol. t. ii. p. 400. Hunter in his work on the Anim. Œcon. note to p. 113, observes, "It is found from experiments, that the heat of an inflamed part is nearly the greatest or standard heat of the animals, it appearing to be a part of the process of inflammation to raise the heat up to the standard;" but there is probably some inaccuracy either in the observation or the expression. In his work on the blood he gives the result of a number of observations which he had made upon the temperature of an inflamed part; the greatest heat that is recorded is 104°, p. 296: this accords with the observations of Becquerel and Breschet. He remarks that the actual temperature in inflammation is not so much increased as the sensation would seem to indicate. Dr. Granville has lately communicated some curious facts respecting the temperature of the uterine system during parturition, from which it appears that it occasionally rises to 120°, the elevation appearing to bear a proportion to the degree of action in the organ; Phil. Trans. for 1825, p. 262..4. In a series of observations, which have been lately made by Donné, "on the relations between the state of the pulse, respiration, and temperature of the body in diseases," the greatest heat which was observed was 104°; Br. and For. Med. Rev. v. ii. p. 246..9.

[1] For the temperature of the cold-blooded animals it may be sufficient to refer to Martine, p. 241 et seq.; Hunter on the Anim. Œcon. p. 116, 9; also on the Blood, p. 298 et seq.; Spallanzani, Mém. p. 255 et seq.; and Ellis, p. 215 et seq. Hunter observes that the warm and cold-blooded animals might be more properly termed animals whose heat is permanent or variable, according to that of the atmosphere; On the Blood, note in p. 15; also Phil. Trans. for 1778, p. 26..8; where he relates a number of experiments on the degree in which the temperature of the cold-blooded

particularly require our examination on this subject are, 1. What is the efficient cause or source of animal heat; 2. By what means is its uniformity preserved; 3. How is the body cooled at high temperatures; and lastly, what is the connexion between the function of calorification and the vital powers of the system.

SECT. 1. *Of the efficient Cause of Animal Heat.*

The body is surrounded by an atmosphere, which is frequently 40, 50, or even a greater number of degrees colder than itself, so that heat must be rapidly abstracted from it, yet it possesses the power of continually supplying the loss thus occasioned. This faculty appeared to the ancients so far beyond the reach of all physical agency, that they did not even attempt to assign any cause for it, but regarded it as an innate quality of the body, or something essentially connected with life, so that in speculating upon the subject of animal temperature, they thought it necessary to direct their inquiries rather to the modes by which the heat of the body might be reduced to the proper standard, than be maintained above that of the surrounding medium. Without entering into any minute detail of opinions, which can be interesting only from their antiquity, it may be sufficient to remark that Galen and the ancients generally conceived animal heat to be an innate or primary quality of the body, and that it is contemporary with life[1]. Its origin or focus was supposed to be in the heart, from which, by means of the blood, it was distributed to all parts of the system. A principal office of the circulation was therefore sup-

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animals is affected by the medium in which they are immersed. Broussonet remarks that the temperature of fish is from ¾ to ⅛ a degree (R.) higher than that of the medium in which they are immersed; Mém. Acad. pour 1785, p. 191, and Despretz found the temperature of a carp, immersed in water at 51·4, to be 53°; Edin. Jour. of Scien. v. iv. p. 185. We have a number of curious observations on the temperature which fish are occasionally found to bear, in Dr. Hodgkin's trans. of Edwards, p. 465..7. Becquerel and Breschet, in the paper referred to above, state the temperature of the carp to be one degree higher than the water; Ann. Chim. t. lix. p. 130, and Ann. Sc. Nat. t. iii. p. 269. Dr. Davy has stated the remarkable circumstance, that he found the temperature of the bonito to be 99°, that of the water being 80·5°. And he farther informs us, on the report of the fishermen in the Mediterranean, that the tunny is likewise warm-blooded. He remarks that the gills and respiratory organs in these animals are provided with numerous nerves, and that there is a considerable quantity of red blood in these parts; Jameson's Journ. v. xix. p. 325. So very remarkable a deviation from the œconomy of all the animals, which are of the same anatomical and physiological structure, renders it desirable that the observations should be carefully repeated, and all the concomitant circumstances accurately noted. It does not appear that this anomaly has been observed in the other species of the same family, as for example in the mackarel.

[1] For the opinions of the ancients see Boerhaave, Prælect. § 169, 202. cum notis; Haller, El. Phys. vi. 3. 8; we have an account of the doctrine maintained by Hippocrates on this subject in his Treatise de Corde, Op. t. i. p. 269; also in Le Clerc, *Hist. de la Médecine*, p. 125.

posed to be the conveyance of this heat to the various organs, while one great use of the respiration was to cool the blood, or to prevent its heat from exceeding the degree which was consistent with the well being of the animal. After the revival of letters, when the causes of the phenomena of the living body began to form an object of investigation, physiologists still very generally regarded the blood as the source of animal heat, and according to the peculiar theory which they had adopted respecting the operations of the system, they explained the production of heat either upon chemical or mechanical principles.

The chemists[1] ascribed it to fermentation in the blood which took place in the heart, while the mechanicians accounted for it by the friction of the particles of the blood against the sides of the vessels, and consequently, supposed that it was produced during the course of the circulation[2].

Although we meet with occasional expressions, which may appear to indicate a more correct opinion, yet perhaps the first clear intimation of a regular hypothesis to account for the generation of animal heat, upon what we should now consider as a legitimate mode of reasoning, is to be met with in the writings of Mayow. After he had made his interesting discoveries on the constitution of the atmosphere, and the share which it has in the extrication of heat in combustion, he was led to extend the analogy to animal heat, and concluded, contrary to the opinion almost universally embraced at that time[1], that the use of the lungs is not to cool the heart, but to generate heat, an effect which is brought about by the absorption of the nitro-aereal spirit of the air. This, mixing with the sulphureous particles of the blood, excites a species of fermentation by which heat is produced, in a way precisely similar to that in which heat is excited by the combustion of inflammable matter generally[2]. Mayow therefore explicitly advanced the two positions, that the effect of respiration is not to cool the blood, but to generate heat, and that it does this by an operation in every respect analogous to combustion. His hypothesis was however imperfect in consequence of the erroneous opinions which he entertained respecting the nature of combustion generally, or the mode by which combustion generates heat, as well as respecting the nature of the combustible matter which is discharged from the blood. Perhaps, however, this latter should be regarded as a verbal, rather than as a real inaccuracy, the word sulphureous being a general term applied to any kind of inflammable matter, not as is now the case, confined to one species. Mayow's doctrine respecting the connexion between respiration and animal heat does not appear to have made any considerable impression upon the minds of the contemporary physiologists, for we find that the old hypotheses still continued to prevail with little alteration, or only with slight and immaterial modifications, until the middle of the last century. During this period the question was warmly discussed, whether the lungs were the agents for generating heat, or for reducing the temperature of the blood by bringing it into proximity with the cold air, and the sentiments of the most eminent physiologists appear to have been, for the most part, in favour of the latter opinion. And it must be remarked, that even most of those who adopted the former opinion, that the lungs serve to generate heat, supposed that the effect was produced entirely by friction or some other mechanical means[3].

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[1] See Haller, El. Phys. vi. 3. 8. 10, for a summary of these opinions. As a specimen of the opinions and mode of reasoning that were adopted on this subject by the chemical physiologists, I may refer to the Treatise of Willis, "de Accensione Sanguinis," which was probably written in the year 1670. The author supposes that there is a proper combustion in the blood, which, according to his general principles, depends upon the fermentation excited by the combination between different chemical substances. He strenuously maintains the doctrine of the life of the blood, which he conceives to consist in its property of producing heat. In the middle of the last century we had a very learned dissertation by Stevenson; Ed. Med. Essays, v. v. pt. 2. p. 806 et seq. He observes that four opinions were then prevalent: 1. That animal heat depends upon attrition between the arteries and the blood; 2. That the lungs are the fountain of this heat; 3. That the attrition of the parts of the solids upon each other produces it; and lastly, the process by which our aliments and juices are constantly undergoing some alteration. With respect to the idea of the heat being derived from the lungs, the author adduces various arguments to prove that the blood is rather cooled than warmed in passing through the pulmonary vessels. This final conclusion is in favour of the last supposition; he remarks concerning it, that heat is frequently excited by the chemical change which results from the mixture of fluids with each other, and that these changes proceed either from fermentation or putrefaction; the case in question he decides to be one of fermentation, or rather something intermediate between the two. He supposes that the process is principally carried on in the veins, and less in the lungs than in the other parts of the body.

[2] For specimens of the mechanical mode of reasoning, see Boerhaave, Aphor. cum notis Sweiten; § 382, 675. Perhaps the last attempt to form a mechanical theory of animal heat (at least in this country) is that of Douglas, published in 1747; he lays down the theorem that, "animal heat is generated by the friction of the globules of blood in the extreme capillaries;" p. 47.

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[1] The following are among the eminent physiologists of that period, who supposed that the effect of respiration is to cool the blood. Sylvius, Disp. Med. cap. 7; Fabricius de Respiratione, lib. 1. cap. 6; Bartholin, Anat. p. 430; Harvey de Motu Cordis, Ex. 2. p. 194, 5. and Ex. 3. p. 232; Swammerdam, de Respiratione, sect. 1. c. 1. § 9. and c. 3. § 4; Descartes supposes that the fermentation of the blood in the heart produces heat, which is carried from this organ over the body; De Homine, p. 197. Boyle remarks that "divers of the new philosophers, Cartesians, and others, think the chief, if not the sole use of respiration, to be the cooling and tempering of the heat in the heart and blood, which otherwise would be immoderate;" Works, v. i. p. 103. Haller discusses the question in El. Phys. viii. 5, 6. This doctrine has been lately maintained by Collard de Martigni; Magendie's Journ. t. x. p. 136.

[2] Tract. p. 151 et seq.; 296, 7.

[3] See Boerhaave, Prelect. § 202. 220, cum notis; Boerhaave, Aphor. 382 cum Comment. Sweiten; Haller, Prim. Lin. § 303; Martine and Stevenson ubi supra; Cullen displays his accustomed caution in examining into the cause of animal heat, Physiol. § 262; he perceived the connexion which

It was not until after Black had completed his valuable train of experiments on fixed air, that more correct ideas began to prevail respecting the cause of animal heat. After he had discovered that the same kind of aeriform fluid which is produced by the burning of fuel is expired from the lungs, a strict connexion seemed to be established between the processes of combustion and respiration. He was hence led to infer, as Mayow had previously done, that respiration is a species of combustion, and that the heat thus extricated is employed in preserving the temperature of the animal body above that of the surrounding medium. But Black's explanation of the cause of animal heat, although extremely ingenious, and founded upon what appeared to be correct principles, was liable to an objection which prevented it from being generally received. It was said, that if the lungs are the seat of the supposed combustion, or the focus whence the heat radiates to all parts of the body, their temperature must be much superior to that of the other organs, and in short, that they would be altogether incapable of supporting the degree of heat, to which they would be necessarily exposed. We do not find that Black made any attempt to repel the objection, and we may presume that he conceived it so formidable, as to have induced him altogether to relinquish the hypothesis[1].

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there seemed to be between the lungs and the production of heat, but he could not account for the method in which they act in producing it. Haller, as usual, gives us an account of the opinions that had been entertained on both sides of the question, and inclines to the negative; El. Phys. vi. 3. 13 sub finem. Perhaps Morozzo is the latest author of any considerable respectability who defends the opinion that the effect of respiration is to cool the blood; Jour. Phys. t. xxv. p. 120.

[1] Although there appears to be no doubt that Black applied his discovery of the formation of carbonic acid in respiration to explain the production of animal heat, we do not find any reference to it in his lectures, as they were afterwards published by Robison. We may, however, conclude that he announced the hypothesis in his lectures, and that it became generally known through this medium; see the Remarks of Menzies on Respiration, p. 35; Murray, Chem. v. iv. p. 571; Thomson, Chem. v. iv. p. 630; and Ellis, Inquiry, p. 234; Dr. Ellis says, that the objection referred to in the text was urged by Cullen; also Robison in his Preface, p. 53. Probably Cullen, in the following passage in his "Institutions," § 268, may refer to Black's hypothesis. "We take no notice of the suppositions which have been made of the generating powers being confined to certain small portions of the system only. These suppositions give no relief in the general theory: and they are not supported by any particular evidence. The breathing animals are the warmest; but that they are warmer because they breathe, is not more probable, than that they breathe because they are warmer." Leslie's Treatise on Animal Heat contains an account of the popular objections to Black's doctrine; his own hypothesis is that phlogiston is naturally developed by the vessels and generates heat. Dr. Black, with the modesty and scrupulous regard to the literary rights of his contemporaries, which formed so distinguished a trait in his character, when speaking of Crawford's hypothesis of the different capacities of bodies, says, that he applied it, "to explain the heat maintained in the bodies of animals," without any allusion to his own opinions; Lect. v. 2, p. 205, 6. It was about this period of the investigation that Franklin threw out a conjecture on the source of animal heat, that the

Lavoisier now entered upon his researches into the nature of combustion and other analogous operations, and after repeating and verifying many of the experiments of Black and Priestley, he pointed out more clearly than they had done, the exact nature of the change which is produced upon the air by the action of the lungs. He found a perfect similarity between the results which he obtained from the combustion of charcoal and the products of respiration, and as the evolution of heat is the necessary consequence of the former operation, he was induced to draw the same inference that had been previously made by Black, that heat must be generated by the formation of carbonic acid in the lungs. He does not appear, at least in the first instance, to have felt the objection that had been urged against the doctrine, but brings it forwards as a correct deduction from acknowledged facts, and as a satisfactory method of explaining the phenomena[1].

It was shortly after this period that Crawford commenced his investigations, and illustrated the subject by a series of very elaborate and ingenious experiments; the result of which was

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matter of heat was taken into the body along with the food, and was set at liberty during the successive changes which the aliment afterwards experiences; Works, v. ii. p. 79, 125. This hypothesis was subsequently taken up by Rigby and dilated into a Treatise; see sect. 1. of his Work on Animal Heat. Richerand, to a certain extent, adopts the same view of the subject, but his remarks are vague and diffuse; Physiol. § 79. p. 214. I may remark, that an opinion something similar to this had been previously formed by Descartes; he says that the change which the food undergoes in the stomach produces heat, in the same manner as when water is poured upon lime, or aqua fortis upon metals; De Homine, p. 7. Hunter also considers it probable that the principal source of heat is in the stomach; On the Blood, p. 292; the remark is, however, made in an incidental manner, and he had expressed himself in another passage in the same work dissatisfied with all the theories of animal heat, as not according with the facts; p. 15.

[1] Mém. Acad. Sc. pour 1777, p. 599. In tracing the progress of discovery on this subject, it appears somewhat difficult to ascertain the share which Black and Lavoisier respectively bore in establishing the chemical theory of animal heat. Lavoisier, in the above memoir, speaks of the hypothesis as entirely original, and altogether derived from his own experiments, without referring to any preceding authors, and the same statement is made by Seguin; Ann. Chim. t. v. p. 259; see also Fourcroy, Méd. Eclair, t. i. p. 56..61. There seems, however, to be no doubt, that as far as respects the production of carbonic acid in the lungs, and the consequent evolution of heat, he had been completely anticipated by Black, while he does not advert to the objection which had been urged against the hypothesis. Although we admit that Robison's zeal for the reputation of his deceased friend may have carried him too far in his observations upon the rival claims of the two philosophers, it seems but too probable that Lavoisier was not always sufficiently correct in appropriating the due share of merit to his contemporaries, a circumstance the more to be lamented, when we consider the very great obligations which he conferred upon science. There are some good observations on this subject in Nicholson's Journ. v. xiv. p. 90, 231 et seq. I may remark that Crawford, who was distinguished for his candour and liberality, does not refer to Black, as having advanced any hypothesis of animal heat, nor does he intimate that his own opinions had been anticipated by Lavoisier.

the formation of a theory of animal heat, which appeared to account for all the phenomena, and to be founded upon decisive and well-established facts, while it was not liable to the objection which had been urged against Black's hypothesis. The scrupulous care with which Crawford endeavoured to avoid every source of inaccuracy, the ingenuousness with which he acknowledged the errors of his first experiments, and the spirit of candour which pervades every part of his work, were calculated to produce a very favourable impression with respect both to the intellectual and the moral qualities of the author, so that his doctrine was very favorably received, and generally embraced by his contemporaries.

Crawford's theory of animal heat may be regarded as founded upon the three following positions: 1. That the air, when taken into the lungs, undergoes the same change as by the combustion of a carbonaceous body, and that consequently, in the same manner as in the combustion of carbon, heat is generated. 2. The same process by which the oxygen of the inspired air is converted into carbonic acid likewise converts the venous into arterial blood, but arterial possessing a greater capacity for heat than venous blood, the heat which would otherwise raise the capacity of the arterialized blood, is employed in saturating its increased capacity, and in maintaining its temperature at the same degree with the venous. 3. The heat is not therefore actually set at liberty in the lungs, although the arterial contains a greater quantity of absolute heat than the venous blood, but it is during the course of the circulation, when the arterial blood is again venalized, and consequently loses its increased capacity, that the heat becomes sensible and supports the temperature of the system. The hypothesis which Crawford constructed upon the above positions professed to be the result of direct experiment in all its parts; it removed the objection that had been urged against Black's doctrine, and afforded one of the most interesting and beautiful specimens of the application of physical and chemical reasoning to the animal œconomy that had been ever presented to the world[1].

The first of the above positions, the different capacities for heat of the air before and after it had undergone the change which it experiences in combustion or in respiration, constituted, as is well known, the main point to which Crawford directed his elaborate train of experiments, and forms the basis of his theory of inflammation generally, of which animal heat composes only one example. With respect to the second position, the different capacities of arterial and venous blood, Crawford made this the subject of a long and careful examination, and although there are many parts in it of very delicate execution, and which required a minute attention to various circumstances that might interfere with the results, yet he appears to have been so well aware of the sources of error, and to have so carefully guarded against them, that, I think, a perusal of the experiments can scarcely fail to impress the mind with a conviction of their general truth. The conclusion which Crawford deduced was, that the specific heat of arterial, was greater than that of venous blood in an average proportion of 114·5 to 100. If we admit the fact, the conclusion is obvious and most important. It follows that when the blood is converted from the venous to the arterial state, it renders latent a part of the heat which would otherwise have been liberated by the union of the oxygen and the carbon, and would have raised the temperature of the blood in the pulmonary vessels. The heat therefore appears to be employed in three ways; a part of it in counteracting the effect of the cold air which is taken into the lungs, another portion in producing the vapour that is expired, while the remainder supplies the arterial blood with what is requisite, in consequence of its increased capacity, to support its temperature at the same degree with that of the venous blood.

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[1] We have a correct and judicious summary of Crawford's theory given us in Prof. De la Rive's elegant inaugural dissertation "De Calori Animali," *p. 26 et seq.*; and in Dr. Henry's Elements, v. ii. p. 407.

It would appear that these operations are so nicely adjusted to each other, that the temperature of the blood in the different parts of the body is kept nearly at a uniform standard, and especially, that while this fluid passes through the lungs, and in conjunction with the air, undergoes that change which serves as the actual origin of the heat of the animal, its temperature is little, if at all affected, the heat which would otherwise be liberated being the whole of it employed in the three ways mentioned above. We have here a remarkable example of that nice adaptation of the different operations of the living body to each other, which forms so distinguishing a feature of the animal œconomy, and in which it so infinitely surpasses any contrivances of a merely mechanical or physical nature. Admitting the truth of the theory it will follow, that the more carbon is separated from the blood and united to oxygen, the more heat will be disengaged, the more completely therefore will the blood be arterialized, and consequently the more will its capacity be increased, and the greater quantity of heat will it require to maintain its temperature. According to Crawford's doctrine the blood is not warmed in passing through the lungs, although this is the organ in which it acquires its heat, but it is in the capillary part of the systemic circulation, when the blood again becomes venalized, that the heat is liberated. This change therefore takes place in that part of the body where it is the most necessary to counteract the effect of the surrounding medium, which is, in most cases, colder than the body itself, and where by its diffusion over a great extent of surface, it must tend to make the heat nearly uniform through the whole of the system.

After the publication of Crawford's theory, Lavoisier, in a subsequent memoir, recurred to the subject of animal heat.

He still maintained his former opinion, that it is derived from the union of carbon and oxygen in the lungs, and that respiration is, in every respect, analogous to the process of combustion. He assumes that all parts of the body are preserved at the same temperature, and with respect to the manner in which the heat is equalized, he observes that it depends upon the three following causes: 1. Upon the rapidity of the circulation of the blood, by which the heat that is acquired in the lungs is quickly transmitted to all parts of the body; 2. Upon the evaporation which takes place from these organs, which carries off a part of their heat; and 3. From the increased capacity for heat, which blood acquires when it is converted from the venous to the arterial state. It will be observed that this view of the subject, so far as animal temperature alone is concerned, is strictly conformable to the doctrine of Crawford[1]. Lavoisier, however, as is too frequently the case, does not introduce the name of Crawford, or even allude to his experiments on the different capacities of arterial and venous blood, and the same silence is observed on this point, when, in another part of this paper, he compares his own hypothesis with that of Crawford respecting the heat generated by combustion, and notices the experiments on the different capacities of oxygen and carbonic acid[2]. Lavoisier seems to have maintained the same opinion respecting animal heat in his future researches; he always speaks of it as a case of combustion, and supposes that the heat is generated upon the same principle, as in the formation of carbonic acid by the more rapid union of oxygen and carbon.

Although Crawford's theory so admirably accounted for all the phenomena, and appeared to be so strictly deduced from facts and experiments, yet the extreme delicacy of the operations on which it rested, as well as the interest excited by its great importance, led to a rigid examination of all its fundamental positions; the consequence of which was that they have all of them been controverted, and we have an account of a number of experiments, the results of which are reported as being directly opposite to those of Crawford, or at least, as not warranting his conclusions. The main points on which the theory is supported are the following; that heat is extricated when oxygen is converted into carbonic acid, in consequence of the diminished capacity of the latter, that the capacity of arterial is greater than of venous blood, and that the blood in the two sides of the heart, and in the large trunks of the pulmonic system, possesses the same temperature. With respect to the first of these points, the different capacities of oxygen and carbonic acid, it must be considered as a question which affects the theory of combustion generally, and in which the cause of animal temperature is not particularly concerned. When carbon is united to oxygen so as to produce carbonic acid, we find that heat is liberated, and this takes place not merely in that rapid union of the bodies, which may be considered essential to a proper combustion, but in the slow combination of them, such as occurs in fermentation, putrefaction, and germination[1]. In whatever way therefore we account for the liberation of heat in these processes, we may employ the same method to explain the heat generated by respiration. We may even go farther, and say, that as heat is always liberated when this union of oxygen and carbon takes place, the same effects must necessarily follow from the production of carbonic acid in respiration. The difficulty therefore is not to account for the heat produced by respiration, but to remove the objection which was originally urged against Black's hypothesis, that the temperature of the blood in the lungs ought to be greater that that in the other parts of the body. It is in fact with the other two points only, the comparative temperature of the blood in the trunks of the pulmonic vessels or in the two sides of the heart, and the different capacity of arterial and venous blood, that the theory of animal heat is concerned, but on both of them experiments have been adduced, which are in direct opposition to the doctrine of Crawford.

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[1] Mém. Acad. Sc. pour 1780, p. 406. We may remark, however, that Lavoisier, in one of his subsequent papers, speaks of the combustion as taking place in the lungs, although he adds, " and perhaps in other parts of the system;" Mém. Acad. Sc. pour 1790, p. 601.

[2] Mém. Acad. Sc. pour 1780, p. 394.

It seems not a little remarkable that there should have been any difficulty in ascertaining the first of these points, which we might have supposed would have been settled by a few simple and easy observations. This, however, appears not to be the case, and it still remains undecided; although the weight of authority inclines to the opinion, that arterialized blood, at least in the heart and great trunks, is a degree or two warmer than the venous; so that upon the whole it appears probable that the blood does not possess that uniformity of temperature which has been generally assigned to it, but that it differs a little, not only in different individuals of the same species, but also in the different internal organs of the same individuals[2]. The latest experiments which have been made on the comparative capacity of arterial and venous blood are likewise unfavourable to the theory of Crawford; for although we are led to conclude from them, especially from those of Dr. Davy, that the specific heat of arterial is greater than that of venous blood, the excess appears to be so small as to be scarcely sufficient to account for the effects which are attributed to it[1].

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[1] In the malting of barley, which is a case of germination, we are informed by Dr. Thomson, that the temperature of the grain is raised as much as 10°; Chem. v. iv. p. 374.

[2] In estimating the temperature of the blood in the internal parts of the body generally, it was formerly stated that when they are so far below the surface, as to be beyond the reach of the influence of the atmosphere, the heat is uniform; yet the remark must be taken with limitations of which most of the modern physiologists appear to be well aware. Boerhaave observes that the arterial blood is warmer than the venous, although it is exposed to the cold air which is taken in by the lungs; Prælect. not. ad. § 202. The general fact of the different internal parts of the body possessing different temperatures, according to their vicinity to the heart, was established by Hunter, although there may be some reason to doubt whether his experiments were, in all cases, perfectly accurate; Observations on the Animal Œconomy, p. 107 et seq. We have also various observations to the same effect made by Sir A. Carlisle; Phil. Trans. for 1805, p. 15, 22. Many

These experiments are, however, to be considered principally as relating to the theory of Crawford, so far as it depends upon the doctrine of the different capacities of arterial and venous blood. The opinion that animal heat results from the union of oxygen and carbon, as originally advanced by Black, and as subsequently illustrated by Lavoisier, although it may be obnoxious to the objection which has been already stated, is unaffected by the attempts that have been made to disprove the results of Crawford's experiments. We should still conceive that the heat must be produced by the formation of carbonic acid, although we might be unable to explain the exact mode in which it is set at liberty, or the precise point in the circulation where it assumes the form of sensible heat. We accordingly find that the chemical theory, or that which supposes respiration to be a process more or less analogous to combustion, generally prevailed, until the doctrine was attacked in its essen-

eminent physiologists, who have examined the temperature of the blood, since the attention was particularly directed to the point by Crawford's theory, have found the left side of the heart to be warmer than the right. Menzies informs us that this is the fact; Essay, p. 58, 62; Plenk says the arterial blood is warmer than the venous; Hydrol. p. 33; Dr. Davy found the arterial blood 1° warmer than the venous, and he observes that the parts of the body are less warm as they recede from the heart; Phil. Trans. for 1814, p. 595..0; Magendie says that the blood is warmed about 1° (cent.) in passing through the lungs; Physiol. t. ii. p. 397; and the same estimate of the different temperatures of arterial and venous blood is made by Thenard, Chim. t. iii. p. 671. Mr. Coleman, on the other hand, states the temperature of the two sides of the heart externally to be the same, but that the blood in the right ventricle is 1° or 2° warmer than in the left; he informs us, however, that the blood in the left ventricle cooled more slowly, thus indicating that it contained more latent heat; On Suspended Resp. p. 31, 2; 35, 6.

[1] The results of Dr. Davy are not uniform, and although we have every reason to place full confidence in his statements, it must be allowed that the experiments require to be more frequently repeated and varied, before we can draw any decisive conclusion from them; they, however, in three cases out of four, indicated a greater capacity in arterial than in venous blood; in those experiments in which he himself placed the most confidence, the relative numbers are 913 and 903; Phil. Trans. for 1814, p. 591..5. The inference that may be drawn from Mr. Coleman's experiments is in favour of the greater capacity of arterial blood; On Suspended Respir. ut supra. Magendie, in making a comparison between the properties of arterial and venous blood, has referred to the experiments of Dr. Davy on their respective capacities; Physiol. t. ii. p. 288; but it is remarkable that he has quoted that only in which the capacity of the venous was greater than that of the arterial, and which Dr. Davy considers as less to be depended upon than the other three; *Phil. Trans.* for 1814, p. 595.

tial point by a series of experiments that were performed by Sir B. Brodie[1].

Sir B. Brodie took advantage of the process which was described in the last chapter for restoring the action of the heart, after decapitation or the removal of the brain, by artificially inflating the lungs. By this means we are able to produce the usual change in the appearance of the blood from the venous to the arterial state, when we find that oxygen is consumed and carbonic acid disengaged, as in natural respiration. The chemical action of the lungs being thus maintained, as in the state of health, it became a curious subject of inquiry, whether heat was also evolved, and the temperature of the animal supported, in the same manner as during the perfect state of the functions. Upon making the experiment this appeared not to be the case, for it was found that when, by any means, the influence of the brain was effectually cut off from the heart and blood vessels, although by means of artificial respiration the contraction of the heart and the passage of the blood through the lungs is maintained, so that it apparently undergoes its change from the venous to the arterial state, yet the generation of animal heat seems to be suspended, and consequently, if the air which is inspired be colder than the body, the effect of respiration is to cool the animal[2]. Sir B. Brodie's experiments were so simple and appeared so decisive in their results, that the conclusion from them was thought to be irresistible, and notwithstanding the mass of evidence that had been accumulated in favour of the chemical theory of respiration, and the numerous arguments and analogies that had been adduced in its favour, the opinion of many of the most intelligent physiologists was, that respiration, as far as respects the production of carbonic acid, is not the cause of animal heat, and is only remotely connected with it, that the lungs have no immediate or direct concern in the operation, but that it depends upon the nervous influence[3].

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[1] There are various remarks in Hunter's Essay on Animal Heat, which appear inconsistent with the chemical hypothesis. See Observ. on the Anim. Œcon. p. 103 et alibi; but those who are conversant with the writings of this physiologist must be aware, that we are not to deduce his doctrines from particular expressions, but from the general scope and tenour of his arguments. He, however, very explicitly states that the power of producing animal heat does not "depend upon the nervous system; for it is found in animals that have no brain or nerves." p. 103, 4.

[2] Phil. Trans. for 1811, p. 37..48; also Phil. Trans. for 1812, p. 378. Sir B. Brodie found in his second set of experiments, that when the sensibility of an animal is destroyed by a narcotic poison, and the lungs artificially inflated, the power of generating heat is destroyed as effectually as by decapitation, while the power is restored in exact proportion to the return of the sensibility, when the influence of the poison ceased to act upon the system.

[3] Mr. Brande unequivocally gives it as his opinion that the researches of Sir B. Brodie have produced the "complete subversion" of the chemical doctrine of animal heat; Manual, v. iii. p. 226. Dr. Young says, that "animal heat depends jointly on circulation and nervous energy, but proba-

But notwithstanding the value of Sir B. Brodie's experiments, there are two circumstances which seem to have been overlooked or disregarded by him, and which it will be necessary to examine, before we can draw the conclusion which, at first view, appeared so naturally to flow from them. In the first place we must inquire into the degree of cooling effect which is caused by the inflation of the cold air into the lungs, and compare this with the opposite effect which might be produced by the generation of the carbonic acid. In the natural or entire state of the animal, the respiration is regulated partly by the influence of volition, and partly of instinct, the quantity of air inspired being just sufficient to answer the demands of the system; but in the experiment which has been related, the air is forcibly sent into the pulmonary vesicles, without any cooperation from the animal, and, of course, independently of the correspondence between the different functions which are connected with the action of the lungs[1]. Now, as we have already observed, respiration may be considered as a compound process, one essential effect of which is to abstract heat from the body; the question therefore to examine is, whether the heating or cooling effect of respiration will preponderate, under the circumstances in which the animal was placed in these experiments. The second point in which Sir B. Brodie's conclusion is premature, respects the part of the circulation in which the heat is extricated. If, according to the theory of Crawford, it is not in the lungs, but in the capillary vessels, that this operation is

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bly little on respiration," and in the list of authors that he subjoins, he designates Sir B. Brodie's paper in the Phil. Trans. as the most important treatise on the subject; Med. Lit. p. 108. His opinion, however, upon this point seems to be somewhat vacillating, as in a subsequent passage, p. 503, he supposes that the chief use of the red particles of the blood, and "of the process which is carried on in the lungs, is to preserve the temperature of the body," and remarks, that the "process may be explained according to the ingenious and important theory of Dr. Crawford;" but adds, "that the nervous system appears to have an influence over the process, without which it cannot be carried on;" p. 504, 5. Dr. Thomson also conceives that Sir B. Brodie's experiments have entirely destroyed the foundation of Dr. Crawford's theory; Chem. v. iv. p. 632. Mr. Earle, who has brought forwards some very interesting pathological facts in support of the connexion between the nervous action and the evolution of animal heat, seems to consider the chemical theory as overthrown by Sir B. Brodie's experiments; Med. Chir. Tr. v. vii. p. 173 et seq. The treatise of M. Chossat, on the influence of the nervous system on animal heat, consists of a series of experiments, in which certain parts of the nervous system were destroyed or mutilated, and the effects which were produced on the temperature were minutely examined. In some of the experiments the spinal column was divided, in others certain portions of it only, in some the pneumogastric nerves, and in others the great sympathetic. It would appear that, in these cases, a diminution of temperature was produced by these operations, but it would be necessary to take into account various considerations, before we could draw the conclusion, that the nervous system is the direct agent in the production of animal heat.

[1] See the judicious observations of Dr. Philip; Inquiry, p. 205.

carried on, we should scarcely expect to find the temperature of the animal supported by the artificial inflation, because the immediate effect in this case is to render heat latent, which is necessarily a cooling process, while it is by no means surprising that the disengagement of the latent heat, which takes place in the natural situation of the animal, should have been entirely prevented, or very much diminished, where the functions of the system generally, and those of secretion and assimilation in particular, were so much deranged, or even entirely suspended. Perhaps, therefore, it may not be going too far to assert, that in the case in question, where a quantity of cold air is forcibly propelled into the lungs, and a portion of heat necessarily rendered latent, by the conversion of venous into arterial blood, and where we may also presume that the air which is discharged from the lungs, would carry off a quantity of aqueous vapour, and in this way produce a farther abstraction of heat, while, on the other hand, it is not improbable, that the various processes by which the blood is venalized in the natural state of the system, must be impeded or deranged, the result which we ought to expect would be the cooling of the blood, as Sir B. Brodie found it to be in his experiments.

These theoretical considerations might have induced us to pause before we admitted the conclusion, that the chemical change of the air in the lungs is not the source of animal heat; but we have two sets of experiments, one by Dr. Philip, and a second by M. Legallois, which directly oppose those of Sir B. Brodie, by showing, that if the attending circumstances are duly considered, we can clearly trace a connexion between the diminution in the quantity of oxygen consumed and the deficiency in the evolution of heat, when we employ the process of artificial inflation. Dr. Philip conceived that the cause why the temperature of the body diminished more rapidly where this operation was employed, than where the animal was left undisturbed, depended upon too large a quantity of air having been propelled into the lungs, and he accordingly found that when a less quantity of air was used, the cooling process was sensibly retarded by the inflation, so as directly to obviate Sir B. Brodie's objections, and to exemplify the power of the lungs in generating heat[1]. That this power is not directly dependent upon the nervous system appears also to be proved, by an experiment in which the rate of cooling in a dead animal, where the brain and nervous system had been removed, was compared with one which was left in its entire state, in which case, and also when the artificial respiration was employed, no difference could be observed in the temperature of the animals[1].

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[1] Inquiry, c. 10, p. 197 et seq. The precise subjects of inquiry which Dr. Philip proposed in these experiments were: 1. Whether the *nervous power* is capable of evolving heat from the blood, after the *sensorial power* has been destroyed; and 2. Whether, under these circumstances, more heat is evolved by artificially supporting the circulation, than by leaving the animal undisturbed. The animals employed in the experiments were rabbits, and they were killed by a blow on the occiput; a comparison was made both between the effect of *inflating the lungs with more or less rapidity, and inflating the lungs and leaving the animal undisturbed*; see ex. 64, 5, 6. We are informed that in one experiment the temperature was actually raised by nearly 1°; p. 199. 0. Dr. Hastings performed similar experiments with the same results, p. 200. See also Quart. Journ. v. xiv. p. 96, 7. This question has also been inves-

A very elaborate and ingenious train of experiments was performed upon the same subject by Legallois, the results of which were decidedly favourable to the chemical theory, and fundamentally coincided with those of Dr. Philip. The experiments consisted in observing the rate of cooling in animals under different circumstances, and in comparing the effect of the inflation of the lungs upon perfect and upon mutilated animals, and also in noticing the degree in which animal temperature is influenced by various impediments to the respiration or to the action of the other functions[2]. He deduced the following important conclusions from his experiments: 1. That the animals upon which artificial respiration has been employed, although they suffered a reduction of temperature, yet it was less by from 1° to 3° (cent.) than in simply dead animals; 2. That in cooling through a certain number of degrees, they parted with more heat than simply dead animals; 3. That inflation of the lungs of perfect and healthy animals lowers their temperature, and that if the operation be continued for a sufficient length of time, they may even be destroyed by cold; and 4. That the same effect may be produced by any circumstance which constrains or impedes the respiration. An important point still remained to be decided; when the cooling process is going forwards by any constraint or impediment to the respiration, is the consumption of oxygen proportionably diminished? Many causes conspire to render the investigation one of considerable intricacy, but by a series of well conceived

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tigated by Dr. C. J. B. Williams, who performed a series of interesting experiments on rabbits and on birds, in which he appears to have paid the most minute attention to the resulting phenomena, and fairly deduces from them the general fact, that artificial respiration, when properly conducted, prevents the abstraction of heat, which would otherwise take place, while, in some cases, the temperature was observed to be sensibly increased by the process. His general conclusion is as follows : "From a consideration of all the facts which I have now stated, I am led to believe that animal heat is the result of chemical changes proceeding in the body, and which I have endeavoured to particularize, as those resulting from the functions of respiration and secretion, and that a due performance of these functions is requisite for the healthy and uniform preservation of animal temperature ;" Edin. Med. Chir. Tr. v. ii. p. 92 et seq. See also the remarks of Dr. Prichard on Sir B. Brodie's experiments, in his Essay on the Vital Prin., note 3, p. 205 et seq.

[1] Inquiry, p. 101, 2. Ex. 67, 8, 9.

[2] Ann. de Chim. et Phys. t. iv. p. 5, 113. In Legallois' work "Sur la Vie," he gives an account of the effect of artificial respiration, which he found to have the power of reducing the temperature, according to the observation of Sir B. Brodie, although, as he conceived, not in so *great a degree* ; Avant-propos, p. xx ; also note in p. 241, 2.

experiments, which were varied and modified in such a manner as to meet the difficulties which successively presented themselves, the general conclusion seems to be well established, that the evolution of heat always bears a relation to the consumption of oxygen, and that the variations are not greater than might be naturally expected, from the complicated nature of the operations which are always going forwards in the animal œconomy.

As Legallois' experiments are many of them novel and ingenious, and lead to many curious results, it may be proper to give a brief account of some of the most important of them. After establishing the four positions that are mentioned above, he informs us that laying an animal on its back lowers its temperature, and he examined whether in this case the consumption of oxygen was diminished. The experiment was performed on rabbits and cats. For the purpose of comparison he first operated upon the animal while it was at liberty, and afterwards when confined on its back; the results are not quite uniform, but, for the most part, there was considerably more oxygen consumed when the animals were at liberty. Upon repeating the experiment, with certain variations, it appeared that when the respiration was in any way constrained, by the animal being tied on its back, or by there being a deficiency of air, less oxygen is consumed. It was found, however, that in certain cases, the cooling was more rapid than ordinary, even when more oxygen is consumed, owing, as the author conjectures, to the struggles which are made, carrying off a portion of the heat. In order, therefore, to render the experiment still more decisive, he rendered the respiration so difficult, that no voluntary effort of the animal could enable it to consume the usual quantity of oxygen; this was accomplished either by placing it in air that was much rarified, or by mixing with it a large proportion of nitrogen[1]. The experiment was then performed in this way upon dogs, cats, rabbits, and guinea-pigs, when the greatest cooling effect was always found to correspond with the least consumption of oxygen. We are informed that the quantity of carbonic acid formed is always less than that of the oxygen which disappears. When a quantity of carbonic acid is mixed with the air, this is in part removed, so as to prove that carbonic acid is absorbed by the lungs. The author's final decision is, that the nervous system affects the temperature, but that it is by an indirect operation, in as far as it contributes to bring the air into contact with the blood. We may therefore conclude upon the whole, that although there are many difficulties which attach to particular parts of the subject, the source of animal heat is in the action of the air upon the blood, and that it ultimately depends upon the abstraction of carbon from this fluid, and the conversion of oxygen into carbonic acid[2].

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[1] Ann. Chim. et Phys. t. iv. p. 21 et seq.

[2] Dr. Holland, in his late inquiry into the laws of life, endeavours to establish, as the direct result of experiment, a doctrine quite contrary to that

As to the question, why the union of oxygen and carbon produces heat, I have already remarked, that it is one which belongs rather to chemistry than to physiology; it is sufficient for our purpose to state, that no greater difficulty occurs in one case than in the other. With respect to the second position of the theory, the mode in which the heat is distributed through the various parts of the body, it is admitted, that we are still unable to form a decisive conclusion. After attentively perusing the experiments of Crawford, and comparing them with those that have been performed with a contrary result, I confess that the balance of evidence appears to me to be greatly in favour of the former, but I acknowledge, that they are of so delicate a nature, as not to be entitled to implicit confidence, and that it would be extremely desirable to have them carefully repeated.

If we consider the subject upon general grounds, we shall find that there are many circumstances which afford a strong presumption in favour of the chemical doctrine, by tending to establish an intimate connexion between the functions of respiration and calorification. In the first place, all animals that have a temperature, much superior to that of the medium in which they are immersed, have their lungs constructed in the most elaborate manner. What are styled the warm-blooded animals, have the organs of a large size, and so formed as to permit the air and the blood to come into the most extensive proximity, and thus to exercise the most powerful influence upon each other. In the amphibia, the lungs are furnished with fewer vessels and with larger air-cells, at the same time that only a part of the blood passes through them at each circulation, and their temperature is consequently found to be proportionally low. In fishes, the quantity of blood which passes through the gills to receive the action of the air, and the chemical effect produced upon it, is still smaller, and it is found that the temperature of this class of animals differs but a degree or two from that of the water in which they are immersed. It appears, in short, that if we compare the different classes of animals with each other, we, in all cases, perceive a strict relation between their temperature, and the quantity of oxygen which they consume. It is further observed, that in the same class of animals, or even in the same individual, whatever quickens or impedes the passage of the blood through the lungs, or whatever promotes or retards the action of the air upon the blood,

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maintained in the text, that the animal temperature is in the inverse proportion to the quantity of blood exposed to the action of the air in the pulmonary vessels. Many of his experiments and of his pathological observations appear, at first view, to favour this position, but I think that they are all of them resolvable into the general fact, that in the instances adduced by him, although more blood might be actually in the pulmonary vessels, yet that its passage through them was retarded, so that in reality a less quantity was exposed to the effective action of the air in a given time. See particularly the first chapter of his inquiry.

in the same proportion increases or diminishes the temperature of the animal, so as to afford a strong argument in favour of the opinion, that these operations bear to each other the relation of cause and effect.

I shall enumerate a few circumstances, principally taken from Dr. Edwards, which, although of rather a miscellaneous nature, may be properly classed together, as they all bear indirectly upon the question of the connexion between animal temperature and the consumption of oxygen by the lungs. The phenomena of hybernation, as they were related in the last chapter, p. 407, confirm this connexion, as we find in all cases, that exactly in proportion to the diminution of the chemical effect upon the air, so is the decrease of animal heat; a fact which is abundantly proved by the references that were made to Hunter, Spallanzani, Carlisle, and others. These authors all agree, that the temperature of the external parts of the body is nearly reduced to that of the surrounding medium, while the internal parts, as well as the blood and the vital organs, are not more than 1° or 2° higher. But we have an experiment of De Saissy's, related by Dr. Edwards, which is peculiarly illustrative of this point, for he found that he was unable to bring an hybernating animal into the torpid state, by the reduction of temperature alone, without also constraining the respiration[1]. Dr. Edwards informs us, that such of the mammalia as are born with the ductus arteriosus large and open, have less power of producing heat, but that in proportion as the duct closes, their power of generating heat is increased; and that when individuals of the same species have the ducts more closed than usual, their temperature is more stationary[2]. He found by experiment, that after making due allowance for their bulk, young animals consume less oxygen than adults, and that they have a less power of generating heat, the production of heat and the consumption of oxygen being in all cases proportionate[3]. I have already adverted to the observation of Buffon, that a newly born animal can live for a certain length of time under water; the fact was confirmed by Dr. Edwards; but he found that this power belonged only to those animals which, while young, generate the least heat, that they soon lose this power, and at the same time acquire the capacity of evolving heat in a greater degree; we may presume that this difference depends upon the state of the ductus arteriosus, and the consequent state of the pulmonic circulation. We learn also that young animals differ from each other in their power of resisting cold, or which amounts to the same thing, of generating heat, and in these cases we can always perceive a relation between this power and the consumption of oxygen[4]. In addition to these considerations I may adduce the experiments of

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[1] De l'Influence &c. p. 152. [2] Ibid. p. 618. [3] Ibid. p. 190.
[4] Ibid. par. 4. c. 2.

Provençal on the effects of dividing the par vagum, of which I have already given an account, p. 391, where it was found that the temperature declines in exact proportion to the difficulty which the air has in getting access to the pulmonary vesicles. The curious observation of Prevost and Dumas, according to which the temperature of the different classes of animals is proportionate to the number of the red particles in their blood, also affords a farther confirmation of the same doctrine; for we can conceive of no other kind of action that these particles can exercise, except a chemical one upon the air[1]. It had been frequently remarked that there was a certain degree of correspondence between the temperature of an animal and the colour of the blood, but the observation had been previously made in a general way only. The lower temperature of the "Cœruleans," see p. 365, is likewise an argument in favour of the chemical doctrine of animal heat[2]. Upon the same principle we account for the lower temperature of asthmatic patients. Dr. Bree informs us that they are generally colder than other individuals; he particularly mentions one case where the temperature during the paroxysm sunk to 82°, while at other times it was 97°; and he believes that there are instances in which it is lower[3]. When from any cause the conversion of arterial blood into the venous state is impeded, as was observed by Hunter to take place during fainting, the temperature is also lowered[4].

It may be further urged as a strong argument in favour of the chemical doctrine of animal heat, that in consequence of the union of oxygen and carbon in the lungs, heat must necessarily be extricated, which can be no otherwise disposed of than in raising the temperature of the animal, although it is admitted that there is a difficulty in explaining in what exact part of the system the extrication of heat takes place[5]. Nor are we obliged to rest upon the general fact only: it is proved by decisive experiments, that under ordinary circumstances, the formation of the carbonic acid, which is produced by respiration, is an actual source of heat, which may be exactly measured, and which must be available in maintaining the temperature of the animal at the required standard. By means of the calorimeter, Lavoisier and La Place compared the heat evolved during the formation of a given quantity of carbonic acid, as produced by the combustion of carbon, with the heat evolved during the formation of the same quantity of carbonic acid by respiration, and the amount of absolute heat appeared to them to be at least as great in the latter case as in the former[1].

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[1] Ann. Chim. et Phys. t. xxiii. p. 64 et seq.; see also Dr. M. Edwards, in Cyc. of Anat., v. i. p. 411, 2.

[2] Blumenbach, Inst. Physiol. note to § 167.

[3] On Disordered Respiration, p. 137, 8.

[4] On the Blood, p. 68.

[5] Sir B. Brodie's experiments, as he correctly observes, clearly prove that the nervous influence is not necessary to the production of the chemical changes which the air and the blood naturally produce upon each other in the lungs; Phil. Trans. for 1812, p. 389.

A series of experiments has been lately performed by Dulong, on the question how far the heat extricated by the generation of the carbonic acid in the lungs is adequate to the supply of the caloric which is expended by the body under ordinary circumstances. From the mode in which the experiments were performed, as well as from the acknowledged talents of the author, we cannot but consider the results as entitled to much attention. The apparatus appears to have been skilfully contrived, so as to allow the animal to remain as little under restraint as possible, while it was capable of measuring with accuracy the quantity of heat that was evolved by respiration; and in order to render the results less liable to objection, six different kinds of animals were employed.

The conclusion that Dulong draws from his experiments is, that the quantity of caloric disengaged by the lungs, although considerable, and an actual source of temperature, is not sufficient to account for the whole of the animal heat[2]. But we may remark, that with all the care which was bestowed upon the experiments, and valuable as they must be considered, there are still some points that require farther inquiry. In the first place, as the author is himself aware, his conclusion essentially depends upon the correctness of the estimate, which was made by Lavoisier and Seguin, of the quantity of heat extricated during the combustion of a given weight of charcoal. For this purpose these philosophers employed their calorimeter, an instrument which, although highly ingenious, we have reason to suppose, is not capable of affording very correct results.

In the second place, it appears that, besides the oxygen which was expended in the formation of the carbonic acid, Dulong found that a considerable quantity, in some cases amounting to one-third of the whole, was expended in some other way; but in whatever manner it was employed, in losing its aeriform state, it must evolve a quantity of caloric, which should be taken into account in forming our estimate. It may be farther observed, that we cannot be permitted to assume that the same quantity of heat will be extricated by the combustion of charcoal heated in oxygen gas, as by the union of the carbon emitted by the lungs with the oxygen of the inspired air; and, lastly, it is not unfair to suppose that, notwithstanding all the care bestowed upon the experiments, there might be various circumstances connected with the actions and organization of the animal, which would affect the regularity of the functions and their relation to each other, and which might, in various ways, interfere with the results.

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[1] Mém. Acad. Scien. pour 1780, p. 407 et seq.; see also Menzies on Respiration, p. 39 et seq.; Thenard, Traité, t. iii. p. 670, 1. De la Rive, De Calore Animali, gives the following estimates; the heat employed in evaporating the water that is discharged from the lungs is sufficient to melt 3.5 lbs. of ice, that necessary to raise the temperature of the inspired air 29 lbs., making together 32·5 lbs. The heat which is evolved by the formation of the carbonic acid would melt 108 lbs. of ice, that by the absorption of oxygen in the lungs, besides what is employed in forming the carbonic acid, would melt 26 lbs., making together 134 lbs.; hence we have the heat which would be required to melt 101·5 lbs. of ice left to maintain the temperature of the body. The first three of the above quantities are pretty correctly ascertained; with respect to the fourth, it would be almost impossible to form an average of its amount.

[2] Magendie, Journ. de Physiol. t. iii. p. 45 et seq.

But even were we to admit the conclusion of Dulong, without any restriction, it will appear that we have still a large quantity of heat extricated in the lungs, which must necessarily raise the temperature of the body, although there may be some reason to doubt, whether it be adequate to the whole of the effect which is produced. Upon the whole, however, I conceive, that the information which we possess on the subject will lead us to the opinion, that, under the ordinary circumstances in which the animal is placed, the heat extricated by the lungs is sufficient to maintain its temperature; the difficulty will therefore be not to find a source for the heat which animals possess under ordinary circumstances, but to account for the deviations which occur in extraordinary cases, or in unnatural situations of the individual. These deviations, it is admitted, we frequently find it very difficult to explain, but they cannot be allowed to form any objection to the general doctrine, which appears to be founded upon a fair induction from a sufficient number of direct and well established facts.

To the deviations which are occasioned by the general state of the system, independent of mere chemical action, may be referred some interesting observations of Dr. Edwards, on the comparative power which young animals possess of producing heat. In opposition to the general opinion, that their temperature is superior to that of the adult, he found that young animals indicate a lower degree of heat[1]; and that in some cases, as that of the puppy, kitten, and rabbit, if they be removed from the mother, and exposed to an atmosphere of between 50° and 70°, their temperature will sink to nearly the same degree. This imperfect power of producing heat exists in the greatest degree immediately after birth, and progressively diminishes, until, in about 15 days, they acquire the faculty of generating heat in the same degree with the adult[2]. The observation does not, however, apply to all the mammalia; this defect appeared to be confined to those animals that are born blind, thus indicating a state of imperfection in the general state of their organs and functions. The case is the same with birds as with the mammalia; i.e. some birds which are born in a defective state, with respect to their organs generally, have a defective power of producing heat, whereas those that are born in a more perfect condition, have the respiratory organs also more capable of exercising their functions. Dr. Edwards correctly points out the analogy which these animals bear to the hybernating tribes; they may both of them be considered as the connecting links between the two great divisions of the warm and the cold-blooded[1].

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[1] Vide supra, p. 436.

[2] There is a difference of opinion respecting the temperature of the human infant, which must be removed by farther observations. Haller remarks, in conformity with the common opinion, that children have less power of producing heat than adults; El. Phys. vi. 3. 10; and this is confirmed by the observations of Dr. Edwards and Despretz, see above, p. 436. We have, however, on the contrary, the direct testimony of Dr. Davy, that the temperature of young animals generally, and that of a newly born child, which he particularly examined, is higher than in the adult; Phil. Trans. for 1814, *p. 602, 3*; a statement which is confirmed by the observations of Dr. Holland.

In the last place it may be urged, that if we reject the chemical doctrine of animal heat, we have no adequate hypothesis to substitute in its place; for although it is admitted that, in some way, which we are not very well able to explain, the nervous system exercises a powerful influence over the extrication of heat, we have no analogy which would induce us to suppose that the nerves can directly produce a chemical change, or that they can alter the chemical relation of substances to each other, unless by the intervention and introduction of some new chemical or physical agency, of which, in this case, we have no intimation, and of the nature of which we can form no plausible conjecture.

An exception to the above remark may appear to be deducible from the curious discovery of Dr. Philip, respecting the action of galvanism upon the blood. We learn from him, that if the galvanic influence be applied to fresh drawn arterial blood, an evolution of heat, amounting to 3° or 4°, takes place, while the blood assumes the venous hue, and becomes partially coagulated; it appears also that this extrication of heat cannot be produced from venous blood, or even from arterial blood, except immediately upon its being discharged from the vessels[2]. Dr. Philip draws an inference from the experiment in favour of his hypothesis of the identity of the nervous and the galvanic influence, for, as he supposes that one of the functions of the nervous system is to assist in maintaining the temperature of the body[3], it appears that we have here, in like manner, the evolution of heat produced by the application of galvanism.

I shall reserve my observations on this hypothesis to the next chapter, which treats upon secretion, but it will be necessary to make a few remarks in this place upon the experiment in question, so far as it relates to the subject of animal temperature. Although there is considerable difficulty in explaining the intimate nature of the galvanic action, yet we know that its specific effect is to induce certain chemical changes in the substances to which it is applied, and that as far as these changes are concerned, it is to be regarded in no other light than as a chemical agent. As the evolution of heat is, strictly speaking, a chemical effect, we must inquire what difference there is between arterial and venous blood, and between arterial blood, when fresh drawn, and the same fluid after it has been for some time discharged from the vessels, which might be supposed to be affected by the chemical action of the galvanic apparatus. With respect to the first of these points, we may go so far as to say, that the various experiments which have been performed on the subject lead us to suppose, that the arterial blood contains an excess of oxygen and the venous of carbon. Dr. Philip's experiments therefore show us, that the galvanic influence has the power of acting upon, or decomposing, oxygenated blood in a greater degree than carbonated blood, but, except this simple fact, it may be doubted whether any further influence can be deduced from them. There is much more obscurity in the change which the blood undergoes when it leaves the vessels, but it is certain that its chemical and physical properties begin almost immediately to experience an alteration; and it seems that this alteration, whatever it be, renders it less liable to be decomposed by the action of galvanism. Upon the whole, however, I may observe that Dr. Philip's experiments, so far as we are able to comprehend their nature, are favourable to the chemical doctrine of animal heat. They prove to us that heat is evolved from blood by a chemical operation, also that it is more easily evolved from arterial than from venous blood, or rather that when the blood becomes venalized, it is no longer capable of giving out heat by the same process which evolves it from arterial blood.

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[1] De l'Influence &c. par. 2. c. 1.

[2] Inquiry, p. 168 et seq.; Quart. Jour. v. xiv. p. 105 et seq.

[3] Inquiry, ex. 76..82. p. 238 et seq.; Quart. Jour. v. xiv. p. 106.

I shall give a brief abstract of the opinions of some of the most eminent physiologists respecting the chemical doctrine of animal heat, in addition to those that have been already noticed. Menzies gives his assent to the hypothesis, and offers various arguments in support of it[1]; and the same is the case with Fourcroy[2]. Dr. Ellis offers various considerations in favour of the doctrine, principally derived from the correspondence between the temperature of the animal and the extent of the lungs, or the facility with which the blood is transmitted through them[3]. Dr. Henry gives his full assent to Crawford's theory[4]; and Dr. Dalton, after discussing the nature of the objections that have been urged against it, concludes that it is not affected by them[5]. Thenard ascribes animal heat to the union of the oxygen of the air with the carbon of venous blood[6]; Magendie supposes that the combination of the oxygen of the air with the carbon of the blood is sufficient to explain most of the phenomena of animal heat, although he conceives there are certain facts which cannot be accounted for upon this principle. The only circumstance to which he refers is the increased temperature of the blood drawn from a part suffering under local inflammation, which, as he says, is considerably above that of the blood in the left side of the heart: but the fact is stated generally without any reference[1]. Dumas remarks upon the correspondences between the capacity and structure of the lungs and the temperature of the animal, and hence draws an inference in favour of the chemical theory. He, however, considers it an objection to this doctrine, that the temperature of the animal remains the same, whatever be that of the surrounding medium; he also thinks that the quantity of oxygen consumed in respiration is not sufficient to serve for the support of the temperature, and for the removal of the vapour and carbonic acid; but his objections are brought forward in a general manner, without reference to any particular facts[2]. Legallois, at the conclusion of the experiments, of which an abstract is given above, p. 451, although he supposes that the nerves have an indirect effect upon the evolution of animal heat, states his opinion to be, that when the blood is arterialized its capacity is increased, and that when it is venalized in the capillaries its capacity is changed, and its heat given out; and adds that when, from any cause, the respiration is rendered laborious, by the proper quantity of oxygen not being admitted to the lungs, the temperature is proportionally reduced[3].

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[1] Essay on Resp. p. 36 et seq.

[2] System, v. x. p. 524, 5.

[3] Inquiry, p. 233, 240 et alibi.

[4] Elements, v. iv. p. 407.

[5] *Manch. Mem.* v. ii. (2d ser.) p. 38, 9.

[6] Chim. t. iii. p. 670.

Seguin brings forward various considerations in favour of the connexion between animal heat and the chemical change which the blood experiences in respiration[4]; and Sœmmering supports the chemical doctrine of animal heat[5]. Cuvier supposes that respiration causes the combination of oxygen with carbon and hydrogen, in consequence of which heat is disengaged. He says, "Le poumon est le foyer de la chaleur animale, et c'est là que le sang puisse celle qu'il porte dans le reste du corps."[6] The chemical doctrine of animal heat, in its essential parts, is also adopted by Blumenbach, although he introduces some modifications into Crawford's theory; he supposes that the blood leaves the lungs charged with latent heat, in consequence of the oxygen which it contains; that it acquires carbon in the small vessels, and sets its latent heat at liberty[7]. Dr. Elliotson, likewise, although aware of the force of the objections that have been urged against it by Sir B. Brodie and others, admits that "many circumstances favour it;" and appears, upon the whole, disposed to adopt it[8]; Dr. Prichard also decides in its favour[9], and Dr. Roget admits "that it affords the best explanation of the phenomena of any theory yet proposed, and that, therefore, it is probably the true one[1]. I may also remark concerning Dr. Philip's opinion, that although he regards the nervous system as a necessary instrument in the process of calorification, yet we may infer, that he supposes the immediate effect to be produced by the union of oxygen and carbon[2]. Sir A. Carlisle remarks upon the correspondence between the comparative anatomy of the lungs and the temperature of the animal[3]. Dr. Davy's general conclusion is against Crawford's hypothesis, because he finds no material difference between the capacities of arterial and venous blood, that the temperature of the left side of the heart is higher than that of the right, and that the temperature of the parts of the body generally diminishes as we recede from the heart. These facts, he observes, are more agreeable to Black's hypothesis, although they may be explained on Sir. B. Brodie's; but upon the whole, he inclines to Black's[4].

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[1] Physiol. t. ii. p. 400.
[2] Physiol. t. iii. p. 115. 121..171.
[3] Ann. de Chim. et Phys. t. iv. p. 118 et seq.
[4] Ann. Chim. t. v. p. 259, 0.
[5] Corp. Hum. Fab. t. vi. § 76. in the chap. on Respiration.
[6] Tab. Elém. p. 46.
[7] Inst. Phys. § 167. p. 97, 8.
[8] Inst. Phys. p. 101, 4.
[9] Essay on the Vital Princ. sect. 11.

SECT. 2. *Of the means by which the Animal Temperature is regulated.*

Having endeavoured to establish the doctrine that the source of animal heat is in the lungs, and that it depends upon the chemical action of the air on the blood, the next point for our consideration will be the mode by which the uniformity of the animal temperature is preserved. We find that, among the warm-blooded animals, to whatever degree of heat or of cold the body is exposed, the blood and the internal parts always indicate nearly the same degree of heat[5]. According to the view which has been taken above of the cause of animal heat, the question in its strict form will be, by what means is the combination of oxygen and carbon in the lungs so regulated, as that the evolution of heat is in proportion to the demand for it in the system.

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[1] Bridgewater Treatise, v. ii. p. 340.

[2] Inq. p. 250 et alibi.

[3] Phil. Trans. for 1805, p. 15.

[4] Phil. Trans. for 1814, p. 600..3. Mr. Mayo observes on this point, "we must admit that the source of vital heat remains unknown;" Physiol. p. 96. Despretz performed a series of experiments on guinea-pigs, from which he concludes, that respiration is the principal cause of the development of animal heat; he also supposes that more oxygen disappears than is consumed in the formation of the carbonic acid; Magendie's Journ. t. iv. p. 143 et seq. Nearly the same doctrine is maintained by Josse, in his Observations sur la Chaleur animale, and by Broussais, Physiol. t. ii. p. 65. Collard de Martigni, on the contrary, considers calorification to be a nervous function, and supposes that the chief use of the inspired air is to cool the lungs; Magendie's Journ. t. x. p. 136. We have a judicious summary of opinions on the various points connected with animal temperature in Adelon, Physiol. t. iii. p. 398 et seq. See also the art. "Chaleur animale," by Coutanceau, Dict. de Méd. t. v.

[5] The latest observations, and we may presume the most correct, that we possess on this subject, lead us to conclude, that this uniformity is not quite so great as was formerly supposed, especially when we arrive at a temperature nearly equal, or superior to, that of the body itself. This is decidedly proved by the experiments of Delaroche and Berger, who found that by exposing warm-blooded animals to very high temperatures, they experienced an elevation equal to 7° or 8° (cent.); Journ. Phys. t. lxxi. p. 289 et seq. Dr. Edwards's observations on birds at the different seasons of the year,

But in order to solve this problem, a previous inquiry presents itself; is the combination of oxygen and carbon in the lungs always going forwards at the same rate, and consequently adapted to the lowest temperature, which is consistent with the continuance of life, or is it found to vary in the inverse proportion to the temperature of the animal? And there is a farther consideration which we must now enter upon; when an animal is placed in a medium, the temperature of which is higher than that natural to itself, we find that the body still remains at nearly its ordinary standard. We have therefore in this case, not merely the suspension of the process by which heat is generated, but it would appear that the contrary effect must be produced, that the body must possess the power of resisting heat, or of actually generating cold. This leads us to the third question which was proposed in the commencement of the chapter; by what means is the body cooled at high temperatures? And as there would appear to be an intimate connexion between this inquiry and the former, it will be more convenient to investigate them in conjunction with each other.

The cases in which the body is exposed to a temperature greater than what is natural to it, are so rare compared to the contrary occurrence, and the effects of such exposure are, in various ways, so unfavourable to the exercise of the vital functions, that it was formerly assumed as an acknowledged matter of fact, that life could not exist under such circumstances. Boerhaave conceived that he had proved this point by direct experiment: but from causes which we cannot now ascertain, there must have been some source of inaccuracy, which inter-

showed that there was a difference of 4° (cent.) between the winter and summer months; De l'Influence &c. p. 489. Dr. Davy observed that the temperature of the inhabitants of Ceylon was 1° or 2° higher than the ordinary standard; ibid. The uniformity of the animal temperature appears to be more steadily maintained in great degrees of cold, where it seems that the heat of the internal parts is scarcely diminished, as long as the functions proceed in their ordinary course. We have many accounts to this effect by travellers and naturalists, but none, perhaps, upon which greater reliance can be placed, than upon that of Capt. Lyon, contained in Capt. Parry's Second Voyage to the Arctic Regions, p. 157. The observations were principally made upon newly-killed foxes, the temperature of which was found to be from 106¾° to 98°, that of the air being from 3° to 32°; it did not appear that there was any relation between the temperature of the air and of the animals. The observations were made at Winter Isle, N. lat. 66° 11′. As far as regards the sensations, the experience of these voyagers proved that the state of motion or rest in the atmosphere had a very great effect in exciting the feeling of cold; they found a calm air of — 50° more tolerable than a breeze at 0°.

fered with his results, and led him to an erroneous conclusion[1]. The first well authenticated facts which disproved this opinion, were communicated by Tillet and Duhamel, who gave an account of some young women, the servants of a baker at Rochefoucault, in Angoumois, who were in the habit of going into the heated ovens, in order to prepare them for the reception of the loaves.

The temperature to which they were exposed seems to have been, in some cases, as high as 278° Fahr., and this heat it is said, they were able to endure for 12 minutes without any material inconvenience, provided they were careful not to touch the surface of the oven[2]. The statement excited great astonishment, and at the time was scarcely credited, but it is fully confirmed by subsequent observations. A series of experiments were performed by Fordyce, Blagden, and others, where a chamber was heated to a temperature considerably above that of boiling water, in which these gentlemen found they could remain without much inconvenience for almost an indefinite length of time[3]. Another set of experiments of the same nature were performed at Liverpool by Dobson with similar results[4]. These experiments are valuable, inasmuch as they fully establish the fact, and prove that there was no deception in the case, while they accurately mark the degree of heat both by the thermometer, and by its effect on inanimate substances that were exposed to it. But it is to be regretted, that the attention was almost exclusively confined to this point, and that while they were observing its action on the surrounding bodies, they almost entirely neglected to notice its effects on the living system. They indeed inform us that the temperature of the body was little if at all raised[1], and that in some of their experiments a profuse perspiration took place; but even this is stated in a general way, so as not to give us any certain grounds for concluding whether it had any effect in lowering the temperature. This is the more remarkable, as Franklin had, some years before, with his usual penetration, suggested that the evaporation from the surface might be a means of diminishing the temperature of the body when exposed to great heats[2]. With respect to the effect of evaporation, the observations of Fordyce and his friends, imperfect as they were, seemed to be adverse to the hypothesis of Franklin; and the same conclusion was likewise formed by Crawford, who conceived that Fordyce had sufficiently proved, that evaporation could not account for the cooling effect which had been observed by him in his experiments. Crawford himself explained it upon the principle, which has been already referred to, that in proportion to the elevation of the temperature to which the body is exposed, the blood becomes less venalized, and, in the same proportion, loses the property of evolving heat, in consequence of its containing a less quantity of inflammable matter[3].

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[1] The experiments were performed by Fahrenheit at the suggestion of Boerhaave, and the result is stated to have been that animals could not support a heat of more than 146°; Boerhaave, Chem. t. i. p. 275, 6. Various accounts were, from time to time, published by travellers, of the high temperature to which the body is subjected in tropical climates, which were often many degrees above that of the human body. See Blumenbach, Physiol. p. 97; Lawrence's Lect. p. 206; and Delaroche, in Journ. Phys. t. lxiii. p. 207. We have some curious observations by Dr. Edwards on the effects of different temperatures on the cold-blooded animals; a frog which can live for 8 hours immersed in water at 32°, is destroyed in a few seconds in water at 105°; this appears to be the highest temperature which cold-blooded animals are able to bear.

[2] Mém. Acad. Scien. pour 1764, p. 186 et seq.

[3] Phil. Trans. for 1775, p. 111 et seq. The conclusion which Blagden draws is, that, "No attrition, no fermentation, or whatever else the mechanical and chemical physicians have devised, can explain a power capable of producing or destroying heat, just as the circumstances of the situation require;" p. 122. In a second paper in the same volume, p. 484 et seq., he observes that although the evaporation from the surface might have some effect, it was "by no means sufficient to account for the whole of the cooling;" p. 488.

[4] Phil. Trans. for 1775, p. 463 et seq. In one of the experiments related by Dobson, a young man remained in a temperature of 224° for 10 minutes; p. 464. In the second set of experiments performed in London, Blagden remained for several minutes in a temperature of 260°; this, however, it will be observed, is considerably less than the heat to which the young women in France were exposed.

Some ingenious observations on the effect of high temperatures were made by Bell of Manchester, in which, although he admitted the correctness of the facts, he endeavoured to prove that the operation of the heated air upon the body would be considerably less than might have been expected, because in proportion as the air is heated, it is so much expanded, that comparatively few particles will come into contact with the surface of the body[4]; hence we find that while there was little difficulty in resisting the heat, when the body was in contact with the air alone, it was impossible to touch any solid substance without exciting pain, so that even the clothes soon acquired a temperature which rendered it difficult to bear them without inconvenience. Bell's remarks, however, although they diminished the difficulty, did not remove it, and it was not until the subject was investigated by Delaroche, that we could be considered as possessing any real insight into this intricate subject. We are indebted to this ingenious physiologist for three valuable memoirs, which were published in succession[1], and which each of them, considerably advanced our information on the effect of high temperatures upon the body, and on the means by which animals are cooled, when exposed to a degree of heat greater than their own.

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[1] Phil. Trans. for 1775, p. 487..9; in this experiment, where the body was exposed, nearly naked, to a temperature of 220°, we are informed that a profuse perspiration took place.

[2] Letter to Lining, Works, v. ii. p. 85; Journ. Phys. t. ii. p. 454 et seq.

[3] Phil. Trans. for 1781, p. 487..1; On Animal Heat, p. 382..9. Some sensible observations were made upon the experiments of Fordyce, shortly after they were performed, by Changeux, Journ. Phys. t. vii. p. 57 et seq., pointing out their imperfection in respect to the physiological inferences that might be deduced from them.

[4] Manchester Memoirs, v. i. p. 1 et seq. He supposes that three circumstances may concur to prevent the operation of the high temperature; 1. The rarefaction of the air; 2. The evaporation; and lastly, The afflux of the colder fluids from the central parts of the system to the centre. We learn from Crawford, Phil. Trans. for 1781, p. 484, that Monro attributed the effect principally to this latter circumstance. Some curious facts that were noticed by Currie, respecting the effect of the application of water at different temperatures to the surface of the body, may be referred to the abstraction of heat by evaporation; to the same cause may, in a great measure, be ascribed the different cooling powers of fresh and salt water; Phil. Trans. for 1792, p. 199 et seq.

His first paper contains an account of a series of experiments performed on himself and Berger, in which they were exposed to a temperature of between 225° and 228°, during which they noticed the amount of the evaporation, and also the chemical change produced upon the expired air. Experiments of a similar nature were also performed on various animals, from which it appeared that the effects differed considerably in the different species, but that, for the most part, the larger suffered more than the smaller animals. It has been already observed that the experiments of Crawford, Jurine, and Lavoisier, seemed to show, that at high temperatures there is a less consumption of oxygen in the process of respiration, and this was generally regarded as, at least, one means by which the temperature of animals is regulated; but Delaroche informs us, on the contrary, that he was not able to observe any relation between the temperature of the air, and the degree of its deoxidation.

In his second paper, he more minutely attended to the temperature of different kinds of animals when exposed to great degrees of heat; he found it to be not so stationary as had been generally supposed, being frequently raised by 10° or 13° above the natural standard, although it still remains very much below the temperature in which an animal can live for a certain length of time without any considerable uneasiness. With respect to cold-blooded animals, although their heat is much less stationary than those with warm blood, he found that in high temperatures it fell considerably below that of the medium; frogs, for example, in air heated from 110° to 115°, were no more than 80° or 82°. He entered upon a second set of experiments for the purpose of directly ascertaining the effect of evaporation upon the temperature of animals, the results of which fully confirmed his former opinion, pointing out an obvious relation between the amount of evaporation and the degree in which the animals were able to resist great heats.

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[1] See, for the 1st paper, Journ. Phys. t. lxiii. p. 207, and Nicholson's Journ. v. xvii. p. 142. 215; for the 2d paper, Journ. Phys. t. lxxi. p. 289, and Nicholson's Journ. v. xxxi. p. 361; for the 3d paper, Journ. Phys. t. lxxvii. p. 1. They were published respectively in the years 1806, 1809, and 1812.

One set of experiments consisted in enclosing various animals, for example, rabbits, guinea-pigs, and pigeons, in a box which was filled with steam; it was found that when the temperature of the apparatus was equal to, or greater than that of the body, the temperature of the animal was raised, and the inconvenience which it suffered was proportionally great.

In his third paper, Delaroche more particularly directs his attention to the chemical effects of respiration upon the air under different circumstances. Having proved in the former paper that when the evaporation from the skin and lungs is prevented, the cooling process is suspended, it was an interesting subject of inquiry whether, in this case, the chemical effects of respiration continue with as much activity as under ordinary circumstances. The experiments which were performed for the purpose of ascertaining this point seem to have been sufficiently numerous and well conducted, and the result was, that the consumption of oxygen is greater as the temperature is lower, upon the average, about as 6 to 5. The formation of carbonic acid does not, however, follow the same ratio with the consumption of oxygen, being not only in all cases less, so as to indicate an absorption of oxygen, but it was found that this excess, or the difference between the oxygen consumed and the carbonic acid produced, was less at a high than at a low temperature, amounting upon the average to no more than one-tenth, or not more than half of the difference of the consumption of oxygen at different temperatures. The opinion of Crawford, Jurine, and Lavoisier, appears therefore, to a certain extent, to be thus confirmed, but Delaroche supposes that the effects observed in these experiments were not sufficient to explain the equalization of the temperature, without the aid of the cooling process of evaporation, and indeed this had been so fully established in the former experiments as to leave no doubt of its agency. Our general conclusion will therefore be, that at high temperatures there is less caloric actually evolved, but that the circumstance which principally contributes to equalize the heat is the cutaneous and pulmonary evaporation, while at temperatures above that which is natural to the animal, the cooling process must be entirely ascribed to this operation[1].

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[1] Black very explicitly states his opinion that the heat which is necessarily absorbed in spontaneous evaporation, contributes to enable the body to bear the warmth of tropical climates. He particularly adverts to Fordyce's experiments, and states his opinion that it was owing to the evaporation from the surface that he was able to endure so high a temperature; Lectures by Robison, v. i. p. 214. Lavoisier, in the course of his researches into the nature of respiration and animal heat, frequently refers to the mode by which it is equalized, when the body is exposed to different temperatures. He particularly notices this subject in his paper on Transpiration, published in the Mém. Acad. pour 1790; he defines this function to consist in "a loss of moisture, which requires heat to dissolve it in the air, and which, by the cold thus produced, prevents the temperature from rising above the degree natural to the animal." This opinion was generally adopted by his contemporaries,

But, although we are very much indebted to Delaroche, there are still some points that require farther investigation. It is proved that evaporation abstracts heat from the body, but it still remains somewhat doubtful, whether the effect thus produced be adequate to the demands of the system, or whether there may not be some other means employed which may co-operate to the same end. It would be desirable to examine with more minuteness than has hitherto been done into the quantity of vapour formed by the body as compared with the cooling effect produced. We should ascertain what temperature would be acquired by an inanimate mass of matter of the same bulk and capacity for heat with the animal body, and compare this with the actual temperature gained, and this again with the quantity of vapour generated. It would also be desirable to ascertain with more minuteness what is the exact effect of the respiratory organs of animals at a temperature higher than what is natural to them, first when the process of evaporation is suffered to proceed, and afterwards when it is suspended; is there any oxygen consumed under these circumstances, or to what amount, or in what degree, does the quantity differ in the two cases? The supposed power of the animal system in producing cold is one that has been treated of in a singularly mysterious manner, and much vague and indeterminate speculation has been employed upon a subject with which the framers of the hypothesis appear to have been very inadequately acquainted. The information which we derive from the experiments of Delaroche, although not in every respect complete, is, however, of great value; and it enables us to trace out a connexion between the different functions of the lungs, so as to afford, perhaps, the most interesting example which occurs in the animal œconomy, of that beautiful adjustment of the functions to each other, upon which I have had occasion to remark in other parts of my work; for it appears that not only have the lungs the power of evolving heat in greater or less quantity in proportion to the demands of the system, but that the same organs, under other circumstances, can produce the directly contrary effects, and actually generate cold.

The researches of Dr. Edwards afford us some valuable information on the property which the system possesses of equalizing its temperature under different circumstances. He found that warm-blooded animals have less power of producing heat, after they have been for some time exposed to an elevated temperature, as is the case in summer, while the opposite effect is produced in winter. A series of comparative experiments were performed, which consisted in exposing birds to the in-

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but it could scarcely be regarded as more than a probable conjecture before the experiments of Delaroche. See Gregory's Conspectus, § 578; the Observations on Animal Heat, from § 571 to § 580, contain a succinct and elegant epitome of the doctrines which were the most approved when the work was published.

fluence of a freezing mixture, first in February, and afterwards in July and August, and observing in what degree they were cooled by remaining in this situation for equal lengths of time; the result was that the same kind of animal was cooled six or eight times as much in the summer as in the winter months[1]. This principle he supposes to be of great importance in maintaining the regularity of the temperature at the different seasons, even more so than evaporation, the influence of which, in this respect, he conceives has been much exaggerated[2]. It would appear, however, that the sudden application of a high or a low temperature has a different effect upon the power of generating heat, from its more gradual and long continued application; in this case the heat is abstracted more rapidly from the animals after they have been exposed to cold, and less so after they have been exposed to heat[3]. Hence we may conjecture that in sudden and great elevations of temperature, the great agent is evaporation, while in that progressive increase and decrease which depends upon the change of the seasons, the equalization of temperature is effected, at least to a certain extent, by an alteration in the power of producing heat[4].

The result of our inquiry into the subject of animal heat has brought us to the conclusion, that it is the immediate effect of respiration, and that the lungs are the apparatus by which the heat of the system is evolved, and its temperature regulated. This is accomplished by the discharge of carbon and water, the first depending upon a chemical combination of the oxygen of the atmosphere with a portion of carbonaceous matter derived from the blood, during which combination heat is necessarily extricated; the second upon the abstraction of a portion of the heat thus extricated in consequence of the evaporation of water from the surface of the pulmonary cavities.

Hence we obtain an answer to the second and third questions which were proposed at the commencement of this chapter, by what means is the uniformity of the temperature preserved under different circumstances? and how is the body cooled when exposed to temperatures higher than itself? With respect to the first point, it appears probable, that there is a provision in the lungs, by means of the combination of carbon and oxygen, for the production of the greatest quantity of caloric that can, at any time, be required for the wants of the system; that when a less evolution of heat is necessary, this is partly effected by a diminution in the quantity of oxygen and carbon combined, but that it is principally brought about by the absorption of heat, in consequence of the evaporation of water, a process which is probably at all times going forwards, but which is increased at high temperatures; and so far in proportion to the temperature, that, within certain limits, it can prevent the undue accumulation, or carry off the excess of caloric, and prevent the body from acquiring a temperature beyond that which is natural to it.

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[1] De l'Influence &c. par. 3. chap. 3.

[2] Ibid. p. 486, 7.

[3] Ibid. par. 4. chap. 3, 4.

[4] I may remark that the observations of Dr. Edwards upon the effect of the long continued application of heat and cold afford a general confirmation of the statements formerly made by Crawford and Lavoisier, and more lately by Delaroche, respecting the different degree in which the chemical state of the air is affected by respiration at different temperatures. They had indeed no idea of the difference which appears to result from the mode of applying the temperature, which, as we have seen, is so great as absolutely to reverse the effect; but so far as the conclusion from their experiments is concerned, it coincides with what may be deduced from Dr. Edwards's. In considering the nature of the operation by which the body is cooled, it is always necessary to bear in mind, that it is effected, under ordinary circumstances, in two ways, by a diminution of the process by which heat is evolved, and by the absolute abstraction of heat. Before quitting this topic, I may remark, that Magendie, in referring to the experiments of Delaroche, observes, "point de doute que l'evaporation cutanée et pulmonaire ne soit la cause pour laquelle l'homme et les animaux resistent à une forte chaleur." Physiol. t. ii. p. 403.

The lungs are materially assisted in this cooling process by the perspiration from the skin, which, like that from the pulmonary vesicles, increases in the ratio of the temperature, and serves still more effectually to abstract the excess of caloric. And I may farther observe, that not only is the pulmonary and cutaneous evaporation immediately increased by whatever raises the temperature, but that the same circumstances provide for the production of a still greater effect, by increasing the quantity of the evaporable matter in consequence of the circulation being quickened, and all the secretions being proportionally augmented.

It would appear that we are not yet in possession of any facts which can enable us to compare accurately the quantity of carbonic acid formed, and of water evaporated, with either the heat generated on the one hand, or with what is absorbed on the other, nor to ascertain the proportional operation of the skin and the lungs. It seems, however, not an unfair assumption to regard them as adequate to the supposed effects, because we not only know that these processes are respectively the cause of the increase and diminution of temperature in bodies that are exposed to their influence, but because we find that they possess in themselves a provision for regulating the temperature of the living system, which would be altogether useless, were it not employed for this specific purpose.

I have endeavoured to show, in the preceding chapter, that this removal of carbon from the blood, is an operation essential to the well-being of the animal, independently of its effects in generating heat, and it is not improbable that the evaporation of the water may serve some other useful purpose, besides that of cooling the body at high temperatures. We may conclude, upon the whole, that these two functions, that of respiration and that of calorification, are strictly connected together, and mutually contribute, not only to the due performance of each other, but likewise to the integrity and perfection of the whole animal œconomy.

Having thus attempted to account for the operations by which the temperature of the body is regulated, it is necessary to consider how far they are connected with the other vital actions. The source of the heat which is evolved in the lungs would appear to be a proper chemical combination, of essentially the same nature with the combustion of charcoal. Now in this case there does not appear to be any thing absolutely necessary more than merely bringing the substances that are to act upon each other into sufficiently extensive approximation, which is effected by the physical structure of the lungs, and the mechanical action of the parts connected with the thorax. By the conjoined operations of digestion, secretion, excretion, and the other functions which affect the constitution of the blood, this fluid is brought into a proper state to be acted upon by the contractile power of the heart and the capillaries, it is propelled into the appropriate organs, and while it is in this situation, the air is allowed to act upon it. The only direct vital action in this case would appear to be the contraction of the muscular fibre, at the same time that various chemical changes are brought about in the nature of the blood, which indirectly co-operate to effect the ultimate object. In the same manner we shall find that the production of cold in the lungs depends directly upon mechanical, and indirectly upon chemical causes, the air being mechanically impelled into the lungs, by the intervention of the contractility of the muscular organs connected with the chest, while a quantity of a secreted substance is provided, the watery part of which is dissolved by, or diffused through the air, and, as in other cases of evaporation, produces cold. In the same manner, or even by a more simple process, is the cutaneous evaporation effected; for here nothing more is necessary than for the secretory arteries to pour out the fluid through their capillary extremities, which is immediately carried off by the air that is in contact with the body.

Although in the actual state of the system it is impossible to conceive of such a successive train of operations, without the intervention of the nervous influence, yet this intervention would appear to be rather of an indirect or incidental kind, than one which is directly essential to the effect produced. How far the nervous agency is concerned in digestion and secretion will be considered hereafter, but supposing the blood and the different fluids to be properly constituted, there seems to be nothing farther necessary, than that the former should be conveyed through the pulmonary vessels, and the latter poured out on its proper surfaces, while the air is duly impelled into the vesicles, for the carbonic acid and aqueous vapour to be generated. The nice adjustment of these operations to each other, and to the demands of the system, affords an example of that admirable adaptation of contrivance, which pervades every part of the animal œconomy, but in which no powers or principles of action are concerned essentially different from those which are exerted in the other parts of the living system. It is indeed to this simplicity of action, or to the complicated results which are brought about by the operation of a few general principles only, that the perfection of the animal machine is mainly owing, in which it so greatly surpasses the most ingenious of human contrivances, and which more especially renders it a subject of our never-ceasing wonder and admiration [1].

That the nervous system is indirectly concerned in the production and regulation of animal temperature appears, however, to be sufficiently proved by various observations and experiments. Legallois, although he found so exact a correspondence between the chemical effects of respiration and the degree of heat extricated, still conceives that the animal temperature is very much under the influence of the nervous system, so as to lead him to conclude that whatever weakens the nervous power, proportionally diminishes the power of producing heat [2]. We may likewise draw the same inference from the experiments of Sir B. Brodie, and from the pathological observations of Mr. Earle and others of a similar kind. This is also the necessary deduction from the experiments of Dr. Philip, in which it appeared very clearly that the nervous influence is so intimately connected with the power of evolving heat as to lead him to conclude, as we remarked above, that the nervous power is a necessary intermedium between the different steps of the operation. In consequence of his discovery of the evolution of heat by galvanism from arterial blood, he regards the process of calorification as a case of secretion, and explains it upon his general principle of the identity of the nervous and galvanic influence, and of this influence being essential to the function of secretion [3]. Although it may, perhaps, be considered as merely a verbal inaccuracy, yet I should object to applying the term secretion to the extrication of heat, when the expression is intimately connected with the establishment of an hypothesis.

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[1] "Mechanice organum id laudat, ejusque auctorem celebrat sapientissimum, quod quæsito effectui producendo aptissimum, simulque inter omnia, quæ eundem præstare possent, simplicissimum est." Boerhaave, Oratio de Usu Ratioc. Mech. p. 14.

[2] The destruction of a part of the spinal cord was found to diminish the temperature of an animal, and this, as far as appeared, without the disturbance of any other function; Philip's Inq. c. 7. § 2; Ex. 55, 6. p. 159, 0; Quart. Journ. v. viii. p. 75, 6.

[3] See Inquiry, ch. 8. In connexion with the nervous hypothesis of animal heat, I may mention the singular speculation of Peart; he supposes that "animal heat is a compound, produced by a mixture of the nervous fluid with pure air;" On Animal Heat, p. 80. He farther informs us, that "the nervous fluid is composed of an earth, united with much phlogiston, and a quantity of ether," p. 111; and that "a quantity of this phlogiston of the nervous fluid unites with the ether of the pure air, sufficient to saturate it, and form that heat which is called animal heat." p. 112.

Nor do I consider Dr. Philip's experiments, in the accuracy of which I place full confidence, to be sufficiently numerous or varied to enable us to draw so important a conclusion from them as that galvanism is an essential agent in the production of animal heat. The legitimate inference from them appears to be, that whatever be the chemical action of galvanism, it has the property of evolving heat from arterial blood; but I presume that the intimate nature of this operation is still unknown. Nor do I think that we are able to explain the facts of a more general nature which have been brought forwards by Dr. Philip and other physiologists respecting the connexion between the nervous influence and the production of heat[1]. I conceive the most probable supposition to be that the capillary arteries are the parts most immediately concerned, both because these are the organs in which the evolution of heat actually takes place, and because they are obviously under the influence of the nervous system[2].

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[1] Hunter, Anim. Œcon. p. 104, remarks, that the power of generating heat cannot "depend upon the nervous system, for it is found in animals that have no brain or nerves." See also Phil. Trans. for 1775, p. 457. Many of the facts brought forward in this paper are curious and interesting, but the author totally fails in establishing his conclusion, that animal heat immediately results from the action of the vital principle, in consequence of his having overlooked many of the accompanying phenomena. An hypothesis has been lately advanced by Sir E. Home, respecting the production of animal heat, according to which it is restricted to the ganglionic part of the nervous system. The doctrine especially rests upon the two positions, that there are certain animals which possess a brain, or some part equivalent to it, but whose temperature is not higher than that of the surrounding medium, while, on the other hand, all the animals that evolve heat are provided with ganglia. The most important facts that are brought forward in this paper are those on the comparative temperature of the two horns of a young deer, in one of which the nerves had been divided, the other being left entire. After some hours the divided horn had its temperature considerably diminished, but in five days it nearly recovered its natural state. The part was examined after the death of the animal, when it was found that "no union had taken place between the divided trunk," the natural conclusion from which, I conceive to be, that the decrease of temperature did not depend upon the division of the nerves, but upon some other circumstance attendant upon the operation. The author, however, formed a contrary conclusion; he remarks, that "it was evident from the recovery of its heat, that some other connexion had been formed between the nerves of the horn and those of the head." I may further observe, that I do not perceive how this experiment bears upon the peculiar hypothesis of the author, respecting the specific effect of the ganglionic nerves in generating animal heat; Phil. Trans. for 1825, p. 257 et seq. Sir Ev. Home subsequently repeated the experiment on the horn of the deer, and found, as before, that its temperature was lowered by the division of the nerve; but that in eight or ten days the effect began to diminish, and ultimately ceased. The nerve being then examined, the divided parts were found to be connected by a newly formed substance; thus, as the author conceives, accounting for the loss of temperature in the first instance, and for its subsequent restoration; Quart. Journ. v. xx. p. 307.

[2] See Blumenbach, Inst. Physiol. § 168 et seq.

CHAPTER IX.

OF SECRETION.

We have now gone through the functions which are more directly essential to the mere continuance of vitality, the circulation and the respiration, functions which cannot be suspended, even for a very short interval of time, without the immediate extinction of life, or a serious derangement of its more important actions. We have now to consider a second order of functions, which are absolutely necessary for our continued existence, but which would appear to be exercised only at certain periods, either when circumstances admit of their action, or, if we regard their final cause, when there is a demand for them in order to supply the wants of the system[1]. These are the three functions of secretion, digestion, and absorption. The first affords the means by which certain parts of the blood are separated from the mass, either to serve some useful purpose after their separation, or to remove some substance which is superfluous or injurious. By the function of digestion the aliment taken into the stomach experiences a series of changes in its constitution and properties, probably by the intervention of certain secreted fluids, which converts it into the substance that seems to be the immediate source of nutrition, while, by means of absorption, the substance thus elaborated is carried from the digestive organs into the blood, where it becomes assimilated to this fluid, and is then transferred to every part of the system, dispensing life and heat, and affording materials for the formation of all the solids and fluids which compose the great machine.

It is obvious that the two former of these functions are so connected together, that it is impossible to give an account of one of them, without presuming upon a certain acquaintance with the other. The secretions cannot be formed until the blood has been already elaborated by the digestive and assimilating processes, while digestion, in its turn, cannot be effected until the stomach has secreted the gastric juice, which is the immediate agent in converting aliment into the materials of the blood. Upon the whole, however, it appears more convenient to begin by considering the function of secretion, as we shall, by this means, be better enabled to judge of the merits of the different hypotheses that have been formed to account for the operations of the digestive organs.

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[1] The old division of the functions into vital and natural essentially depends upon this principle; but there was some inaccuracy in the mode of *applying it*, while the nomenclature is decidedly inappropriate.

In considering the subject of secretion, I shall begin with a few preliminary observations on the nature of the function and on the structure of the secretory organs; I shall next give a brief account of some of the more important of the secreted substances, and shall endeavour to form an arrangement of them. I shall, at the same time, inquire into the mode of their formation considered individually, and shall conclude with the various hypotheses that have been formed to explain the operation.

SECT. 1. *Description of the Organs of Secretion.*

The term secretion, according to its original and primary meaning, is equivalent to separation, and it would appear that this was likewise the technical sense in which it was used by the ancient physiologists, and by the earlier of the moderns, who, for the most part, conceived that the secretions previously existed in the blood, and were merely separated from it, either by mechanical means, or by certain chemical operations, somewhat analogous to precipitation. At present we generally attach a different meaning to the term, and conceive of it as essentially consisting in the production of some change in the secreted substance, either of a physical or chemical nature, proceeding upon the supposition that it did not previously exist in the blood[1]. Perhaps, however, it would be more correct to combine both these ideas in our conception of the process of secretion, and to define it, that function by which a substance is separated from the blood, either with or without experiencing any change during its separation.

But although it may be found convenient, or even technically correct, to extend the term secretion to both these classes of substances, it may still be proper to employ this difference, as the foundation for a subdivision of the secretions, into such as are simply separated and such as are actually formed, as it may be presumed that the separation of the former and the production of the latter will depend upon essentially different operations.

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[1] The late experiments of Prevost and Dumas, where urea was detected in the blood, after the extirpation of the kidney, may, indeed, lead us to the former opinion; they will be more fully considered in a subsequent page. Magendie's definition may appear to favour the idea of mere separation. "On donne le nom generique de secretion à ce phenomène par lequel une partie du sang s'échappe des organs de la circulation pour se répandre au dehors ou au dedans; soit en conservant ses propriétés chimiques, soit après que ses éléments ont éprouvés un autre ordre des combinaisons." Physiol. t. ii. p. 343. Haller simply defines secretion to be "ea corporis animati functio, qua de communi sanguinis massa, alii, et a sanguine diversi, et a se ipsis varii, humores ea lege parantur, ut in qualibet ejus corporis particula idem constanter humor generetur." El. Phys. vii. l. 1. It may be objected to this definition, that the latter clause prevents our admitting those varieties of action which not unfrequently take place in the same organ, in consequence of different circumstances, both external and internal. Cullen's section on secretion in his "Institutions," § 275..285, besides its other merits, is valuable as showing into how small a compass what was really known respecting this function, at that period, might be comprised.

Secretion, in the same manner with all the other operations of the animal œconomy, may be considered as consisting in a vital process, operating through the intervention of certain physical powers; those which we suppose to be concerned in secretion are both mechanical and chemical; the mechanical means employed are usually conceived to be something analogous to filtration or transudation, where a portion of a compound is separated from the remainder, in consequence of the minuteness of its particles enabling it to pass through orifices or pores, which will not allow of the larger particles being transmitted. It is not, however, impossible, that a substance which previously existed in the blood may be separated by a chemical operation, although, in this case, we may generally suspect that the substance separated will have its constitution altered. In the case of those substances which are actually formed, not having previously existed in the blood, it is extremely probable, although not absolutely necessary, that something more than a mere mechanical operation must have taken place; and when a new chemical compound is formed, we may reasonably infer, that it must have been produced by the intervention of a chemical agent. These agents may be of two kinds; they may be either extraneous bodies introduced into the blood, and acting upon some of its elements, or they may consist of some of the constituents of the blood acting upon the other parts of this fluid. We shall find, in our examination of the different secretions, that it is often very difficult to ascertain in which of these two operations the substance in question may originate, or what is the exact nature of the action, even where we have reason to conceive that we know to what class of operations it should be referred. It may be assumed as a general principle, that those secretions which are formed by mere transudation are produced by a much more simple apparatus than those of the other class; and, indeed, in the greatest number of cases, we are not aware of the existence of any appropriate structure. Hence it follows that the secretory organs, which possess a complicated fabric, belong almost exclusively to that class where the substance did not previously exist in the blood, but is actually generated by the process of secretion.

In those cases where we have an organ possessing what is considered as the most perfect structure, or one which consists of the greatest variety of distinct parts, we find a body, which is more or less of a rounded form, and has hence obtained the name of gland. When we examine the intimate structure of glands, they appear to be composed of a number of small arteries, which ramify in various directions through a mass of cellular texture[1]. A part of the blood is returned by corresponding veins, but we also find another set of vessels containing the secreted substance, which generally unite into one or more excretory ducts. The gland usually consists of a number of rounded bodies, which may be detached from each other, and compose what are termed lobes; these are again divisible into smaller and smaller lobes, until we at length arrive at the smallest parts into which it is possible to subdivide them, which have obtained the name of acini. The glands are also provided with nerves, but it is a question which will be discussed hereafter, how far the nervous influence is essential to secretion, or in what way it operates[2].

There is considerable obscurity respecting the intimate structure of glands, and particularly, whether any specific organ intervenes between the secreting arteries and the excretory ducts. Malpighi, who was one of the first anatomists that attended to the minute structure of glands, supposed that there was, in all cases, a cavity or follicle, as it was termed, which was the immediate organ of secretion. Ruysch, on the contrary, who bestowed much care on the investigation of this point, and who particularly excelled in the art of injecting minute vessels, conceived that he had disproved the doctrine of Malpighi, and concluded that there is no intervening organ, but that the artery immediately terminates in the duct, so as to show that the operation of secretion must take place in the artery itself. It is upon the whole probable that neither of the opinions is absolutely correct, but that different glands possess different structures in this respect, and that upon the presence or absence of the follicle, a part of the specific effect produced by the different glands may depend[1].

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[1] It is stated, the Prof. Müller, of Bonn, has discovered that the glandular structure essentially consists in a duct, with a closed extremity, on the parietes of which plexuses of blood-vessels ramify, from which vessels the secretions are immediately produced; Elliotson's Physiol. p. 92, 3; Kiernan, in Phil. Trans. for 1833, p. 713. We have an elaborate dissertation on the structure of glands by Dr. Graves, in the Dublin Med. Journ. No. 1. p. 42 et seq.

[2] For an account of the structure of glands, I may select the following authors, as those who have given a summary of the more modern discoveries and opinions: Cheselden, Anatomy, p. 146, 7; Winslow, Anat. sect. 10. § 575 et seq.; Boerhaave, Prælect. t. ii. § 240 et seq.; his account is more physiological than anatomical; Haller, El. Phys. vii. 2; Sabatier, Anat. t. iii. p. 342 et seq.; Dumas, Physiol. t. ii. p. 20, 1; Bichat's section, "systême glanduleux," Anat. Gen. t. ii. p. 598 et seq., contains much that is ingenious, but as usual, in some parts, is hypothetical, and the expressions obscure and mystical; Blumenbach, Inst. Phys. § 468 et seq.; Monro (Tert.) Elem. v. ii. c. 3; the article "Gland," in Rees's Cyclop., may be perused with advantage. Nuck's elaborate treatise entitled "Adenologia," being especially intended to illustrate the structure of the lymphatic glands, will be more properly considered in a subsequent chapter; I may, however, remark in this place, that he commences by giving us a list of all the proper secretory glands, which are found in the body, amounting to 53 different sets, or attached to as many different organs.

It is necessary to remark, that it is comparatively in but a few instances, that we meet with a complete glandular apparatus; in some cases the proper glandular structure appears to be altogether wanting; in some we have a pouch, or cavity, in which the secretion is lodged, being with or without an excretory duct; while, in other instances, there would seem to be neither the cavity nor the excretory duct, but where the secreted substance is simply poured out upon the surface of a membrane, whence it is removed without any specific apparatus[1]. There are likewise many examples, where a substance that did not previously exist in the blood is separated from it, and conveyed to its appropriate destination, but where we can perceive neither the organ by which it is produced, nor that by which it is afterwards deposited. It is also to be observed that in those cases where we are able to detect the secretory apparatus, its magnitude and structure, as far as we can judge, bear no relation to the change which is produced upon the matter secreted; we observe, for example, a substance, which apparently differs but little from the blood or any of its constituents, to be formed by a very complicated organ, while an organ, apparently of the most simple kind, is employed in producing a body of totally new properties.

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[1] This controversy occupied the attention of the most learned anatomists and physiologists during the latter part of the 17th, and the commencement of the 18th centuries. Malpighi seems to have first published his opinion in the year 1665; it was pretty generally adopted, until Ruysch called it in question in 1696. Malpighi is admitted to have been one of the most learned and correct anatomists of his age, and particularly excelled in the investigation of the minute structure of parts. Haller thus describes Ruysch; "etsi neque ingenii velocitate valde eminuit, neque assiduitate legendi, aut eruditione, frequentissima tamen cadaverum consuetudine, et opportunitate ad consulendam naturam, et dissectionibus penè per totos octaginta annos continuatis, et artifice etiam manu, plerumque mortales superavit, eoque majorem auctoritatem sibi comparavit, quod ab hypothesi alienior, parum ultra ea doceret, quæ viderat." El. Phys. vii. 2. 8. We have a very full detail of the successive steps of the discussion in the El. Phys. vii. 2. 6..14; Haller entitles the last of these sections, "causa Ruyschiana vincit." See also Boerhaave, Præl. § 247, 257 cum notis. Those who are disposed to consult Malpighi and Ruysch in the original, may find a detail of their opinions and discoveries in Malpighi, Exerc. de Struct. Viscer. cap. 2, 3, containing an account of the liver; Epist. ad Reg. Soc. Lond. de Struct. Gland. written in 1688, and in Op. post. p. 101. Ruysch's account of his opinions is contained in his Epistle to Boerhaave de Fab. Gland. He informs us, p. 65, that at a very early period of his life he examined the structure of the mesenteric glands by means of fine tubes, that were made by Musschenbroek, through which he inflated them with air from the lacteals; in p. 81, we have a small plate of the mesenteric vessels and the connected glands, composed of their minute terminations. For the particular illustrations which he gives of his opinions, see Epist. probl. 4. ad Campdomercum, de liene, p. 6; Epist. probl. ad Grœtz, de mam. struct. &c. p. 7; Adver. Anat. dec. 1. § 4. p. 13; Thes. Anat. 6. No. 3. not. 2. p. 18. on the small glands of the nose; No. 33. not. 1. p. 28. on the glands of the stomach; No. 73. p. 38. on the cortical substance of the brain; Thes. Anat. 8. No. 34. p. 13, 4, in describing the vessels of the placenta, he takes occasion to state his sentiments respecting the structure of glands generally; Thes. Anat. 10. No. 61. p. 14, on the mesenteric glands; Thes. Anat. max. No. 118, 9. p. 17, 8. on the glands of the mesentery and stomach. Boerhaave's Epistle to Ruysch, in answer to the one referred to above, is a very interesting specimen of the learning and candour of the writer. In p. 36, he acknowledges his conviction of the force of Ruysch's reasoning, "Fateor," says, "hoc tam forte mihi videri, ut fere nesciam, an effugere quis possit vim hujus argumenti." He refers to an experiment in which an injection had been passed directly from the vena portæ into the pori hepatici, without passing through any intervening follicle. Again, in p. 37, he observes, "Ars itaque tua certo nos docet, in multis, forte et in omnibus, corporis humani partibus ita disponi extremas canales ortas ab arteriis sanguiferis, ut directe, absque interposita glandulosa machina, desinant in apertos meatus, &c." We have a perspicuous account of this controversy in Bell's Anat. v. iv. p. 16 et *seq.* See also Mr. Kiernan's paper on the liver, to which I shall have occasion to refer more particularly, in a subsequent part of this chapter.

Glands have been arranged in various ways, according to their anatomical structure, and their supposed physiological uses; but it may be doubted whether any real advantage has accrued from these arrangements, or whether there can be said to be any natural foundation for them. Thus the division of glands into conglobate and conglomerate[2], which was generally adopted by the older writers, the former consisting of only one lobe, the latter of a number of lobes, connected together by cellular substance, it would be difficult in many cases to adhere to, and would lead to no practical advantage. And the same observation may be made upon the division of glands into secretory and excretory, the former producing a substance which serves some immediate useful purpose in the system, while the object of the latter is to separate something from the blood which is useless or noxious, and is accordingly rejected as soon as it is formed. For though there are various bodies which are obviously intended for the first of these objects, and there are some which appear almost certainly to be confined to the latter, yet there are many which it is difficult to say in which division they ought to be placed, and some, perhaps, have an equal claim to either class, being, in the first instance, secreted from the blood as injurious to the system, and yet after their separation, and before they are finally discharged, serving some useful purpose. Probably the separation of carbon from the blood in the lungs may be a case of this double action, where the excess of carbon in the system is positively noxious, at the same time that the process of separation may be so conducted, as to act in a very important manner upon the animal œconomy. Upon the whole then, it will appear, that there is no arrangement of the glands which can be of any considerable use in explaining the mode of their action, or in throwing any light upon the nature of the substances which they produce.

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[1] Dr. Young enumerates the following among the different structures that are employed in secretion; exhalent vessels, tubular glands, conglomerated glands, follicles, pores, parenchymatous glands; Med. Lit. p. 110. Adelon reduces the structures that are concerned in secretion to three; exhalents, follicles, and glands; Physiol. t. iii. p. 438 et seq.

[2] The division of glands into conglomerate and conglobate appears to have been made by Sylvius; Disput. Med. 3. § 25, 6, 7. Op. p. 11; the former designating the more compounded structure, which is found in certain of the secreting organs, the more simple or conglobate glands being technically restricted to those which are connected with the lymphatic system. See Haller, El. Phys. ii. 3. 16; vii. 2. 2.

We shall probably find it almost as difficult to make any arrangement of the secretions themselves as of the organs which produce them; yet before we proceed to consider the properties of so numerous a class of substances, it would be very convenient to be able to dispose them into groups or classes, the individuals composing each of which might bear some relation or have some reference to each other. One of the oldest arrangements of the secretions was into recrementitious and excrementitious, a division which corresponded to the secretory and excretory glands respectively, and of course liable to the same objections, besides its being neither sufficiently comprehensive nor sufficiently minute to be of any real utility. As the knowledge of physiological science advanced, other classifications were proposed, constructed upon more technical or scientific principles, generally referable either to the supposed chemical or mechanical properties of the substances, according to the tenets of those by whom they were respectively advanced. Haller adopted the chemical method, and classed the secretions under the four heads of aqueous, mucous, gelatinous, and oily[1], a classification obviously too general to throw much light upon the nature of the substances concerned. Fourcroy also employed the chemical method, and made the more elaborate arrangement of the secretions into the eight classes of hydrogenated, oxygenated, carbonated, azotated, acid, saline, phosphated, and mixed[2]. But although this arrangement was more scientific than any which had preceded it, and, in some measure, corresponded with the improved state of modern chemistry, it may be questioned whether it was not rather formed upon theoretical principles, than upon the actual nature of the substances concerned.

Before I attempt to form a new arrangement, there is a question to which I have already alluded, and upon which it will be necessary to decide, whether we are to consider those substances as secretions, which appear to exist ready formed in the blood, and which are separated from it, as far as we can perceive, without any appropriate or specific apparatus, such for example as the muscular fibre. This substance in all its chemical, and we may perhaps add, its mechanical properties, resembles the fibrine of the blood, and we cannot doubt that the fibrine is, by some means, separated from the mass of blood, and deposited in the situation in which we find it to exist, as a constituent of the muscles; yet we are altogether unable to trace the intermediate steps of the operation. The same kind of remark nearly applies to some of the fluids, especially those which are the result of morbid action. The various dropsical fluids, for instance, seem to have the same constitution with the serum, except in the proportion of water which they contain, and it would appear that they are separated from the blood by mere transudation through a membrane, a process analogous, or very similar to filtration. Upon the whole, I conceive it will be more convenient to regard all these substances as secretions, whatever may be their relation to the blood or to any of its constituents, and whatever we may conceive to have been the mode of their formation.

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[1] El. Phys. vii. 1. 2.

[2] System, v. ix. p. 159. Richerand gives us a different arrangement as the one proposed by Fourcroy, Physiol. p. 235; that in the text being taken from his great systematic work, we may regard as the result of his more matured reflection; Richerand does not inform us in which of *Fourcroy's* works the arrangement is contained which he has adopted.

Another question of a nature not very different from the above, respects certain substances, which are found both in the blood and in some of the secretions, but concerning the origin of which there is great uncertainty, whether they are originally received into the stomach along with the aliment, and pass into the digestive organs without undergoing any change, whence they are taken into the blood and again separated from it, still without any alteration; or whether there be a provision made for their formation in some part of the system. The doubt that arises on this point, depends principally upon the difficulty which we have in conceiving of any way by which they could be produced from the elements that enter into the composition of the blood, as unless we admit of the operation or existence of new affinities, or affinities totally different from those which we observe in any other natural objects, we cannot suppose them to be actually generated in the system; at the same time there are perhaps equal, or even greater difficulties, in the supposition that they are introduced into the system ab extra. This question will more particularly fall under our consideration in the following section, where the evidence will be examined on which each opinion rests; at present I shall only remark, that it appears more convenient to consider all these substances as secretions, because, in whatever manner they may be introduced into the blood, we find that they actually exist there, and are probably removed from it by the secretory organs.

I think there can be little doubt, that the only method of arranging the secretions, which can be of any use in giving us an insight into their nature, and the relation which they bear to the blood, must be founded upon their chemical composition; and although from our imperfect knowledge on this subject, such a mode of classification must be likewise necessarily imperfect, yet as there is no mode of arrangement to which a similar objection might not be urged, I shall not hesitate to make the attempt.

But there are certain technical difficulties which meet us at the outset. The secretions many of them consist of substances composed of a number of ingredients, possessed of different properties, where it may not be easy to decide which ingredient predominates, or gives its peculiar qualities to the compound. We shall also find that if we examine a series of secreted fluids, we shall perceive that they all closely resemble each other in what may be termed their specific properties, yet that those at each extremity of the series may so far differ from that which was adopted as the standard or type of the rest, that the resemblance may be more nominal than real, and the difference may at length proceed so far, that they may even bear a nearer resemblance to some other class than to that in which they are placed. Another circumstance, which causes considerable embarrassment in a chemical arrangement of the secretions, is, that the same gland, in different states of the system, produces substances of a very different nature, and when the affection amounts to the degree which constitutes disease, entirely new substances are frequently formed, unlike any thing which previously existed; and which, although they must be regarded as altogether morbid effects, yet they are strictly entitled to the appellation of secretions, and which indeed, had we a complete knowledge of the subject, would form the most interesting object of our investigation, by making us acquainted with the nature of the morbid action which had taken place, and even the amount of the deviation from the standard of health. This, however, in the present state of our knowledge, we are quite unable to accomplish. In order to take a complete view of the subject, it would be necessary to examine the secretions in all their various states, to mark the gradations from the healthy to the morbid condition, and to observe what new characters were assumed in each of them.

Taking into account all these circumstances, and bearing in mind that the present state of our knowledge is confessedly imperfect, it will be sufficiently evident, that any arrangement which I can propose must be necessarily incomplete; but I am not, on that account, deterred from making the attempt, because, if the method itself be fundamentally correct, even an imperfect view of it will be useful, by teaching us what parts require further elucidation, and instructing us in the best method of accomplishing it. The classes into which I propose to arrange the secretions, are the eight following; the aqueous, the albuminous, the mucous, the gelatinous, the fibrinous, the oleaginous, the resinous, and the saline[1].

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[1] I shall insert in this place the arrangements of the secretions that have been proposed by some of the most eminent of the modern physiologists.

SECT. 2. *Account of the Secretions.*

The first class of secretions, the aqueous, are those that consist almost entirely of water, where the properties of the

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Sabatier and Boyer, with most of the French anatomists, adopt the division of the secretions into recrementitious and excrementitious, to which they generally add an intermediate class. Boyer places the following among the recrementitious humours, as he styles them; blood, lymph, jelly, fibrous matter, fat, marrow, matter of internal perspiration (serous transudation) and the bony juice; among the excrementitious, the matter of insensible perspiration, sweat, discharge from the nose, ears, and eyes; and in the third or intermediate class, tears, saliva, milk, bile, pancreatic juice, and semen; Anat. t. i. p. 8, 9. Magendie divides them into exhalations, follicular secretions, and glandular secretions, but he candidly acknowledges the imperfection of his method; each of the classes is subdivided into numerous species; Physiol. t. ii. p. 343 et seq. Plenk divides the humours of the body into crude, sanguine, lymphatic, secreted, and excrementitious; the secreted fluids he arranges under the heads of milky, watery, mucous, albuminous, oily, and bilious; Hydrol. p. 31, 2. Richerand arranges them into the six classes of saline, oily, saponaceous, mucous, albuminous, and fibrinous; Physiol. § 88, p. 235. Blumenbach makes the following classification; milk, the aqueous fluids, the salivary, the mucous, the adipose, and the serous, while the semen and the bile are supposed to be substances sui generis; Inst. Phys. § 467. Berzelius adopts the old division into secretions and excretions, a division which is founded rather upon the final cause of their formation, than upon their properties, or the mode of their production; he remarks, however, that the secretions are all alkaline, while the excretions are acid, a remark which, I conceive, will scarcely be found to apply in all cases; Med. Chir. Tr. v. iii. p. 234. See the remarks of Donné on the nature of the secretions, whether acid or alkaline; Ann. Chim. et Phys. t. lvii. p. 400 et seq. Dumas classes the secretions in four divisions, according to the more or less simple structure of the organs which produce them; those that are formed without any specific organ, by the most simple organ, by a gland, and by the complete secretory apparatus; Physiol. t. ii. p. 15..8. Dr. Young arranges the secreted fluids into the classes of aqueous, urinary, milky, albuminous, mucous, unctuous, and sebaceous; Med. Lit. p. 109. The writer of the article "Anatomy," in Brewster's Encyclopedia, has drawn up the following arrangement of the secreting and excreting organs, with the fluids which they produce, which is valuable as pointing out the relation which exists between them. They are first divided into secreting surfaces, and secreting organs. Of the surfaces we have three divisions; 1. Those which separate matters already formed in the blood, viz. the serous, producing serum or coagulable lymph, and the cellular, producing serum and fat; 2. Those which separate from the blood matters that are little changed, viz. synovial membranes, forming synovia, and mucous membranes, forming mucus; 3. Excreting surface, viz. the skin, giving out the matter of perspiration. The secreting glands are arranged under the four heads of such as are attached to the organs of sensation, those of digestion, those of reproduction, and glands that are partly secretory and partly excretory. Under the first head we have the papillæ of the tongue, which secrete a watery fluid, the ceruminous glands, which secrete the ear wax, and the lachrymal, which secrete the tears. Under the second head we have the parotid, submaxillary, and sublingual glands, which secrete saliva, the pancreas, which secretes its peculiar juice, the spleen, to which no secretion is assigned, and the liver, which produces the bile. Under the third head we have the testes, prostate gland, and the mammæ, which respectively secrete the seminal fluid, the prostatic fluid, and the milk; and under the fourth head, we have the kidneys, which produce the urine, and the renal glands, which produce a substance depend upon its watery part, or when any other ingredient which it may contain is in too small a quantity to give it any specific characters. The only two secretions which fall under this class, are the cutaneous perspiration and the aqueous exhalation from the lungs. Of the cutaneous perspiration I have already given some account in the last two chapters, where I have stated, that under ordinary circumstances, a portion of water is exhaled from the surface of the body in the form of an invisible vapour; but when its quantity is by any means increased, it assumes the state of a fluid, and is collected in drops on the skin. It does not appear that its chemical nature is different in these two states, although it is not very easy to decide absolutely on this point, in consequence of the difficulty of obtaining any quantity of it when in the state of vapour. The sensible perspiration may be procured in sufficiently large quantity, and has been examined by several eminent chemists, but except the water, it seems doubtful whether any of the ingredients that have been detected in it are essential to its nature. It appears probable that the perspiration differs considerably according to the states of the system, not only as affected by various morbid actions, but from internal causes, or the effect of internal agents upon it, and there is likewise reason to believe, that it may be habitually different in different individuals. Upon all these points, however, it must be confessed that we have no very accurate information, as the attention of those who have examined this substance has been almost exclusively confined to ascertain its quantity, and the pathological effects which have been supposed to be the result of its discharge from the system. There is, however, reason to suppose, that these have been very much exaggerated, and that many diseases which were conceived to depend upon the suppression of the

blackish fluid; vol. i. p. 830, 1. In addition to the eight classes of secretions which are enumerated above, I am disposed to think that we might with propriety admit a ninth class of aeriform fluids, of which the air in the swimming bladder of fishes may be adduced as an example; upon strictly technical principles, the air of expiration may be placed in the same division. See remarks of Dr. Baillie; Works by Wardrop, v. i. p. 69 et seq. Perhaps we ought to refer to the head of aeriform secretions certain facts, which appear to rest upon unexceptionable authority, where gases, which may be termed morbid, are secreted or excreted from the blood. It appears that gases of an inflammable nature have been occasionally deposited in the cellular texture, giving rise to a peculiar kind of emphysema, of which an instance is recorded by Bally, in Magendie's Journ. t. xi. p. 1 et seq. He conjectures, as I conceive, with some plausibility, that to this circumstance may be ascribed the cases of spontaneous combustion, which were, for some time, generally discredited, but to the truth of which, however wonderful, it appears impossible to withhold our assent. For a well digested account of the facts and opinions on this subject, I may refer to the art. "Spontaneous Combustion," by Dr. Apjohn, in the Cyclop. of Med., also to the articles by Breschet, in Dict. de Méd. t. v. p. 473 et seq., and by Devergie, in Dict. Méd. Chir. prat. t. v. p. 367. We have a distinct treatise on the subject by Lair. "Sur les Combustions humaines," which may be consulted with advantage.

cutaneous perspiration, are owing to an entirely different cause, of which the peculiar condition of the skin was only one of the effects or symptoms.

There are many facts that appear to prove, that the skin emits a peculiar odorous matter, by means of which dogs, and such animals as possess a delicate scent, are enabled to detect the presence of other individuals, or to trace them out for long distances. We have, perhaps, no decisive means of ascertaining whether this odorous effluvium depends upon the perspiration itself, or upon some other secretion which is mixed with it, and discharged from the body along with the perspirable matter. Upon the whole, however, it is probable that they are distinct substances, that the proper matter of perspiration is produced from every part of the surface, and is nearly or altogether without odour, while there are certain parts of the body which are provided with glands, that secrete a peculiar or specific substance, which composes the odoriferous effluvium. This latter would appear to be of an oily nature, and will therefore belong to a different class.

The perspirable matter, in the purest state in which we are able to procure it, seems to have been first examined by Berthollet[1], and afterwards by Fourcroy[2], but the most elaborate analysis is that of Thenard. He considers it to be essentially acid, and supposes that the acid is the acetic; it contains an appreciable quantity of muriate of soda, and perhaps of potash, with traces of the earthy phosphates, and of oxide of iron; there also appears to be a very minute quantity of an animal matter[3]. The matter of perspiration has been still more recently examined by Berzelius, and with results considerably different from Thenard's. He indeed supposes it to contain a free acid, but this he conceives to be the lactic, accompanied with the lactate of soda, together with the muriates of potash and soda, and a minute quantity of animal matter[4]: it appeared, indeed, to be identical with the substance which Berzelius had announced as existing in the serosity of the blood, and many other of the animal fluids.

It may be reasonably doubted whether the aqueous exhalation from the lungs should be considered as an immediate secretion from the blood. I have already made some remarks upon its origin, and have stated that I conceive it, upon the whole, more probable that it proceeds merely from the aqueous part of the mucus which is evaporated from the surface of the pulmonary vesicles, than that it is a distinct or separate secretion. So far as its chemical constitution has been examined, it appears to be the same with the cutaneous transpiration, and to consist of water, perhaps, holding in solution minute portions of saline or animal matter, but we have no very certain information respecting either their quantity or exact nature[1].

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[1] Journ. de Phys. t. xxviii. p. 275.

[2] System, v. ix. p. 280 et seq. He informs us that Vauquelin and he discovered urea and phosphate of lime in the perspiration of horses; p. 289.

[3] Chimie, t. iii. p. 712.

[4] Thomson's Ann. v. ii. p. 415; Med. Chir. Tr. v. iii. p. 256, 7; Ann. Chim. t. lxxxix. p. 20. See also Thomson's Chem. v. iv. p. 547 et seq.; Henry's Elem. v. ii. p. 434; Ure's Dict. Art. "Sweat." Since writing the above, I have had an opportunity of examining the fluid of perspiration; an account of the experiments is contained in the Med. Chir. Tr. v. xiv. p. 424 et seq. I may remark upon this case, that as the subject from which the fluid was taken was labouring under disease, and as the fluid was morbidly increased in quantity, its properties may not improbably have been likewise affected.

The second class of secretions, the albuminous, constitute a very numerous and important series of substances, some of which are in the solid, and others in the fluid form. All the membranous, or white parts of animals, as they have been termed, consist essentially of albumen, which appears, from the experiments of Mr. Hatchett, to differ from the albumen of the blood only in being detached from the greatest part of the extraneous matter with which it was united, and in being in a coagulated state[2]. We have also a considerable number of fluid albuminous secretions; the surfaces of all the close cavities of the body, such as the thorax, the abdomen, the pericardium, the ventricles of the brain, and even the interstices of the cellular substance, are continually secreting a fluid, which seems to differ from the serum of the blood principally in containing a much smaller quantity of albumen. There are many morbid conditions of the body, in which these fluids become preternaturally increased in quantity, sometimes to a great extent, so that we have an opportunity of examining them with great accuracy. Many chemists, both on the continent and in this country, have applied themselves to this investigation, and it is clearly ascertained that they consist of a certain quantity of albumen, which may be regarded as what gives them their essential character, of another animal matter similar to that found in the serosity of the blood, and of the same neutral and earthy salts which we find in that fluid. It would appear that the salts and the additional animal matter are nearly in the same proportion in all cases, while the proportion of the albumen is varied from a quantity nearly equal to that in the serum of the blood, to one almost too small to be recognized even by the most delicate tests[3]. This, therefore, affords an instance of that inconsistency, to which all attempts at arrangement are liable, where we place a secretion in the class of albuminous, although the smallest quantity only of albumen enters into its composition.

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[1] We are informed by Magendie, in his Mem. on Transpiration, that Chaussier has proved that the vapour from the lungs contains a quantity of animal matter, by keeping a portion of it in a close vessel exposed to an elevated temperature; upon opening the vessel a very evident putrid odour was exhaled from it; p. 16.

[2] Phil. Trans. for 1800, p. 399 et alibi. See p. 27 et seq. of this work.

[3] Berzelius, Ann. Phil. v. ii. p. 384, 5, and Med. Chir. Tr. v. iii. p. 251 et seq.; Henry's Elem. v. ii. p. 431, 2; Thomson, Chem. v. iv. p. 528; Thenard, Traité, t. iii. p. 683, 4; p. 686, 7; Magendie, Physiol. t. ii. *p. 344..6.*

The morbid albuminous fluids, which we have the most frequent opportunities of examining, are those from the abdomen, from the ventricles of the brain, from the pericardium, from the cavity of the spine, of that from the testicle, and from the cellular texture generally[1]. As a general rule, the fluid from the cavity of the abdomen contains the greatest proportion of albumen, and that from the brain, the least, but there are many exceptions to it[2]. We also find that the fluid from the same part contains more or less of the animal matter according to the states of the constitution, the rapidity with which the deposition of matter is made, the length of time in which the fluid has remained in the cavity, and probably from other circumstances; but I do not find that we are able to lay down any general principles which are applicable in all cases. By comparing together a considerable number of experiments, which I have performed at different times on fluids of this description, I have been led to conceive, that the variation in the quantity of the albumen is much greater than of the other ingredients, so that while in certain of these fluids, as for example, in that of hydrocephalus, it is not more than one-third or one-fourth of what is in the fluid from ascites, the quantity of the saline contents, and of the uncoagulable animal matter are nearly the same[3], Nor is it in its quantity or proportion alone that the saline matter of these fluids resembles that of the blood; in various instances where it has been examined, it would appear to be similar to it in its composition, and particularly in the circumstance of its containing uncombined soda. This salt is so generally found in those fluids, which in other respects exhibit the albuminous properties, that it would appear to be in some way necessarily connected with, or essential to them, while, in the fluids possessing other physical characters, and which do not contain albumen, we never find any indications of a free alkali[1]. With respect to the formation of the albuminous secretions, we have no knowledge of any appropriate organ by which they are produced; and as they so exactly resemble the serum of the blood, except in the proportion which there is between the water and the solid contents, it may be fairly questioned, whether they are not formed simply by filtration or transudation; still, however, according to the view which I have taken of the nature of secretion, this will not exclude them from the list of secreted substances.

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[1] Marcet, in Med. Chir. Tr. v. ii. p. 340 et seq.; Bostock, in ditto, v. iv. p. 53 et seq.

[2] In a very remarkable case of chronic hydrocephalus, which occurred lately in Guy's Hospital, the fluid was not only in extraordinary quantity, but contained an unusually large proportion of solid contents. I examined a portion of it, with which I was favoured by Mr. Aston Key, and found the proportion, both of the animal and of the saline ingredients, to be very much more than is usually present in fluids of this description, so as to be nearly double the average quantity. How far this peculiarity belongs generally to the disease in its chronic form, is a question which, I believe, the present state of our knowledge does not enable us to answer. For an account of this case see Bright's Med. Rep. v. ii. p. 431 et seq. See also an analysis of a similar kind of fluid by Baruel, in Magendie's Journ. t. i. p. 95.

[3] See the paper referred to above, and more particularly the synoptical table, p. 73.

We now come to the third class of secretions, the mucous, which differ from the aqueous and the albuminous in this essential particular, that whereas the two former appear to consist of substances that are merely separated from the blood, the essential character of the mucus depends upon a substance, which did not exist in the blood, but which is formed by the action of the gland. We accordingly find, that in the case of these secretions, we are generally able to demonstrate the organ by which they are produced, and that some of the mucous glands are among the most elaborate with which the body is furnished[2]. The mucous secretions are distinguished by their viscidity, or their capacity of being drawn out into threads, and by being with difficulty soluble in water, although they are already united to a considerable quantity of it. The animal matter which forms the basis of the mucous secretions, and which gives them their essential characters, appears in many of its chemical relations to resemble albumen in the coagulated state, so that we are led to suppose, that at least one effect of the mucous glands consists in the coagulation of the albumen of the blood[3]. Another circumstance which characterizes the mucous secretions, and especially distinguishes them from the albuminous, is the nature of the salts which they contain, for whereas the latter are very similar to those in the serum, and resemble it in containing uncombined soda, the salts in the mucous secretions are in the neutral state[4].

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[1] Mr. Brande, proceeding partly upon this circumstance and partly upon his discovery of the effect of the galvanic apparatus in coagulating albumen, considers it, while in the liquid state, as essentially a solution of this peculiar animal matter in alkali, and attributes its coagulation to the subtraction of this alkali by the negative end of the interrupted circuit; Phil. Trans. for 1809, p. 377. I have already given my reasons for conceiving that this hypothesis cannot be maintained; Med. Chir. Trans. v. ii. p. 173, 4.

[2] Bichat considers the fluids produced from serous membranes as only exhalations, while those from mucous membranes are properly secretions; Traité des Membranes, p. 5, 6. Magendie also, who makes a distinction between exhalations and glandular secretions, places some, at least, of the mucous fluids in the latter class; Physiol. t. ii. p. 360 et seq. According to the test proposed by Berzelius, see above, p. 481, the mucous fluids held a kind of intermediate rank between the secretions and the excretions.

[3] Ed. Med. Journ. v. ii. p. 44, 5; Nicholson's Journ. v. xiv. p. 149.

[4] Mr. Brande informs us that the saliva is not alkaline; he finds, however, that the galvanic apparatus separates albumen from it in the coagulated state,

The mucous secretions differ from the albuminous in their seat, as well as in their composition; the albuminous are lodged in the close cavities of the body, while the mucous are always found in those cavities or passages that have a communication with the atmosphere; such as the mouth, the nose, the œsophagus, the stomach, the alimentary canal, the bladder, the trachæa, and the air vesicles of the lungs. In many of these parts we find a glandular apparatus, which is often peculiarly large and elaborate in its construction, as for example, in the gland which secretes the saliva; but there are other cases, where a substance that appears very nearly to resemble the saliva, is formed without the intervention of any glands that we are able to detect.

All the mucous membranes, as they are termed, secrete a fluid which appears to be nearly similar in the various parts of the body; and to the same class we refer the saliva[1], and the gas-

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as it does from the alkaline serous secretions; Phil. Trans. for 1809, p. 374 et seq.

[1] As the saliva may be easily procured in considerable quantity, it has been frequently made the subject of chemical analysis. An account of what had been done on the subject by the earlier physiologists will be found in Boerhaave; Prælect, t. i. § 66; and in Haller; El. Phys. xviii, 2, 10. A correct detail of the modern discoveries is contained in the Systems of Dr. Thomson, v. iv. p. 515 et seq.; and of Dr. Henry, v. ii. p. 409. In some experiments which I performed on saliva in the year 1805, I conceived that I had detected in it two kinds of animal matter, one composing the soft masses, and giving it its consistence and physical characters, nearly similar to coagulated albumen, the other dissolved in the water of the saliva, along with the salts, and resembling the serosity of the blood; Ed. Med. Journ. v. ii. p. 44, 5; and Nicholson's Journ. v. xiv. p. 149. Dr. Thomson agrees with me in thinking that the former of these substances exhibits the properties of coagulated albumen; System, v. iv. p. 517. Berzelius has more lately examined saliva; he also supposes that it contains two kinds of animal matter; the one which he styles mucus appears to be the same with what I conceive to be coagulated albumen; he likewise supposes that there is a relation between some of the component parts of the saliva, and the serosity of the blood, although not of that nature which I had announced. His analysis of the saliva is as follows:

Water	992·9
Peculiar animal matter	2·9
Mucus	1·4
Alkaline muriates	1·7
Lectate of soda and animal matter	0·9
Pure soda	0·2
	1000·0

Ann. Phil. v. ii. p. 379, 0; Med. Chir. Tr. v. iii. p. 242..4; View of Animal Chem. p. 61, 2. We have a still later account of the composition of the saliva by Tiedemann and Gmelin, who, in the course of their researches on digestion, performed a series of experiments on this secretion, which led to some curious and important results. The solid contents of the saliva are stated to vary from 1 to 25 per cent.; there are three proximate animal principles which appear to be essential to it, proper salinary matter, mucus, and osmazome, to which, in some cases, is added a little albumen, and a little fatty matter containing phosphorus. The salts are numerous, being no less than tric juice[1], although we can scarcely conceive, but that this latter must contain some ingredient besides its mucous part, to which it owes its peculiar property of acting upon the aliment taken into the stomach: but this point will be more fully considered in the next chapter.

It is somewhat doubtful whether the tears should be referred to the class of albuminous or of mucous fluids. An analysis was made of them by Fourcroy and Vauquelin, which, considering the period when it was performed, must be regarded as very accurate; and as from this we learn that they contain an uncombined alkali, we might be induced to place them among the albuminous secretions. It appears, however, that besides albumen, some other animal matter enters into their composition, which gives them their specific properties, and which, as far as we can form an opinion from the detail of the experiments, resembles mucus, and would therefore entitle them to be placed among the mucous secretions[2]. This opinion appears

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nine; six soluble in water, and three insoluble. The soluble salts are an alkaline acetate, carbonate, phosphate, sulphate, muriate, and sulpho-cyanate; it is to the second of these that the saliva owes its alkaline properties, and which had been generally ascribed to the presence of an uncombined alkali. The authors announce the curious circumstance, that the alkali which exists in these various salts is, in man, almost solely potash, while in the dog and the sheep it is soda, with very little potash. The presence of the sulpho-cyanic acid is likewise a curious circumstance, but of the reality of which the professors entertain no doubt; it is most abundant in the human saliva, and is scarcely perceptible in that of the dog. The insoluble salts are the phosphate of lime, the carbonate of lime and magnesia, in very minute quantity; Recher. sur la Digestion, par Jourdan, p. 23, 4. We have also an account of the saliva by Leuret and Lassaigne, contemporaneous with that of Tiedemann and Gmelin, and which, in like manner, contains the result of their experimental researches; the conclusions which they deduce from them are, however, in many respects very different. Leuret and Lassaigne, for example, state, that the chemical properties of the saliva are essentially the same in all animals; Recher. Physiol. et Chim. &c. p. 33; whereas we are led to suppose that a very remarkable difference exists with respect to the saline ingredients; and with respect to the animal matter, while Tiedemann and Gmelin suppose that it consists of three distinct substances, Leuret and Lassaigne unite all these together under the denomination of mucus. I shall refer those of my readers who take an interest in tracing the progress of knowledge, to the account of the saliva which is given by Baglivi; he examined the action of various chemical re-agents upon it with considerable accuracy, and discusses with much ingenuity, although not always very correctly, its supposed effect in the process of digestion; Dissert. 2. Circa Salivam; Op. p. 412 et seq.

[1] The accounts which had been generally given of the gastric juice were that it possessed no properties which were not common to all the mucous secretions, and especially that it was neither acid, nor alkaline. We have, indeed, occasional observations by different chemists, who asserted that they had detected an uncombined acid in it, but this was supposed to be accidental, or to be owing to some morbid cause, until Dr. Prout announced that, during digestion, the contents of the stomach were essentially acid, and that this acid was the muriatic; Phil. Trans. for 1824, p. 45 et seq.

[2] Journ. de Phys. t. xxxix. p. 256 et seq. It would appear from Berzelius's analysis of the humours of the eye, that their chemical constitu-

to be confirmed by the nature of the organ which produces them, which is not a membranous surface, but is a part possessed of a proper glandular structure.

A considerable part of the seminal fluid appears to be mucus, although both from its physiological and its physical properties it would seem to contain something of a peculiar or specific nature, upon which we may presume that its appropriate action more especially depends. What is commonly styled the pancreatic juice, is described to be a substance resembling saliva, and we are informed that the anatomical structure of the pancreas is very similar to the salivary glands of the fauces; but I believe that we have no very accurate information on either of these points[1].

tion might induce us to place them in this class, rather than in the albuminous, although they do not possess the physical properties of the mucous fluids. The "peculiar matter," which forms between 35 and 36 per cent. of the crystalline lens, as far as its properties are detailed, appears to differ from every other animal substance with which we are acquainted; Med. Chir. Tr. v. iii. p. 254; Ann. Phil. v. ii. p. 386. Fourcroy and Vauquelin also examined the nasal mucus, and the result of their examination was, that it very nearly resembles tears in all its chemical relations; they particularly state, that it contains uncombined soda, p. 359; but this does not agree with my own experience.

[1] De Graaf's Treatise on the Pancreas, Tract. Anat. Med. &c. may be referred to, as the production of one of the most eminent anatomists of the 17th century, giving an account of all that had been discovered or imagined upon the subject. We may presume that the descriptions are correct, but the work is extremely diffuse, and contains a large portion of physiological and pathological hypothesis, which is now entirely superseded. See also Boerhaave, Prælect. § 101. cum notis; Haller, Prim. Lin. cap. 22; and El. Phys. lib. xxii; Sœmmering, Corp. Hum. fab. t. vi. p. 142..8; Blumenbach, Inst. Phys. § 24; Fordyce on Digestion, p. 70..2. The nature and composition of the pancreatic juice formed a part of the experimental researches of Tiedemann and Gmelin, to which I have referred above. Their account is the more interesting when taken in connexion with that of the saliva, to which substance it has been commonly supposed to be nearly identical. We learn, however, that this opinion is erroneous, and that the secretions differ in the following respects. 1. The amount of the solid contents of the saliva is only half as much as that in the pancreatic juice; 2. The saliva contains mucus and proper salivary matter, with perhaps very minute quantities of albumen and caseous matter, whereas in the pancreatic juice the proportion of these principles is reversed, the two latter existing in great abundance, and the two former in very minute quantity; 3. The pancreatic juice is either neutral, or contains a little alkaline carbonate; 4. There was no sulpho-cyanate in the pancreatic juice of the sheep, although it is found in the saliva of this animal; it does not appear that the human pancreatic juice was examined; Recherches, t. i. p. 41, 2. We find the same discrepancy in the analysis of the pancreatic juice by Leuret and Lassaigne, which we noticed above with respect to the saliva; for while the Heidelberg professors point out various circumstances, in which it differs from the saliva, the French Physiologists state that the fluids are nearly similar; Recherches, p. 49 et seq. I may observe that the pancreatic juice was one of the substances to which Leuret and Lassaigne particularly directed their attention, and which they appear to have been fortunate in procuring in considerable quantity for the purpose of examination. It may be worthy of remark, that in a case of inflammation of the pancreas, which is detailed by Mr. Lawrence,

If the idea be correct that the substance which gives the mucous secretions their characteristic properties be albumen in the coagulated state, it will follow, that the solid membranous bodies must belong to this class rather than to the albuminous, and ought indeed to be considered as the completion of that process, of which a mucous secretion is the first step. But it would be premature, in the present state of our knowledge, to proceed upon such strictly technical principles. I may farther remark, that if the two classes of albuminous and mucous substances be so nearly connected, as this view of the subject would suppose them to be, we may readily imagine that a slight change in the action of the secreting organ may effect a change in the substance produced, and convert a mucous to an albuminous secretion or the contrary; and I am disposed to think that I have witnessed several examples of this kind of conversion[1].

The fourth class of secretions, the gelatinous, are so named from their essential characters depending upon the jelly which they contain, the specific property of which is to liquefy by heat and to become concrete by cold, exhibiting the phenomena to which the term gelatinization is applied. Jelly was formerly supposed to be one of the constituents of the blood, and to enter into the composition of several of the animal fluids; but more accurate experiments have proved that this opinion is erroneous[2]. It is, however, found very plentifully in many of the solids, and particularly in the membranous or white parts, although it is not confined to them[3]. It may seem remarkable that while jelly is so abundant in various animal solids, it should not exist in any of the fluids, notwithstanding its solubility in water. As it is not found in the blood, it follows that it is the result of a proper secretion; yet as we know of no organ especially appropriated to this office, it would seem to

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Med. Chir. Tr. v. xiv. p. 367 et seq., the patient, for some time before death, exhibited an unusual degree of paleness, which would seem to have been occasioned, more by a deficiency of the red particles of the blood, than of the fluid generally.

[1] A high degree of mercurial action on the salivary glands appears to convert the fluid which they secrete into a substance of an albuminous nature, while a certain state of irritation in some of the serous membranes has caused them to secrete a mucous fluid; see Med. Chir. Tr. v. x. p. 80, 1; and v. xiii. p. 73 et seq. For remarks on the mucous secretions, the student may consult Thomson's Chem. v. iv. p. 424, 5; 525 et seq. Thenard, Traité, t. iii. p. 687 et seq. Henry's Elem. v. ii. p. 366, 7. Berzelius in Ann. Phil. v. ii. p. 381 et seq.; and Med. Chir. Tr. v. iii. p. 242 et seq. Magendie, Physiol. t. ii. p. 365 et seq. Children's Thenard, § 270. Bostock, in Med. Chir. Tr. v. iv. p. 75 et seq.

[2] Med. Chir. Tr. v. i. p. 71..3; and v. iii. p. 233; Ann. Phil. v. ii. p. 205.

[3] Berzelius, however, appears to think that jelly does not actually exist as one of the constituents of the body, but that it is generated by the boiling, instead of being merely extracted by this process; View of Animal Chemistry, *p. 50.*

follow, either that it must be secreted by the minute capillaries that are dispersed over the parts where jelly is found, or that we are led to regard it as an extra-vascular production, being formed by some change in the elements of the body from which it is composed, after it leaves the arteries. We learn from an interesting experiment of Mr. Hatchett's, that albumen may be converted into jelly by digestion in diluted nitric acid[1]; and there is reason to suppose that the conversion is produced by the addition of a portion of oxygen to the albumen, a conclusion which is in some degree confirmed by the ultimate analysis of the two substances[2]. It is therefore reasonable to suppose that the same change may take place in the living body; and that the albumen which is conveyed by the capillary arteries, either while it remains in these vessels, or when it is discharged from them, is united to a portion of oxygen, and thus converted into jelly.

It is to be observed in respect to the relation between these two substances, that jelly exists in much greater quantity in the corresponding parts of young than of old animals, so that those parts which in the early stages of existence are almost entirely composed of jelly, as age advances, are found to consist principally of albumen. We are not aware of any peculiarity in the state of the body during infancy, or in any of its functions, which can explain the superabundance of jelly at this period, nor do we know of any provision which there is in the constitution of the system for its more copious production at this period. We are equally ignorant of the mode in which it is afterwards disposed of: if it be taken up by the absorbents, it must be altered in its composition before it arrives at the blood, because we do not find any traces of it in this fluid, so that, perhaps, upon the whole, it is more probable that it undergoes some farther change, as we are not acquainted with any purpose which it serves while in the state of jelly, nor is it easy to understand in what way it is removed from the system. Jelly is never found but in connexion with a membranous substance, between the fibres or interstices of which we may presume that it is deposited, but there are some cases in which the jelly seems to form by far the greater proportion of the compound.

One of the parts of the body from which jelly is procured

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[1] Phil. Trans. for 1800, p. 385. Fourcroy maintained an opinion that jelly is constituted by the combination of a portion of oxygen with albumen; Ann. Chim. t. iii. p. 261; but it does not appear that he ever actually effected the conversion, although this discovery has been ascribed to him; Thomson's Fourcroy, v. iii. p. 273.

[2] According to Gay-Lussac and Thenard, the following are the elements of albumen and jelly; Children's Thenard, p. 357; Thenard, Chim. t. iv. p. 404.

	Carbon.	Oxygen.	Hydrogen.	Nitrogen.
Albumen....	52·883	23·872	7·54	15·705
Jelly........	47·881	27·207	7·914	16·988

the most copiously is the skin, but it is somewhat doubtful in what state of combination it exists there; whether it be dispersed through a substratum of coagulated albumen, which appears generally to form the basis of the animal solids, or whether the jelly be itself organized, a supposition which is rendered probable by the large proportion which it forms of certain substances. In isinglass for example, the insoluble part is not more than about 1·5 per cent. of the whole, and unless we conceive the jelly, in this case, to form a mere concretion, (an idea which is inconsistent with all our conceptions of the constitution of the animal body,) we are almost reduced to the necessity of supposing the jelly itself to be organized. Like the albumen, jelly is nearly free from salts or any other extraneous substances.

The fifth class of secretions, the fibrinous, are so named from their resemblance to the fibrin of the blood, and from this being the probable source whence they are immediately derived. They differ from those that have been hitherto examined in the circumstance of their containing a larger proportion of nitrogen, or being, as it is said, more completely animalized, in their chemical composition[1], while in their physical structure, they retain the peculiar fibrous texture of the substance from which they are produced. In this class we must place the muscular fibre under all its various forms, which, whether constituting the long fibres of the proper muscles, or the short ones of the muscular coats, appears to possess exactly the same chemical composition, and nearly the same physical form and arrangement with the fibrin of the blood[2]. This is one of those cases where the effect of secretion appears to consist merely in separation, and this we may conceive to be accomplished by the separated substance having been simply discharged by the mouths of the capillary arteries, and deposited in its appropriate situation in the body, so as to be adapted, without any farther change, to the office which it is afterwards to serve in the animal œconomy.

It may be questioned whether there be any substance, except the muscular fibre, which ought to be arranged under this division. The other constituents of the body which exhibit a fibrous structure are, for the most part, what have been already included among the albuminous secretions, as being formed of this substance in the coagulated state, so that, upon the principles of the chemical arrangement, it appears necessary to include them in this division. It must, at the same time, be admitted, that the difference between the elementary constitution of albumen, jelly, and fibrin, is not very considerable, nor is it very decisively established, yet there appears reason to conclude that an essential difference between them does exist. There are some other parts of the body which also possess a fibrous texture, such as the basis of the cutis, but this would seem to have more relation in its chemical composition to albumen or to jelly, than to fibrin. The fibrous coat of the arteries belongs to the class of substances which we are now examining, as also the fibres of the iris [1], and probably both of these must be considered as nothing more than mere varieties of the muscular structure. Fourcroy and Vauquelin have described a peculiar substance of a fibrous texture, which is found in the seminal fluid, and would appear to compose the specific part of this secretion, which, perhaps, ought to be referred to this class [2]. A fibrous substance was found by Dr. Marcet composing the basis of a urinary calculus, and similar substances have been occasionally met with in other morbid concretions, which are lodged in the different cavities or passages of the body; but it is probable that these were merely portions of fibrin that had been effused from a ruptured vessel, and not the result of any new action of the vessels [3].

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[1] Gay-Lussac and Thenard's analysis of fibrin is as follows:

Carbon 53·360, Oxygen 19·685, Hydrogen 7·021, Nitrogen 19·934; thus giving us 2·946 per cent. more nitrogen than in jelly, and 4·229 per cent. than albumen; Recherches, t. ii. p. 350; Thenard, Traité, t. iii. p. 523, and t. iv. p. 305; Children's Thenard, p. 357. It is generally admitted that the elementary constitution of the pure muscular fibre is identical with that of the fibrin of the blood.

[2] Cuvier indeed observes, Leçons, t. i. p. 90, 1, that fibrin is not found in any of the food that is taken into the stomach, and concludes that it is formed by respiration; the operation of this function he supposes is to remove carbon and hydrogen from the blood, and consequently to leave in it a larger proportion of nitrogen.

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[1] I have remarked above, p. 242, that Prof. Berzelius and Dr. Young conceive the chemical composition of these parts to differ from that of the proper muscular fibre; with every feeling of respect for such high authority, it appears to me that the experiments are not sufficiently decisive to enable us to form an opinion upon the subject. It would be desirable to compare the elementary analysis of the muscles of fishes and of the mollusca with those of the mammalia, in order to ascertain with what variety of chemical composition muscular contractility can be connected; we should probably find some difficulty in reconciling the chemical with the physiological arrangement.

[2] Ann. Chim. t. ix. p. 64 et seq.; Thomson's Chem. t. iv. p. 534..7; Thenard, Traité, t. iii. p. 694, 5. The nature and functions of the spermatic animalcules, which formerly gave rise to so much controversy, Blumenbach, Inst. Phys. § 528, and note (G), and the existence of which appears to be confirmed by the late observations of Prevost and Dumas, Edin. Phil. Journ. v. vii. p. 247, will be more properly considered hereafter.

[3] Perhaps the synovia ought to be included among the fibrinous secretions, as we are informed by Margueron, that its specific character depends upon a substance of a fibrous texture, but the experiments do not enable us to decide, whether it be of the nature of muscular fibre or of membrane; Ann. Chim. t. xiv. p. 123; Thomson's Chem. v. iv. p. 532..4; Thenard, Traité, t. iii. p. 685, 6; Henry's Elem. v. ii. p. 433, 4; See Vauquelin's Analysis of Synovia from an elephant; Ann. Chim. et Phys. t. vi. p. 399 et seq.; and Journ. Pharm. t. iii. p. 289 et seq. I had once an opportunity of examining a fluid from the cavity of the knee joint; it consisted of water holding in solution about 5 per cent. of albumen, in which a number of flakes or masses were floating, that appeared to be composed of coagulated albumen; Med. Chir. Tr. v. iv. p. 74.

We now arrive at a class of bodies that are more distinct from any of those that are found naturally existing in the blood, and which we may therefore suppose to be the result of a more elaborate or complicated action of the secretory organs, the oleaginous secretions, those that derive their essential character from the presence of an oily ingredient. These compose a numerous, and at the same time, a considerably varied class of substances, in some of which the oil is nearly in a state of purity, or at least forms the greatest part of the body in question, while in the others, the oil is mixed with other animal principles, in such a manner, that it is not easy to decide in which division the substance under consideration ought to be placed. As a matter of convenience, I have thought it better to place every substance in this class that contains oil in any notable proportion, or of which any of the specific characters depend upon oil, although the actual quantity of it may be less than that of some of the other ingredients. Of the oleaginous secretions, the first that claims our attention, both from its quantity and the state in which it exists, is the fat of all kinds, which is found connected with the muscles and many of the viscera. In its chemical constitution fat appears to agree very nearly with the expressed vegetable oils ; like those it varies in its consistence, or rather in its freezing point, so as in the ordinary temperature of the atmosphere, to be found sometimes in a solid state, as is the case with suet and tallow, and at other times perfectly fluid, as we find it more particularly diffused through the cellular texture of the cetacea. We are not acquainted with any apparatus that is appropriated to the secretion of oil, nor are there any facts which can enable us to decide positively upon the mode of its formation. As a substance of an oily nature has been said to enter into the composition of the chyle, and as the formation and deposition of fat appear to bear a relation to the quantity of chyle that is produced, it has been conjectured that the oleaginous secretions originate in the process of chylification, but it may be objected to this idea that the fat cannot be detected in the blood. Individual cases had indeed been recorded, where a substance of an oily or creamy consistence and appearance had been observed floating on the surface of the serum; but these were of rare occurrence, and were regarded as depending on a morbid, or at least an unusual state of the system. The late experiments of Dr. Trail and Dr. Christison[1] have enabled them to ascertain more precisely the nature of this oily matter, while, as I have stated in a preceding chapter, those of Dr. B. Babington[2] and M. Lecanu[3] seem to show, that a certain species of oil is one of the constituents of healthy blood, which becomes visible when it exists in more than its usual proportion[1].

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[1] Ed. Med. Journ. v. xvii. p. 235, and v. xxxiii. p. 274.

[2] Med. Chir. Tr. v. xvi. p. 46 et seq.

[3] Ann. Chim. et Phys. t. xlviii. p. 308, and Journ. Pharm. t. xvii. p. 485 *and 544.*

But from the circumstance of there being no appropriate organ for the secretion of fat, and also from the fact of its being deposited in so many parts of the body, or rather connected with textures of such various descriptions, we may conclude that fat must be formed by some peculiar action of the capillary system generally. The effect may be produced either by some change in the action of the vessels themselves, or in the composition of the fluid which is transmitted through them; and it is most probable that this last is the case, because the deposition of fat is obviously connected with the state of the digestive organs. We have no certain grounds for enabling us to judge from what part of the blood the fat is immediately produced, but perhaps it may be considered more probable that it is from the albumen than from the fibrin, as we should scarcely expect that after the fibrin has been elaborated in the blood, it should be again decomposed. It may, however, be remarked, on the other hand, that the fibrin appears to be the most variable in its proportions of any of the constituents of the blood, and that the formation of fat may be the means by which it is carried off, when it is formed in greater quantity than is required for the wants of the system.

If we consider fat in its chemical relation to the other constituents of the blood, either the albumen or the fibrin, we shall find that it differs from them in containing no nitrogen, little or no oxygen, and a less proportion of carbon, so that when they are converted into adipose matter, the nitrogen and oxygen are retained with a portion of the carbon, while the hydrogen and a portion of the carbon are separated and compose the fat. There are two modes in which we may conceive of this operation being performed, according as we suppose the fat to be produced while in the vessels themselves, or not until it is just upon the point of being excreted from them. In the first case the operation must consist in the abstraction from the fluid of its nitrogen and oxygen and a part of its carbon; in the other, of the hydrogen and a part of the carbon, while the remainder of the elements are left in the vessels. The circumstances which favour the deposition of fat, are an excess of nutritive matter received into the blood, at the same time that the secretions or excretions of various kinds are not duly discharged; we may therefore suppose that in this case there is a provision in the system for the removal of the superfluous matter, and that this is done by various means and by various organs. The principal part of the hydrogen appears to be disposed of in the formation of fat, the carbon is carried off partly by this means, and partly by the lungs, while the nitrogen is, perhaps, principally removed by the kidney.

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[1] Haller, El. Phys. lib. i. sec. 4, p. 35..9, conceived that the fat existed in the arterial blood, and exuded from it through small pores, with which the vessels were supposed to be furnished; Wm. Hunter; Med. Obs. and Inq. v. ii. p. 33..36; thought that the fat must be secreted by a specific organ, but the considerations which they have adduced in favour of their respective opinions are altogether of a general nature. Magendie; El. Phys. t. ii. p. 347, 8; places fat among the cellular exhalations, because no specific structure can be detected for its formation; but it appears inconsistent to call a substance an exhalation, which is so unlike any of the proximate principles of the blood. Sir Everard Home conceives, that fat is not a secretion, but that it "is formed in the colon, and is thence taken up into the blood vessels, and distributed to the different parts of the body;" Lect. on Comp. Anat. v. i. p. 468 et seq. and Phil. Trans. for 1821, p. 34; but there appears no sufficient ground for this opinion. For an account of the different adipose secretions, the student may examine Thomson's Chem. v. iv. p. 438..0; Thenard, Chem. t. iii. p. 620..637; Henry's Elem. v. ii. p. 382, 3; and Blumenbach, Inst. Phys. sect. 33. I shall beg to refer to some experiments which I performed in the year 1807, on the action of alcohol, upon different oleaginous or fatty substances; *Nicholson's* Journ. v. xvi. p. 165, 6.

The secretion of fat, however it is effected, is an operation which proceeds with great rapidity, for we find that no animal solid is so quickly generated where circumstances are favourable for its production. It is, on the other hand, the substance which is first removed from the body, when the system is suffering from inanition or disease[1], so as to indicate that the fat is the part upon which the absorbents are the most disposed to act, while it is, at the same time, the most readily deposited by the capillary arteries. The great facility with which fat is generated, and afterwards removed from the system, has led some physiologists to regard it as one of those substances which are properly excrementitious, separated from the blood, rather in consequence of their being noxious, or at least useless, than from any important purpose which they serve. There may be some foundation for this opinion, but still, as I have had occasion to remark, it appears to be more analogous to the contrivance which we observe in the animal frame, and in the adaptation of its parts and actions to each other, to conceive that it may serve some important secondary purpose, and that even in the very act of being discharged it may produce some useful effect, which could not have been so well accomplished by any other means.

The uses which were assigned to the fat by the older physiologists, were principally of a mechanical nature, as that of lubricating the muscles and tendons, or giving them their proper degree of flexibility and suppleness; but it seems not easy to conceive how the fat, lodged as it is in distinct receptacles, can produce this effect. In the cetacea, it may serve to render the body less disposed to part with its heat and thus enable it to resist the cold medium to which it is exposed; but this purpose cannot be served by the fatty matter of quadrupeds, a large part of which is situated about the internal viscera[2]. We seem, therefore, to be under the necessity of looking out for some other more important purpose which the fat may serve in the animal œconomy; and it has accordingly been suggested, that the adipose secretions may compose a reservoir of inflammable matter which will serve for the formation of carbonic acid in the lungs, when from any cause there is a deficiency of the usual supply as derived from the chyle. In ordinary cases, the thoracic duct pours into the venous trunks a quantity of chyle sufficient both for the growth and the nutrition of the body, and for the consumption of carbon by the lungs, but if, from any cause, the supply is insufficient, the absorbent system takes up the adipose matter from its various receptacles, and introduces it into the sanguiferous system, where it serves for the generation of carbonic acid, and consequent production of animal heat[1].

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[1] Haller, El. Phys. xix. 2, 3.

[2] Magendie regards the uses of fat to be principally connected with its *physical properties*; Physiol. t. ii. p. 349.

Besides the fat under its ordinary forms, and in its various states of solidity, the marrow belongs to this class of secretions[2], and also the substances which are produced from the sebaceous glands that are found in various parts of the body. These probably exist in a greater or less quantity in all animals, and impart to them their specific odours, many of which are very peculiar and powerful, and are connected with some of the most important instincts of the brute creation. Among the oleaginous secretions we ought probably to place the cholesterine, which forms the basis of biliary calculi, although it differs from fat in some of its chemical relations, and it may moreover be doubted whether it be not formed after the substance of which it is composed leaves the vessels, and is simply lodged in the biliary ducts[3].

We have several other secretions which owe some of their

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[1] See the elegant inaugural dissertation of Dr. Skey, "De Materia Combustibili Sanguinis;" also Prof. De la Rive, "De Calore Animali," passim. That the fat is the origin of the inflammable matter which serves to maintain the animal heat, was maintained by Moschati, but his opinion was obscured by much false reasoning and incorrect experiment; see Journ. Phys. t. xi. p. 389. Adelon conceives, that the principal use of the fat is to serve as a reservoir of nutritive matter, when the body is deprived of its regular supply by the ordinary channel; Physiol. t. iii. p. 477. Adouin supposes that the fatty globules, which are observed in the epiploon of the Arachnidæ serve the same purpose; Cyclop. of Anat. v. i. p. 204.

[2] Marrow has lately been analyzed by Berzelius, and was found to consist of the following substances:

Pure adipose matter	96
Skins and blood-vessels	1
Albumen Jelly Extract Peculiar matter Water	3
	100

Thomson's Chem. v. iv. p. 487.

[3] See Chevreul, Ann. de Chim. t. xcv. p 7..10, and Ann. de Chim. et Phys. t. vi. p. 401. Spermaceti and wax are also adipose secretions, but they are not produced by the organs of the human subject.

peculiar characters to the oil which they contain. Among these is milk, a very compound fluid, which is formed principally of oil in combination with albumen, so united as to form a kind of emulsion. By mere rest the greatest part of the oil separates, the albumen still remaining combined with the water and the other ingredients, from which it cannot be detached without the intervention of a chemical re-agent, which, by coagulating it, renders it easily separable by mechanical means. Milk likewise contains a saccharine matter, which assists in adapting it for its appropriate office, that of nourishing the young animal immediately after birth, and may also have the farther use of contributing to preserve the milk in a fluid state, by rendering the emulsion of albumen and oil more perfect. Milk is secreted from a body which possesses all the appropriate parts of the glandular structure on a large scale, and appears to be possessed of a very elaborate organization. Were we to reason from the analogy of the other secretions, we might be led to form a conclusion, which is probably very different from the common opinion, that of the three substances which essentially compose milk, the sugar is the one for which the glandular apparatus is more particularly required. The albumen does not appear to differ essentially from the albuminous part of the serum, and we do not find that oil, in other parts of the system, requires any distinct gland for its formation.

It may, indeed, be thought an objection to this idea, that the kidney, in a certain morbid state, acquires the property of secreting sugar, from which it would seem that this substance, although so different in its nature from any of the constituents of the blood, may yet be formed from it, without any thing very peculiar or specific in the structure of the secreting organ, so that we might be inclined to ascribe the effect, rather to some alteration in the fluid that is brought to the part, than to the action of the organ itself. It is to be observed, however, that the sugar of diabetes exactly resembles vegetable sugar, while the sugar of milk differs from it in the proportion of its elements, and likewise in the result of the action of nitric acid upon it, which produces mucic acid with the sugar of milk, and oxalic acid with the sugar of diabetes[1].

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[1] The ultimate analysis of vegetable sugar, as given by the latest experiments, is as follows:

	Gay-Lussac and Thenard.	Berzelius, the average of 4 processes.	Prout.	Ure.
Hydrogen..	6·9	6·882	6·66	6·29
Carbon....	42·47	43·125	39·99	43·38
Oxygen....	50·63	49·993	53·33	50·33
	100·00[*]	100·000[†]	99·98[‡]	100·00[§]

The elements of the sugar of milk are as follows:

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[*] Research. t. ii. p. 289.

[†] Ann. Phil. v. v. p. 264..6.

[‡] Med. Chir. Tr. v. viii. p. 536, 7.

[§] Phil. Trans. for 1822, p. 467.

The milks of different kinds of animals have been minutely examined by various chemists, and although they have been found to differ considerably in the amount of their solid contents, and in the proportion which their constituents bear to each other, they essentially agree in their composition, as consisting of albumen, oil, and sugar, dissolved or suspended in a large quantity of water. In many cases we can perceive a relation between the nature of the milk and of the animal which is to be nourished by it, and we may remark generally that this fluid appears to be the combination of all others, the best adapted for supplying the elements of nutrition in the early stages of existence, when there is a necessity for a copious supply of nutriment, while the digestive organs are in a state of extreme delicacy. Berzelius, who has lately analyzed cow's milk, has found it to contain a small quantity of the earthy phosphates, an evident provision for the formation of bone, while it would appear to differ from most of the other secretions, which consist principally of albumen, in containing no soda, either in the combined or uncombined state[1]. The temporary existence of this secretion at a period when its utility is so obvious, with its cessation

	Gay-Lussac and Thenard.	Berzelius.
Hydrogen	7·341	7·167
Carbon	38·825	39·474
Oxygen	53·834	53·359
	100·000[*]	100·000[†]

It would appear from these analyses, that the sugar of milk contains more hydrogen and oxygen, and less carbon, than vegetable sugar. Dr. Prout, however, gives a somewhat different account of their comparative composition: he says, "sugar of milk yielded very near the same results" with vegetable sugar, and that their apparent difference is "to be attributed to the influence of the presence of minute portions of foreign matters, analogous, for example to what occurs in the inorganic kingdom, in the mineral called arragonite;" Med. Chir. Tr. v. viii. p. 538. With respect to diabetic sugar, Dr. Prout could not find it to differ, in its ultimate analysis, from vegetable sugar; p. 537. Dr. Ure, on the contrary, finds its elements to be considerably different; he gives the following proportions:

Hydrogen	5·57
Carbon	39·52
Oxygen	54·91
	100·00[‡]

As to the question of identity in this case, perhaps we ought to depend more upon the effects produced by nitric acid, than upon the results of the elementary analysis. We are informed by Vogel, Ann. Chim. t. lxxxii. p. 156, that sugar of milk may be converted into a sugar resembling that from vegetables, by being digested with very dilute sulphuric or muriatic acid, thus furnishing an additional analogy between animal sugar and gum; See Thenard, Chim. t. iii. p. 549..1.

[1] The following is Berzelius's analysis of skimmed cow's milk:

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[*] Research. t. ii. p. 293.

[†] Ann. Phil. v. v. p. 266.

[‡] Phil. Trans. for 1822, p. 467.

when no longer required, must be regarded as a very remarkable example of the adaptation of the system to the circumstances in which it is placed; we are quite ignorant of the means by which this change is brought about or of the other changes in the system with which it is connected.

A substance which bears an analogy to milk, as consisting of an intimate combination of albumen, and an oily ingredient, is the cerebral matter. It, however, differs from milk in the albumen appearing to be in a half coagulated state, as well as in its other constituents, the brain containing no saccharine matter, while a portion of the peculiar substance called ozmazome is said to be found in it. Vauquelin, who performed an elaborate set of experiments on the cerebral matter, informs us that he procured a quantity of phosphorus from it; but from the nature of the process which he employed, it is not certain whether the phosphorus might not have been in the state of a phosphate. According to his analysis, rather more than one-fourth part of the solid contents consists of a fatty substance, and nearly one-third of albumen, the remainder being composed of osmazome, phosphorus, acids, salts, and sulphur[1].

Water	928·75
Cheese, with a trace of butter	28·00
Sugar of Milk	35·00
Muriate of potash	1·70
Phosphate of potash	0·25
Lactic acid, lactate of potash, and a trace of lactate of iron	6·00
Earthy phosphates	0·30
	1000·00

Ann. Phil. v. ii. p. 424.

From the above analysis, as well as from the remarks of Berzelius in Med. Chir. Tr. v. iii. p. 273..6, we find that milk differs from blood, as well as from most of the animal fluids, in containing salts of potash, in place of the salts of soda; see also Progress of Anim. Chem. p. 111..3. I may remark, that the absence of an uncombined alkali in milk would, on Berzelius's principle, exclude it from the class of secretions strictly so called; vide suprà, p. 481. For the account of milk, beside the above references, see Parmentier and Deyeux, Journ. de Phys. t. xxxvii. p. 361 et seq.; Plenk, Hydrol. p. 86 et seq.; Fourcroy, System, v. ix. p. 468 et seq.; Haller, El. Phys. xxviii. 1. 16..22; Henry, Elem. v. ii. p. 419 et seq.; Thomson's Chem. v. iv. p. 503 et seq.; Young, De Lacte, in Sandif. Thes. t. ii. p. 523; this treatise, which was originally published in 1761, contains a very full account of all that was then known upon the subject, both with respect to human milk and that of other animals.

[1] Vauquelin's analysis is as follows:

Water	80·
White fatty matter	4·53
Red ditto	0·70
Albumen	7·00
Osmazome	1·12
Phosphorus	1·5
Acids, salts, and sulphur	5·15
	100·00

Ann. Chim. t. lxxxi. p. 37; Ann. Phil. v. i. p. 332 et seq.

The seventh class of secretions, what I have denominated the resinous, are in their chemical properties considerably similar to the oleaginous, yet they appear to be so far distinguished from them, as to justify their being placed in a separate class. They derive their specific characters from an ingredient which is soluble in alcohol, and in various respects resembles a resin. Of these the most remarkable is the substance which constitutes the basis or specific ingredient of the bile. Bile is a very compound fluid, which is secreted by a gland of a peculiarly large size and elaborate structure. In consequence of its supposed use in the animal œconomy, and its remarkable chemical properties, bile has been frequently made the subject of examination; but although various chemists of the first skill and dexterity have exercised themselves in investigating its nature, we still find considerable discordance in their account of it; we may, however, conclude from them that it contains a substance which is analogous to a resin, upon which its peculiar characters most especially depend[1]. From the anatomical structure of the liver, and particularly from the connexion of its blood-vessels with the other parts of the sanguiferous system, it appears probable that the secretion of bile is more immediately made from venous blood. The veins that collect the blood from the abdominal viscera, which are more immediately concerned in the process of digestion[2], unite together into a large trunk, which is named the vena portæ; this enters the liver, and divides into numerous branches, that are distributed through all its substance. The minute ramifications of these vessels terminate partly in the hepatic ducts, which contain the bile, formed, as it thus appears, from this venous blood, while the rest of it passes off by the hepatic veins, and is transmitted by the ordinary course of the circulation into the vena cava[1]. Nothing certain is known respecting the mode in which venous blood is converted into bile, but there are some facts and analogies, which would lead us to conceive, that it is more particularly derived from the red particles, and that the conversion is effected by the addition of oxygen to these bodies[2].

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[1] For the composition of bile, see appendix No. 1, at the end of the chapter.

[2] Jacobson, of Copenhagen, has made some interesting observations on the comparative anatomy of the venous system of birds, reptiles, and fishes, which tend to illustrate this point; Edin. Med. Journ. v. xix. p. 78. This distribution of the veins is well displayed in Bell's Dissect. pl. 4. fig. 1. The case which is related by Mr. Abernethy, Phil. Trans. for 1793, p. 59 et seq., where the vena portæ terminated in the vena cava; and one by Mr. Wilson, where an unusual termination of the vena portæ was likewise observed, Monro, (tert.) Elem. v. i. p. 564, 5, only prove that the elements of bile may be obtained from arterial blood. See the remarks of Sir C. Bell, Anat. v. iv. p. 121, 2, and of Dr. Fleming, Phil. of Zool. v. i. p. 327. Bichat regularly discusses this question; but as the arguments which he adduces in favour of the opinion are very hypothetical, it was not difficult to find answers to them; Anat. Gén. art. 6. t. i. p. 406..8. Among the earlier anatomists, one of the most minute accounts that we have of the liver and its functions, is by Glisson; Anatomia Hepatis. He discusses at length the question concerning the excrementitious nature of the bile, and concludes in favour of the doctrine; c. 38 et alibi. Another point which he very minutely examines, is respecting the connexion between the gall bladder, and the other parts of the hepatic system; the mode in which the bile is conveyed into and out of this receptacle was a problem that the older anatomists found it very difficult to solve; Glisson states in detail the hypothesis of Laurens, Fallopio, Bartholin, Riolan, and others, which he formally discusses; C. 17..20. See on the subject Sœmmering, t. vi. § 101. p. 194, 5. Before Harvey's discovery of the course of the circulation, the peculiar distribution of the blood-vessels of the liver led to the opinion that this organ was to be regarded as the part whence the venous system took its origin. Sœmmering, t. vi. § 84. p. 181..3, and Monro, (tert.) Elem. v. i. p. 563 et seq., offer various considerations in favour of the opinion, that the vessels connected with the vena portæ secrete the bile. See also Blumenbach, Inst. Physiol. § 427.

The great size of the liver, and the peculiar nature of the fluid which it secretes, naturally led to various conjectures respecting its relation to the general actions of the system, many of which are palpably incorrect, and founded upon fundamentally erroneous doctrines. It was a favourite opinion of some of the older physiologists, that the bile was a highly putrescent fluid, and they supposed that one principal use of the liver was to carry off from the system all the matter which was disposed to the putrid fermentation. By the modern physiologists, the bile has been supposed to be immediately useful in promoting the process of digestion[3]; and it is very probable that this is the case, but there are various pathological considerations which induce us to regard the bile as essentially an excrementitious substance, although, in conformity with the usual operations of the animal œconomy, some other important purpose may be served by it. When the venous blood becomes loaded with inflammable matter which cannot be discharged from the lungs, principally in consequence of the high temperature to which the animal is exposed, and when, from certain causes, one of which appears to be the increase of cutaneous perspiration, this excess of inflammable matter is not employed in the deposition of fat, the liver would appear to be the organ by which it is removed. In ordinary cases, the quantity discharged is small, probably no more than what is sufficient to preserve the liver in its healthy state, and to perform the secondary objects to which the function is subservient; but when, from a conjunction of circumstances, there is an excess of inflammable matter, its accumulation is prevented by an increased discharge of bile[1].

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[1] Bell's Dissect. p. 22; the figures in plate 4 of this work, present an excellent view of the vascular system of these organs.

[2] The paper of Mr. Kiernan, published in the Phil. Trans. for 1833, p. 711 et seq. on the intimate structure of the liver, the connexion of its vessels, and their relation to each other, must be regarded as an admirable specimen of anatomical and physiological investigation. The general conclusions which he draws, and which he appears to have fully confirmed by his investigations are, that the hepatic artery is destined solely for the nutrition of the liver, having no direct connexions, except with the branches of the vena portæ, after its own blood has become venalized. He describes the lobules of which the liver is composed as consisting of a central vein, which he styles intralobular, and which constitutes one of the hepatic veins, the office of which is to carry off the blood after the bile has been separated from it. Hence we have four systems of vessels in the lobule, those of the portal veins, of the hepatic veins, of the hepatic arteries, and of the biliary ducts. The portal vein forms a plexus in the lobule, from which the bile is separated, and the remainder of the blood is then received into the hepatic vein. The globule is enclosed in a cellular capsule, on which the portal vein, the hepatic artery, and the hepatic duct ramify; the hepatic vein is in the interior of the lobule. As far as anatomical structure is concerned, Mr. Kiernan's investigations appear to be most complete and satisfactory; the physiological question, however, still remains to be answered, what is the immediate cause of the separation of the bile from the venous blood? How far is it to be regarded as a mere vital action, or is there any chemical or mechanical principle concerned in the operation? The part of the organ in which the secretion is actually produced would appear to be a plexus of minute branches of the portal vein, which ramifies on the parietes of the biliary ducts, but it may be presumed, that the duct itself can have no influence on the actual formation or even the separation of the bile, it being merely the organ in which it is deposited.

[3] The purposes that the bile was supposed by the earlier physiologists to

Another very important secretion, which may be classed under the head of the resinous bodies, is the urea, or that substance which constitutes the peculiar or specific ingredient in the urine. The urea does not indeed possess the characters of a resin in so remarkable a degree as the biliary matter, but it approaches more nearly to this class than to any of the rest. It ought probably to be regarded still more than bile, in the light of an excrementitious substance[2]; for although it may serve

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serve in the animal œconomy, are very numerous, but as they are, for the most part, quite hypothetical, it will not be necessary to examine them in detail; it may be sufficient to refer to Boerhaave, Prælect. § 99 cum notis, t. i. p. 213 et seq.; and to Haller, El. Phys. xxiii. 3. 32..35.

[1] Dr. Stoker conceives, "that the chief use of the liver is to bring the hydro-carbonous principles with which the blood in the vena portæ is charged more nearly to the state of fat, thereby rendering the blood more prepared for the changes it has to undergo in the lungs, as well as to provide for the due supply of fat in different parts of the body." To illustrate the operation of the liver, he compared the blood from the vena cava with that from the vena portæ, when it was supposed that the former exhibited some indications of the presence of unctuous matter, which were not observed in the latter; Trans. of the Assoc. Phys. v. i. p. 163, 4. Tiedemann and Gmelin, in their researches on digestion, performed a series of experiments on the effect produced on this function by tying the biliary duct, and they are hence led to make some observations on the primary use of the liver in the animal œconomy. This is supposed to consist in its separating from the venous blood certain substances, which contain an excess of carbon and hydrogen; thus fulfilling a function very analogous to that of the lungs. The lungs remove these elements from the system under the form of gas and vapour, the liver under that of a semi-fluid substance. There is also this essential difference between the excretions of these organs; the first are similar to the products of combustion, while the latter are still combustible. Recherches; t. ii. p. 60. Dr. Elliotson maintains a similar opinion; Physiol. p. 102, 3.

[2] Berzelius supposes that there exists in various parts of the body a peculiar substance which is formed of decayed or decomposed animal matter, united to lactic acid or to some of the lactates; that this compound is taken up by the absorbents, carried to the blood, and afterwards discharged by the

other secondary purposes in the œconomy, there seems sufficient reason to believe, that its primary use is to discharge from the system a quantity of matter which is noxious, or at least superfluous. There is this peculiarity in the chemical nature of the urea, that it contains a very large proportion of nitrogen, so as to make it appear that the kidney is the outlet provided in the constitution, by which any excess of nitrogen is removed, or its accumulation prevented.

The quantity of nitrogen which is discharged in the form of urea is so considerable, even in animals whose food does not essentially contain this element, that we are led to inquire in the first place, how it is introduced into the system, and secondly, what purpose can be served by its introduction, when it appears to be discharged again almost as rapidly as it is received. While it was thought that the stomach was the only channel through which nitrogen was introduced, there was great difficulty in accounting for the quantity of it which was obtained by the graminivorous animals; but this difficulty is at least diminished, if not removed, upon the supposition that nitrogen may be absorbed by the lungs. And with respect to the second question, it may be sufficient, in this place, to reply, that from the great importance of the fibrin, as the source and origin of the muscles, and the seat of contractility, it was of the first importance to have a supply of nitrogen for its preparation, and that to ensure a sufficiency of it in every case of emergency, there must necessarily be, in most instances, an excess of it, which excess is carried off by the kidney.

The apparatus by which the urea is secreted, is of comparatively small size, but is complicated and elaborate in its structure, containing all the parts that are ever found in the composition of a gland. The kidney, like the liver, may be classed among the compensating organs, or among those which, independently of any useful office which they may habitually perform, have their actions occasionally increased for the purpose of supplying the deficiencies of other parts. Thus when a larger quantity of fluid is received into the stomach than can be imbibed by the absorbents, the residue is carried off by the kidney; and, in like manner, if the usual discharge from the lungs or the skin is prevented from taking place, we frequently observe that the kidney supplies the place of these organs[1].

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kidney; View of Animal Chemistry, p. 16, 82. We may suppose this substance either to be identical with, or bear a near relation to the animal matter which is in the serosity of the blood. Blumenbach places the fluid of perspiration and the urine in the same class of excrementitious substances; Inst. Phys. sect. 34.

[1] Lining found that the quantity of the urine in summer was to that in winter, taking, in each case, the average of 30 days, as 1 to 2·03; Phil. Trans. for 1743, p. 509. The proportion of the urine to the perspiration in July, was as 977 to 1941; in January, as 1846 to 1006; Ibid. for 1745, p. 321. Stark found the quantity of urine in the day, to that in the night, during *equal times*, to be in the proportion of about 1·8 to 2·7; Works, p. 178; but

Besides the urea, the urine contains several other substances, and particularly a great variety of salts, both earthy and neutral, which will belong to the next class of secretions[1].

With respect to the relation which the urea bears to the other parts of the blood, before we can form any decisive conclusion on this point, it will be necessary to determine how far we are to admit of the speculations of Berzelius, and of the conclusion which Prevost and Dumas deduce from their experiments.

The object which they had in view was to ascertain the nature of the changes which are effected in the blood by secretion, and for this purpose they removed the kidneys from a living animal. The operation was not productive of any immediate injury to the functions, but in a few days various morbid symptoms arose, which seemed to indicate an inflammatory state of the system. The blood was carefully examined after death, and was found to contain a much greater quantity than ordinary of the animal matter which enters into the composition of the serosity, and by subjecting this substance to the action of various re-agents, and also by reducing it to its ultimate elements, it appeared that it exactly resembled urea, so as to lead the authors to conclude "that the urea of the blood is identical with that of the urine."[2]

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his experiments were performed under such peculiar circumstances, that we can scarcely draw any general conclusions from them. He found the perspiration during the day to be rather greater in weight than the urine, while, during the night, its weight was not half that of the urine; Ibid.

[1] For the composition of urine, see appendix, No. 2, at the end of the chapter.

[2] Ann. Chim. et Phys. t. xxiii. p. 90 et seq.; Quart. Journ. v. xvi. p. 119 et seq. The following is the analysis of the urea procured from the blood of one of the dogs that had had the kidneys extirpated:

Nitrogen	42·23
Carbon	18·23
Hydrogen	9·89
Oxygen	29·65
	100·00

This does not differ very much from Dr. Prout's analysis of urea:

Nitrogen	46·66
Carbon	19·99
Hydrogen	6·66
Oxygen	26·66
	99·97

Med. Chir. Tr. v. viii. p. 535.

Of the methods that have been proposed for obtaining urea in a state of purity, those of Dr. Prout, in this paper, and of Berzelius; Journ. Roy. Inst. v. i. p. 401; may be regarded as the most perfect. We have some useful observations on the subject by Wohler, Ann. Chim. t. xliii. p. 64 et seq.; and by Dumas, Ibid. t. xliv. p. 273 et seq. From the recently published number of the British and Foreign Medical Review, p. 592, 3, we learn that these experiments have been confirmed by some that have been performed by Tiedemann, Gmelin, and Mitscherlich.

I may observe, with respect to the result of the experiments of Prevost and

The experiments of the Genevese physiologists, which seem to be countenanced by some lately performed at Tubingen[1], would lead us to the opinion, that the serosity is the part of the blood from which the urea is more particularly derived, or rather that these substances are nearly identical, so as to give the kidney the mere office of separating the urea from the mass of blood, in which it existed ready formed[2]. I conceive that the facts of which we are in possession, however valuable and interesting, will scarcely entitle us to go the full length of this conclusion, but still I think it highly probable that an intimate relation subsists between the serosity of the blood and the urea. We are indebted to Dr. Prout for some very curious observations respecting the connexion between the urine and the digestive organs, which show in how great a degree the chemical properties of the former depend upon the condition of the latter; but these will be considered with more propriety in the next chapter[3].

The cerumen, or ear-wax, as it is termed, would appear, from the analysis of Vauquelin, to have a relation to the resinous secretions[4]; and there are some substances derived from different

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Dumas, that Haller maintains the possibility of urine being produced after the destruction of the kidney; El. Phys. vii. 1..9. Dr. Davy informs us that urea has been found in the liquor amnii of the human fœtus; Phil. Trans. for 1835, p. 535, note. See also Dr. Lee's observations on the functions of the fœtal kidney; Med. Chir. Tr. v. xix. p. 238 et seq.

[1] Edin. Med. Journ. v. xii. p. 473 et seq.

[2] Berard remarks that the secretion of urine appears to have for its object the separation of the excess of azote from the blood, as respiration separates from it the excess of carbon; Ann. Chim. et Phys. t. v. p. 296. It is an ingenious observation of Berzelius, to which Dr. Prout is disposed to consent, that one great purpose which is served by the kidney, is the acidification of the constituents of the blood; Inquiry, p. 30, 1. Adelon considers the excretion of the urine as the process which serves to purify the blood; Physiol. t. iii. p. 526 et seq., see also the art. "Urine," by Andral fils, Dict. de Méd. t. xxi. p. 75 et seq., where we have an account of the changes which the urine experiences in various morbid states of the system; also "Urinaire," by Adelon, ibid. p. 64 et seq., for remarks on the secretion of urine. We are informed that Wohler has succeeded in forming a substance exactly resembling urea by treating cyanite of silver with sal-ammoniac; Brewster's Journ. v. iii. p. 33. Dr. Willis remarks that the kidney is the organ which is specifically adapted to the depuration of the system; Art. "Animal," in Cyc. of Anat. p. 140.

[3] We are indebted to Chossat for an elaborate memoir on the urinary function, consisting principally of a valuable series of statical experiments. With respect to the physiological relations of urine, he conceives that the urea is immediately formed from the chyle, and that the change is effected by the abstraction of a portion of its carbone, during its passage through the lungs. Magendie's Journ. t. v. p. 65 et seq. Upon this hypothesis I may remark, that although we must suppose, that both the urea and the carbone are to be referred to the chyle, as their original source, yet that it appears to disregard the primary use of the chyle, viz., the nutrition and reparation of the system.

[4] Fourcroy, System, by Nicholson, v. ix. 451 et seq.; Thomson, Chem. v. iv. p. 523. We have a valuable paper by Dr. Haygarth on this subject, the principal object of which is to point out the best means of dislodging it

species of animals, as civet, musk, and castor, which may be placed in the same class [1]. We must also refer to this class the peculiar substance which was first pointed out as a distinct animal principle by Rouelle, and was afterwards more accurately examined, by Thenard, and named by him osmazome [2]. It was originally procured from the muscular fibre, of which it forms one of the component parts, and appears to be that ingredient upon which the peculiar flavour and odour of the flesh of animals principally depends [3]. According to some experiments, of which we have the detail in three dissertations lately published at Tubingen, by Gsell, Gmelin, and Wienholt [4], osmazome is found in most of the component parts of the body, as well solids as fluids, although in very different proportions. It was found by Gsell to be much more copious in the muscles than in the bones and tendons, and in the muscles of old, than in those of young animals. Gmelin's experiments were particularly directed to the composition of the kidney; he examined this organ in the human subject, in the ox, and in the rat, and the result was, that a considerable proportion of osmazome was, in all cases, detected in the different parts of it, along with various neutral and earthy salts.

Wienholt extended his examination to other parts of the body, and found them all to contain certain portions of this substance. Some of the most important of his results are on the comparative quantity of osmazome, procured from blood as taken from different vessels; the greatest quantity was obtained from the vena portæ, a smaller quantity from the vena cava, and least of all from the aorta. He seems to consider the animal matter in the serosity as a compound of osmazome and urea, an opinion which nearly coincides with that maintained by Prevost and Dumas [5]. This view of the subject to a certain extent, countenances the idea of Berzelius, that the serosity consists of decomposed matter, which is carried into the blood-vessels, in order to be afterwards removed from the system, while it tends to throw some doubt upon the correctness of the deductions made by Prevost and Dumas from their experiments, as far as respects the theory of secretion generally [6].

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from the ear when it has accumulated there in an excessive quantity; we should be induced from his experiments to conclude that a considerable proportion of it consists of a mucous substance, depending, however, for its specific properties upon a body resembling the resin of the bile; Med. Obs. and Inq. v. iv. p. 198 et seq.

[1] Thomson, Chem. v. iv. p. 441 et seq.; Thenard, Chim. t. iii. p. 777 et seq.

[2] Thomson, Chem. v. iv. p. 425; Henry, Elem. v. ii. p. 465.

[3] Berzelius, however, doubts whether osmazome is to be regarded as a distinct proximate principle; he seems to consider it as a compound of animal matter with lactate of soda; Ann. Phil. v. iii. p. 201, note.

[4] Ed. Med. Journ. v. xii. p. 473 et seq.

[5] Ann. de Chim. et Phys. t. xxiii. p. 94.

[6] The observations of Jacobson referred to above, p. 501, on the comparative anatomy of the venous system of the abdominal viscera, tend to show,

The eighth and last class of secretions are the saline, a very numerous set of bodies, which are found dispersed over every part of the system, and more or less mixed with almost all its constituents. They consist of acids, alkalies, and neutral and earthy salts[1]. The most important of these, both from the quantity in which it exists, and its uses in the system, is the phosphate of lime, which constitutes the earthy matter of the bones, giving them their hardness and solidity, and composing a large part of their substance; but, with the exception of the bones, the fluids generally contain more saline matter than the solids. A certain quantity of salts is always present in the blood, and it would appear that the class of albuminous secretions contains nearly the same kind of salts, and in the same proportion, and this, as I remarked above, without any exact relation to the quantity of animal matter. The proportion of saline matter that is attached to the solid albuminous secretions and to the gelatinous, is much smaller; the pure oleaginous secretions appear to be entirely destitute of any saline impregnation, while, on the contrary, the compound oleaginous secretions contain it in considerable quantity. It is found still more plentifully in the resinous secretions, and more especially connected with the urea, in the urine, where we meet with the greatest variety of salts, and where, with the exception of the bones, the saline substances exist in the greatest proportion of any part of the body.

It has been supposed that a reference to the nature and

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that there is some connexion between the functions of the liver and the kidney, and might lead us to suppose that these organs are both of them rather excrementitious than secretory. That there is some connexion between the functions of the liver and the kidney seems to be proved by a singular circumstance stated by Mr. Rose; Ann. Phil. v. v. p. 424..7; and confirmed by Dr. Henry, ibid. v. vi. p. 392, 3; that in hepatitis there is no urea secreted by the kidney. Should this be found to be a general occurrence, it would seem to indicate, that in some way or other, the secretion of the bile is a preliminary step to that of the urea, but we do not possess any data, either anatomical or pathological, which can enable us to determine how this preliminary change is effected.

[1] The following acids are generally recognized as entering into the composition of animal substances, for the most part, however, in combination with an alkaline or earthy basis; the phosphoric, the muriatic, the sulphuric, the fluoric, the lithic, the lactic, the benzoic, the carbonic, and the oxalic, as existing in certain species of urinary calculi. To these we may add some others of more doubtful existence, such as the rosacic and the amniotic; Leibig has also announced the existence of a new acid in the urine of the horse, to which he gives the name of hippuric; Ann. Chim. et Phys. t. xliii. p. 188 et seq. There are other acids which we obtain in our examination of animal substances, as the prussic, and the mucic, but which appear to be generated during the process. Soda, potash, and ammonia, are all found in the animal fluids, the soda alone in the uncombined state. Of the earths, lime is by far the most abundant; magnesia is found in small quantity, and also silex. Sulphur, phosphorus, iron, and, according to Vauquelin, Nicholson's Journ. v. xv. p. 145, manganese, appear also to exist in some of the constituents of the animal body, which, although not properly saline, may be conveniently placed in this class, in consequence of their relation to the salts.

quantity of the saline substances that are found in the secretions, might enable us to form a natural classification of them, which would throw some light on the mode of their production, and even on the nature of secretion in general. Upon a principle of this kind, Berzelius divided all these bodies into secretions and excretions, the first being always essentially alkaline, and the latter acid[1]; but this, I conceive, would exclude many substances to which the title of secretion strictly belongs. It does not appear to me that we can lay down any arrangement of this description, which will apply to all the substances in question, but there may, perhaps, be a foundation for a division of them into four classes: 1. Those that are nearly without any mixture of salts; 2. Those which possess a definite quantity of salts, and this different from what exists in the blood; 3. Those containing salts, similar both in their nature and quantity to those in the blood; and 4. Such as contain salts different from those in the blood, and which are also variable in quantity. The fat, the saliva, the fluid discharged from the serous membranes, and the urine, may be taken as an example of each of these divisions. If we inquire in what way we are to connect the constitution of these substances with the supposed mode of their production, we may consider the two first, as the effect of proper secretion, where a substance that did not previously exist is elaborated by the action of the vessels; the third, of transudation, where, in consequence of an operation that is, in a great measure, mechanical, a certain portion of the blood is, by a kind of filtration, strained off from the mass; while the fourth will constitute the excretions, substances which consist of the residual mass of the blood, after the secretions and transudations have been removed from it. If we apply this principle to the secretions as we have found them actually to exist, we must consider the solid albuminous, the gelatinous, and the simple oleaginous, as the only substances belonging to the first class; the mucous, the fibrinous, and the compound oleaginous to the second; the liquid albuminous will belong to the third; while the aqueous and the resinous will be placed in the fourth.

A very curious and important physiological question here presents itself respecting the origin of these salts, and more especially concerning the earth of bones, whether it is actually formed in the body, or whether it is, in the first instance, received into the system along with the aliment, and after being conveyed into the blood is separated from it, and gradually accumulated in the different organs of which it afterwards forms a constituent part. The question becomes particularly interesting as it respects the physiology of some of the inferior orders of animals, and the connexion which they have with certain geological phenomena. A great proportion of the substance of several of the testacea and crustacea, consists of carbonate of lime, and it appears probable that many of the large calcareous strata which exist in different parts of the world, have originated from the detritus or decomposition of these animals. We are then naturally led to inquire what is the origin of this lime? Did it exist in some other form previous to the creation of these animals, did they receive it into their system and organize its particles, so as to mould it into their shells and crusts, or have their digestive and secreting organs the power of actually generating lime? The former opinion is the one that appears the most obvious, and accords the best with our ideas of the usual operations of nature; but it is rendered improbable from the immense quantity of matter which the animals must have appropriated to themselves, and it is not very easy to conceive in what state the lime could have existed previous to its reception into their system.

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[1] View of Animal Chemistry, p. 61, 2.

The same kind of question occurs with respect to vegetables, and although they differ so essentially from animals as to render it dangerous to extend the analogy from the one to the other, yet in this particular point, they appear to be placed in precisely a similar situation. A considerable part of the solid matter of vegetables consists of carbon, and they also contain small quantities of various earths and metals, but as these substances are insoluble in water, which is the only medium through which plants are supposed to obtain their nourishment, it has been asked, how are they procured by the plants? Are they suspended in a state of minute division in the water which is absorbed by their vessels? can they be derived from the atmosphere? or do the plants actually generate them? The question, as it respects both animals and vegetables, has been attempted to be resolved by direct experiment.

A set of experiments were performed on this subject by Vauquelin. They consisted in ascertaining the exact quantity of earthy matter which entered into the composition of the shell of the eggs, and also of the excrement of fowls; he carefully analyzed the food which they received, so as to learn precisely the quantity of earthy or saline matter which it could derive from this source, and then compared this with the quantity of lime and other earths which was found in the egg shells or the excrements[1]. Another train of experiments was performed by Dr. Prout. He examined, with his usual accuracy, the composition of recent eggs, ascertained the nature and amount of their elements, and compared these with the composition of eggs after incubation, when the chick was fully developed. There are considerable difficulties attending experiments of this kind, and they require a degree of continued accuracy which few individuals are disposed to devote to them; but in the case of the two chemists above mentioned, no deficiency of this kind can be suspected, and in both cases the results appeared to indicate that the animal had acquired a greater quantity of earthy matter than could be accounted for, except by supposing that, in some way, a portion of it had been developed by the vital powers[1].

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[1] He examined the relation which these quantities bore to each other in the male and female, and in the latter during the period of laying her eggs, and other analogous circumstances, and was led to draw the following conclusion: that a quantity of lime had been formed by the digestion and the assimilation of the oats; that a portion of phosphoric acid had also been formed: that a certain quantity of carbonate of lime had been produced; and that a small quantity of silex had disappeared; Ann. de Chim. t. xxix. p. 3 et seq.

The experiments on vegetables were conducted upon similar principles; the composition of certain plants, seeds, or bulbs, was accurately ascertained; they were placed in distilled water, or planted in clean washed sand, sulphur, or some substance whence they could not be supposed to derive any extraneous matter, and were moistened with distilled water. After they had grown for some time they were carefully analyzed, and a comparison was made of the elements which they contained before and after their germination and growth. The results of these experiments, like the former, seemed to prove that the solid matter which entered into the composition of the vegetables, in the more advanced periods of their growth must, in part at least, have been produced by some action of the vital powers, and could not have been obtained ab extra. For although it might be possible, which, however, does not appear to be the case, to refer the whole of the carbon to the decomposition of carbonic acid, as dissolved in the water employed, or diffused through the atmosphere to which they were exposed, and although a part of the earthy matter which was found in them might be derived from the soil, and suspended in the fluid which entered into their vessels; it seems very difficult, if not impossible, to explain the whole of it upon this principle[1].

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[1] Prof. Berzelius, referring to Vauquelin's experiments, unhesitatingly concludes, that the earthy substances, which are evacuated by the animals, "must be capable of being composed or decomposed, as occasion requires, by the processes of organic chemistry;" View, p. 73 et seq. Dr. Prout is led to think that the earthy matter of the bones of the chick "does not pre-exist in the recent egg;" Phil. Trans. for 1822, p. 399. The average quantity of lime in the shells of eggs was found to vary so much, that it appeared impossible to determine positively whether the earth was derived from this source by chemical analysis; but he observes, there are "very strong reasons for believing that the earthy matter is not derived from the shell." He, however, adds, with philosophic caution, "I by no means wish to be understood to assert that the earth is *not* derived from the shell; because, in this case, the only alternative left me is to assert, that it is formed by transmutation from other matter; an assertion which I confess myself not bold enough to make in the present state of our knowledge, however strongly I may be inclined to believe that, within certain limits, this power is to be ranked among the capabilities of the vital energies;" p. 400. The experiments of Fordyce, in which gold fish were kept in water that was supposed to be pure, and by being merely supplied with air, not only lived for many months, but increased very considerably in size, prove that the functions of these animals may be maintained in a perfect and healthy state for an indefinite length of time, merely by means of atmospheric air and water, but they do not exactly bear upon the question discussed in the text; Treatise on Digest. p. 78..0.

Of the nature of any powers which could produce such an effect as that described above, or even of their existence, independent of the case now under consideration, we are altogether ignorant, but should future experiments confirm the results of those that have been hitherto performed, and compel us to adopt this conclusion, it will be necessary to inquire what are the possible modes by which such extraordinary operations can be effected. In the present state of our knowledge it would be premature to enter into any long discussion upon the subject; I shall merely remark, that there appear to be only three possible or conceivable modes. Either some of the bodies, which we suppose to be elements, as for example, oxygen, hydrogen, or carbon, are in reality compounds, and are decomposed by the powers of life; or these powers are capable of converting the elements into each other; or, in the third place, there is a creation of absolutely new matter. The first of these suppositions is in itself by far the most probable, and although it appears to be directly contradicted by numerous facts of the most decisive nature, yet, on the contrary, there are certain analogies which seem rather to favour it. But the further consideration of this question will more properly belong to the subject of digestion.

SECT. 3. *Of the Theory of Secretion.*

I must now proceed to offer some remarks upon the theory of secretion. The speculations that have been formed upon this subject are very numerous, but they may be all referred to five heads: that secretion is a species of fermentation; that it depends upon the immediate agency of what is termed the vital principle; that the secretions are separated from the blood by a mechanical process; that they are produced by a chemical action either of extraneous bodies upon the blood, or of the constituents of the blood upon each other; and lastly, it has been supposed to be effected by the agency of the nervous influence.

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[1] Dr. Thomson has given us a very good summary of the experiments that have been performed on this subject in his section on the "food of plants;" Chem. v. iv. p. 320 et seq. The conjoint evidence of the experiments of Braconnot, Shrader, and Einhof, seems to prove that we cannot account for the introduction of the constituents of vegetables from the soil or water with which they are in contact. Braconnot appears to have conducted his operations with great attention to accuracy. He concludes that "organic force, aided by solar light, developes in plants substances which have been regarded simple, such as earths, alkalies, metals, sulphur, phosphorus, carbon, perhaps azote;" Ann. Chim. t. lxi. p. 187 et seq. Saussure conceives that his experiments prove that plants, during their growth, acquire an additional quantity of carbon, but he supposes that they procure it from the air; Researches, p. 50..3. The remarks of Dr. Daubeny on this subject tend to show, that plants exercise a kind of selection in the materials that are presented to them, but they do not countenance the opinion, that they actually generate the substances in question; Lond. et Edin. Journ. v. iv. *p. 52, 3.*

It will not be necessary to enter into any detailed account of the doctrine of fermentation considered as explaining the nature of secretion. It is generally supposed to have been originally advanced by Vanhelmont[1], and was adopted by Sylvius, Willis, and by the chemical physiologists generally of the sixteenth and seventeenth centuries[2]. Each gland was supposed to possess a peculiar species of fermentation, which assimilated to its own nature the blood that passed through it, as in the case of the vinous and the acetous fermentations. But it may be sufficient to remark, that we have no evidence of the existence of these ferments in the different glands, nor have we any proof that the action of the glands, or the series of changes which they produce upon the fluids that pass through them, resembles any kind of fermentation with which we are acquainted. Perhaps, indeed, when we come to inquire a little more minutely into the nature of this hypothesis, as proposed by the chemical physiologists of the seventeenth, or even of the earlier part of the eighteenth century, we shall find that it may in fact be resolved into a mere chemical change of the blood into the secreted fluid, for they used the term fermentation in a much more extensive, or rather vague manner, than it is employed in the present day, applying it to any change which is effected in the elementary constitution of the substance operated upon, when it is produced without the visible interposition of external agents, or by the action of its constituents upon each other[3].

Nor shall we find the hypothesis of the animists more satisfactory. It was zealously defended by the Stahlians of the seventeenth century, and has been received in our times, at least with certain modifications, by J. Hunter[4], Darwin,

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[1] I do not find in the writings of Vanhelmont any direct attempt to form a theory of secretion, but there are many passages in which it may be fairly ascribed to him by implication. Among others I may adduce the following: "Quomodo fermentum transmutationum parens sit, non melius, quam per Pyrotechniam inveni. Cognovi enim, quoties corpus dividitur in subtiliores atomos, quam suæ substantiæ ferat exigentia, continuo etiam sequi corporis illius transmutationem, excepto elemento. Quatenus haustum fermentum, arripiens præfatos atomos, eos alieno sui charactere imbuit, in cujus susceptione fiunt divisiones partium, quas partium heterogeneitates, et divisiones, resolutio materiæ consequitur;" taken from the treatise, "Imago fermenti impraegnat massam semine;" Ortus Med. p. 93. § 23.

[2] Haller, El. Phys. vii. 3. 28.

[3] It is in this way that the term fermentation is employed by Willis and Mayow; see the Treatise de Fermentatione by the former, and various passages in the essays of the latter. Cole, however, had a more correct and definite idea of the nature of fermentation: De Secret. c. 9. p. 32.

[4] Mr. Abernethy's sixth lecture may be perused, as an excellent illustration of what has been already noticed, the error of supposing that we have obtained any real insight into a subject, merely by employing a new phraseology: see particularly p. 244, 5.

Blumenbach, Bichat and others[1], and may perhaps be regarded as the prevailing doctrine of the London school. Yet, notwithstanding its general reception, I shall think it sufficient to refer to what I have already said respecting the vital principle and its supposed operations; we have no independent evidence of its existence, nor have we any conception of the mode of its operation, or of the general laws by which it is directed[2].

The three remaining hypotheses of secretion it will be necessary to examine more in detail, as they each of them profess to be founded upon direct facts and experiments, and as their respective supporters have attempted to point out the mode in which they operate. When the doctrines of the chemical physiologists began to be exploded, and those of the mechanical school come into vogue, the function of secretion was explained by supposing that the glands acted like filtres. It was supposed that the secretions were already formed in the blood, and that when it arrived at the secretory vessels, the various substances were mechanically strained from the mass. The obvious objection to this hypothesis was the difficulty in conceiving how mere filtration could separate so many substances from one fluid; but in order to meet this objection two speculations were proposed, for one of which it seems that we are indebted to Descartes, and the other to Leibnitz. The first of these philosophers proposed the whimsical and perfectly gratuitous supposition, that the particles of the various secreted substances were of different figures, and that the pores of the glands possessed the same figures, each gland therefore allowing those particles to pass through it which possessed the same figures with its own pores[3]. The hypothesis of Leibnitz, although equally unsupported by facts, is less palpably absurd; he compared the glands to filtres which had their pores saturated with their own peculiar substance, so as to admit of this substance alone passing through them to the exclusion of all the others, in the same manner as a paper saturated with oil prevents the passage of water, and vice versa[1].

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[1] Darwin's "Animal Appetency," Zoonomia, Sect. 37. § 2. v. 1. p. 463; Blumenbach's "Vita Propria," Inst. Physiol. Sect. 32. § 476. p. 252, and Bichat's "Vie Propre," Anat. gén. système glanduleux, art. 3. § 2. t. 2. p. 637 et seq., may be ranked among those mysterious doctrines, which it is sufficient to controvert by saying that these supposed agents have never been proved to exist, and are at the same time incomprehensible. Bichat, however, endeavours to show that the power of the nerves is not directly essential to secretion; ubi supra, p. 624..7; also Anat. gén. t. iv. p. 604..6.

[2] See p. 402. There is a considerable approach to the doctrines of the animists in Dumas' opinions respecting secretion; Physiol. t. ii. p. 33 et seq.; his chapter on secretion may, however, be perused with advantage, as containing an interesting sketch of the various hypotheses that have been formed on the subject. Perhaps also we ought to refer the hypothetical opinions which Richerand maintains respecting secretion to this head, although he supposes that the nerves are the immediate agents. He says, that the nerves give to each of the glands, "a peculiar sensibility, by means of which they discover in the blood which the vessels bring to them, the materials of the fluid which they are destined to secrete, and these they appropriate to themselves by a real selection. Besides, the nerves communicate to them a peculiar mode of activity, the exercise of which makes those separated elements undergo a peculiar composition, and bestows on the fluid which is the product of it, specific qualities always bearing a certain relation to the mode of action of which it is the result." Physiol. p. 248.

[3] De Homine, p. 18, § 11, et de Form. Fœtus, p. 212, § 25.

As our knowledge of the nature of the secretions was gradually advanced, and when the improved spirit of philosophical inquiry taught us to reject such purely conjectural speculations, it was found necessary to examine a little more minutely into the circumstances which might be supposed to operate in the production of the secretion, and to compare these with the actual state and condition of the secretory organs. Still, however, the mechanical doctrines continued to prevail among the most enlightened physiologists, and both Boerhaave[2] and Haller[3] maintained opinions respecting secretion, which are essentially of this description, although reduced into a much more rational and tangible form. Haller displays his usual candour and caution in forming his opinions upon the subject;

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[1] Quesnay adopts the hypothesis of filtres, but adds to it the operation of the nervous influence; he supposes that secretion is accomplished by the joint action of "the sensibility of the filtres" and the acrimony of the fluids; Œcon. Anim. t. iii. p. 437, 8; the section is entitled, "Affinity of the Secretory Organs with the Juice which filtres through them." Haller gives us the names of various eminent anatomists and physiologists who adopted the one or the other of these hypotheses. He is led to make the following observation, of the truth of which these pages afford us so many examples. "Sæpius monui, infelicibus exemplis expertus, raro eam esse hominum felicitatem, ut vera sint, quæ facile et sponte quasi menti se offerunt." El. Phys. vii. 3. 29. Some judicious remarks on the mechanical hypothesis of secretion are contained in a paper of Winslow's; Mém. Acad. Scien. 1711, p. 241 et seq.

[2] Prælect. § 253, cum notis.

[3] Haller's doctrine respecting secretion is contained in the first 27 paragraphs of the 3d section of his 7th book; it may be asserted that no part of his great work displays in a higher degree his extensive information and correct judgment. It is to be observed that Haller laid it down as the foundation of his hypothesis, that the secretions all exist perfectly formed, or nearly so, in the blood, El. Phys. vii. 1. 8, and that he regarded the glands in no other light than as sieves or strainers to carry off their appropriate fluids, vii. 2. 1 et alibi. His great argument is the facility with which metastases take place, which, as he supposes, proves that the gland cannot form the substance which it discharges; he particularly notices the fact that urine is found in the fluids after the destruction of the kidney, vii. 1. 9. Although not directly applicable to the function of secretion, yet I am induced to refer my readers to a paper of Balguy's, on the mode of ascertaining the doses of certain medicines, Ed. Med. Ess. v. iv. p. 33 et seq., as a curious example of the length to which the mechanical physiologists carried their doctrines. One of the rules to be observed is as follows; "You are to dose so much of the medicine as is spent on the stomach and intestines, directly as the constitution; and so much as is carried into the blood, as the square of the constitution, and the sum into the person's size is the quantity required;" p. 35. A less extravagant, but equally unfounded attempt is that of Gorter, in which he refers the whole affair of secretion to the physical properties of the fluid and the size of the vessels; De Secretione; see particularly the accompanying plate.

he states the facts upon which he reasoned, endeavours to appreciate their value, and after maturely considering the premises, he deduced from them his conclusion. He proceeds upon the principle, that all the secretions are ready formed in the blood, but he did not appear to think it necessary to inquire by what means they were originally generated there, or how they were introduced into it. Assuming, however, their existence, he conceives that there are seven causes which may contribute to their separation; 1. a difference in the nature of the blood itself; 2. the velocity of the blood as caused by the size of the vessel; 3. the transmission of the blood from one vessel to another which differs from it in size; 4. the angles at which the secreting arteries pass off from the trunk; 5. the course of the vessel, whether straight or winding; 6. the density of the vessel; and lastly, the structure of the excretory duct. There may be some foundation for all these causes, as affecting the contents of the vessels, although we shall probably conceive of them as very inadequate to produce the variety of substances that we meet with. In fact they may be all referred to the size of the vessels, and the velocity with which their contents are propelled through them; the formation of the substances, by the intervention of external agents, or the action of the constituents upon each other having been either not contemplated, or not conceived to form a necessary part of the hypothesis.

As animal chemistry was more attended to, and we became better acquainted with the changes which the component parts of the blood are capable of experiencing, by subjecting them to various chemical re-agents, it was conceived that all the secretions might be produced solely by the operations of chemical affinity. It does not very clearly appear with whom this theory of secretion originated. Perhaps Keill was the first who endeavoured to explain secretion upon chemical principles, but his opinions were altogether so imperfect, as to bear but little resemblance to the modern doctrine[1].

The main argument for this hypothesis is, that by certain chemical processes, we are able to form from the blood, out of the body, substances similar to the secretions; hence, upon the principle of not unnecessarily multiplying causes, it is said that the same kind of operation must produce the secretions in the body. And this, it is supposed, may be effected by conveying the blood in its entire substance, or any of its components separately, as occasion requires, to different parts of the system, there to be subjected to the action of external agents; or by placing the entire blood, or any of its components, in such a situation, as that it may undergo the spontaneous changes to which it is liable. An argument has been urged in support of the chemical hypothesis, derived from our knowledge of the variety of substances which may be produced from only a very few elements, merely by their being united in different proportions. This is very remarkably the case with oxygen and nitrogen, which, in one proportion, compose atmospheric air, in another nitrous oxide, in a third nitric oxide, in a fourth nitrous acid, and in a fifth nitric acid, substances which differ from each other, at least as much as the secretions differ from each other, and from the blood. And what is still more in point, some of the late investigations into the atomic constitution of animal substances, exhibit the same production of new compounds by a different proportion of their elements. Dr. Prout has, with his usual sagacity, developed this kind of relation between the three substances, urea, lithic acid, and sugar, and shown how they may be converted into each other by the addition or subtraction of single atoms of their constituents. Urea, which appears to be the substance secreted by the kidney in the healthy state of the functions, is composed of two prime equivalents of hydrogen, and one of carbon, oxygen, and nitrogen respectively; by removing one of the atoms of hydrogen, and the atom of nitrogen, we convert it into sugar, or by adding to it an additional atom of carbon into lithic acid[1].

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[1] Haller, El. Phys. vii. 3. 33. Keill, in his treatise on animal secretion, proposes to illustrate the following positions; "1. To show how the secretions are formed in the blood, before they come to the place appointed for secretion; 2. To demonstrate in what manner they are separated from the blood by the glands." p. 1. The 4th of the essays in the "Tentamina Medico-physica" is nearly a translation of the above. The reasoning is strictly mathematical, and affords a very remarkable specimen of the misapplied learning of the mathematical physiologists. The doctrine of attraction, as applied to secretion, is particularly laid down in the 7th and 8th *prop.*

There are many instances of bodies, both of vegetable and animal origin, which are converted into each other by the operation of apparently slight causes, while we have likewise substances that possess specific and sufficiently distinctive characters, which yet seem to consist of the same elements, and almost exactly in the same proportion, so as to render it probable, that a very minute variation in the affinities might originally have produced one or the other of them, or after they were produced, might transform them into each other. As examples of the first species of operation, we may adduce the conversion of farina into sugar, according to the process of Kirchoff, and of

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[1] Dr. Prout has constructed the following table for the purpose of illustrating his views on the subject; Med. Chir. Tr. v. viii. p. 540.

ELEMENTS.	UREA.		SUGAR.		LITHIC ACID.	
	Per Atom.	Per Cent.	Per Atom.	Per Cent.	Per Atom.	Per Cent.
Hydrogen ...	2·5	6·66	1·25	6·66	1·25	2·85
Carbon......	7·5	19·99	7·5	39·99	15·0	34·28
Oxygen......	10·	26·66	10·	53·33	10·	22·85
Azote.......	17·5	46·66			17·5	40·00
	37·5	100·00	18·75	100·00	43·75	100·00

fibrine into adipocere, as in the well known case of the burial ground at Paris, both of which appear to be effected merely by the addition of a small portion of water, and as an illustration of the latter position, the elementary constitution of gum and farina, and of fibrin, albumen, and jelly, may be adduced. But in reference to the above facts, it may be said, if by so apparently slight a cause, as the addition of a small portion of the elements of water, an oily substance can be produced out of the body, there would seem to be no assignable cause, why the same change might not be effected in the vessels, by placing blood in a situation where it might either procure the elements that are necessary, or part with those that are superfluous. That changes of that description can take place in the fluids, while they are still circulating in the vessels, is proved by the action of the air upon the blood in the lungs, for whatever be our theory of the ultimate effect of respiration upon the system, we cannot doubt, that the air, by its chemical agency, is instrumental to the conversion of venous into arterial blood, while we have found it probable that the vessels of the lungs are capable of both absorbing and discharging certain substances through their coats.

It is further urged in support of the chemical theory, that the blood is a substance which we suppose to be peculiarly well adapted to experience changes of this description; it is composed of a number of ingredients, which are held together by a weak affinity, liable to be disturbed by a variety, even of what might appear the slightest causes. And although we have been induced to conclude that mechanical causes alone are inadequate to produce the changes which take place in the blood, they may have considerable effect in modifying the operation of the chemical actions. For example, they may not only bring substances into contact or proximity, but they may render this contact more or less extensive, and may continue it for a longer or shorter space of time; they may subject the substances to various degrees of pressure, and may propel them through the vessels with various degrees of velocity, and in this way, they may not only determine the amount of effect to be produced, but they may vary the nature of it by protracting the action, or arresting it in different stages of its progress, and in this way infinitely vary the results. Now all these incidents must happen to the blood in the different parts of the circulation; it passes through vessels of various diameters, and with various degrees of velocity, and in short is subjected to all the actions which can arise from mere mechanical operations.

In order to assist us in conceiving how a variety of substances may be produced from a single compound, by the intervention of physical causes alone, we may suppose a quantity of the materials adapted for the vinous fermentation to be allowed to flow from a reservoir, through tubes of various diameters, and with various degrees of velocity. If we were to draw off portions of this fluid in different parts of its course, or from tubes which differed in their capacity, we should, in the first instance, obtain a portion of unfermented syrup; in the next we should have a fluid in a state of incipient fermentation; in a third, the complete vinous liquor; while in a fourth, we might have acetous acid. To apply these remarks to the blood; we know that mere rest allows it to separate into serum and crassamentum, and that if the crassamentum be formed under certain circumstances, the red particles are detached from the fibrin, which is left in a pure state. Then, when we consider how many re-agents have the power of coagulating albumen, we shall not find it very difficult to conceive that this process may take place in the minute capillaries, and according to the degree in which the coagulation takes place, and consequently, in which the albumen is separated from the serum, so shall we have the serosity left, containing a certain quantity of albumen affording us the different kinds of fluids which compose the albuminous secretions. We farther find that by the action of dilute nitric acid upon fibrin and upon albumen, we are able to convert these substances respectively into adipose matter and jelly, changes which are probably effected by adding oxygen to the fibrin, and to the albumen; and there is some reason to believe, that by applying the same re-agent to the red particles we may produce a substance nearly resembling bile. Such facts as these seem to warrant us in concluding, that certain chemical operations, which it is not unreasonable to suppose may take place in the blood, can form from it some of the substances, which in the ordinary course of the animal œconomy, are the result of secretion. That we are not able to imitate all the secretions, can scarcely be regarded as any decisive objection to the hypothesis, because it can only prove our ignorance of the intimate nature of some of the operations, without affecting our opinion respecting the means by which they are produced.

If we adopt the chemical theory of secretion, we must conceive of it as originating in the vital action of the vessels, which enables them to transmit the blood, or certain parts of it, to the various organs or structures of the body (the blood itself being previously prepared by the process of assimilation, and adapted for its appropriate functions, one of the most important of which is the formation of the secretions) where it is subjected to the action of those re-agents which are necessary to the production of these changes. The re-agents themselves are, at least, in some cases, conveyed to the blood by muscular action, or by other vital operations, analogous to the contraction of the diaphragm, by which the air is received into the thorax. The remaining part of the process will be strictly chemical, or such as the substances would exercise upon each other, whenever they were placed in the same relative circumstances[1].

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[1] I may refer my readers to Adelon's strictures on the chemical theory of secretion, *in Dict. de Méd. t. xix. p.* 225 et seq.

The last hypothesis of secretion which I proposed to notice, is that which ascribes it to the action of the nerves. It is supported partly by a number of facts and observations, which tend to establish a general connexion between secretion and the nervous influence, and partly upon certain experiments that have been performed in order to show that the secretion of particular glands is deranged or destroyed by depriving them of their nerves. With respect to the first point, there are many well known occurrences, which clearly indicate an intimate connexion between the action of the nerves and the formation of the secretions. Certain secretions are increased in quantity and have their qualities materially altered by the intervention of mental emotions, and of various agents which we can only conceive to operate through the medium of the nerves.

So many examples of this kind will immediately suggest themselves to the mind, as to render it scarcely necessary to particularize them. The secretion of tears from the impressions of grief, and the encreased flow of saliva from the idea of grateful food, are among the most familiar instances of this nature. The process of digestion, as connected with the secretion of the gastric juice, is peculiarly liable to be affected by the state of the nervous system, so that, according to its excitement or depression, the fluid appears to be produced in greater or less quantity, the functions of the stomach being proportionably depraved, or even entirely suspended. The secretion of milk seems also to be very much under the influence of the nervous energy; not only does the mechanical excitement of the termination of the excretory duct increase the action of the gland, by an operation which must have been conveyed to the part through the nerves, but the increased flow of milk may be produced by causes which can only act upon the mind of the mother. There are many known occurrences of an opposite nature, which equally illustrate the subject under consideration, where the secretion of a gland is diminished or entirely stopped, in consequence of the removal of the exciting cause, which cause must have acted through the medium of the nerves. The case of the mamma may be again cited as a striking illustration of this position, for by the removal of the young from its mother, the secretion of milk is, after a short time, entirely suspended, in circumstances where the gland would otherwise have continued its action for an indefinite length of time[1].

These facts, and many others of a similar description prove a close connexion between the action of the glands and that of the nerves, but they do not absolutely prove any thing respecting the intimate nature of secretion; they only afford us a new illustration of what has so frequently fallen under our notice, that the different systems of which the body is composed are closely connected together. Nor is there any thing which ought to excite our surprise in these facts, nor do they appear, in any respect, adverse to the principles which we have laid down; for it cannot be thought more remarkable that a change in the effect should be induced by a change in the degree of the operating cause, than that the cause itself should originally be able to produce the effect[1].

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[1] Quart. Journ. v. i. p. 165, 6.

An attempt was made by Sir Ev. Home, to establish a more intimate connexion between the action of the nerves and the secretory organs, by the intervention of the galvanic influence. The extraordinary power of decomposition which this agent had been discovered by Sir H. Davy to possess, taken in connexion with the electrical apparatus which is found in certain animals, suggested the idea that a similar kind of operation might be employed to act upon the blood, while the extreme sensibility of the nerves to the stimulus of galvanism, pointed them out as the parts that were the best adapted for effecting the change[2]. Dr. Wollaston, about the same time, formed a similar idea with respect to the agency of galvanism on the animal fluids, and illustrated his opinion by a very ingenious experiment, in which a low galvanic power produced the decomposition of a saline solution, and caused its component parts to transude through a portion of bladder, each of them being separately attracted to the corresponding wire in the interrupted circuit. Upon this experiment he remarks that the quality of the secreted fluid may probably enable us to judge of the electrical state of the organ which produces it, as for example, "the general redundance of acid in urine, though secreted from blood that is known to be alkaline, appears to indicate in the kidney a state of positive electricity; and since the proportion of alkali in bile seems to be greater than is contained in the blood of the same animal, it is not improbable that the secretory vessels of the liver may be comparatively negative[3]. Some experiments were afterwards performed by Sir B. Brodie, which seemed to indicate a close connexion between the secretion of the glands and the integrity of the nervous system[1], but scarcely any attempt was made to ascertain the extent of this connexion, or directly to prove its existence, until Dr. Philip entered upon the investigation.

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[1] There is a fact that occurs with respect to birds, which ought probably to be referred to the influence of the nervous system over an organ of secretion, and which illustrates the extent of this influence in a remarkable manner, at the same time that it proves it to be exercised over a part that might have been supposed to be beyond the reach of its action. I refer to the formation of eggs in the ovarium. A bird which, when left in its natural state, would lay a few eggs only, if they be removed as they are produced, may be made to lay almost an indefinite number of them, and apparently without any derangement or injury to the system, or without the application of any external exciting cause. The only explanation that we can offer of this fact seems to be, that the instinct of the animal leads it to continue depositing its eggs in the nest until a certain number are accumulated; of the mode of operation in this case we are entirely ignorant, but we can scarcely doubt that it must be by the intervention of the nervous system. Berger, de Nat. Hum. p. 121..3; and Bordeu, Sur les Glandes, § 98 et seq., may be consulted with respect to the influence which the nerves possess over the action of the secretory organs.

[2] Phil. Trans. for 1809, p. 385 et seq.

[3] Phil. Mag. v. xxxiii. p. 488..0.

He has endeavoured to identify secretion with the immediate result of the nervous influence by a series of very interesting and curious experiments on the effect that is produced upon the secretion of the gastric juice, by the division of the par vagum.

In the chapter on respiration I had occasion to refer to the effects which had been observed to follow from the division of these nerves, one of which was stated to be the derangement of the functions of the stomach. Blainville appears to have been one of the first experimentalists who minutely attended to this circumstance; it was subsequently noticed by Legallois[2], and Sir B. Brodie[3], and Dr. Philip, on carefully repeating the experiment, for the express purpose of noticing the effect on the stomach, announced, that after this operation, the digestive process was entirely suspended[4]. But notwithstanding the apparently simple and decisive nature of the experiment, the fact was controverted by physiologists of great respectability, who asserted, that after the division of the par vagum, the digestion was continued nearly in the natural state, or at most was only slightly retarded, so as to require rather a longer time for its completion. This opposition of opinion gave rise to a very warm controversy, in which experiments were adduced on both sides with equal confidence, and apparently with equally decisive results[5], when at length the curious circumstance, to which

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[1] Phil. Trans. for 1811, p. 38, 9; and for 1814, p. 104 et seq. In the first of these papers Sir B. Brodie found that the secretion of urine was suspended by the removal or destruction of the brain, although the circulation was maintained by the inflation of the lungs; in the second paper it is stated that when an animal is destroyed by arsenic, after the division of the par vagum, all the usual symptoms are produced, except the peculiar secretion from the stomach. Sir B. Brodie's conclusion from his experiments was, that the nervous influence is a step in the process of secretion, but he does not decide that it is absolutely necessary to it. It appears that Berzelius had announced, some years before, his opinion of the connexion between the nerves and the function of secretion; See Phil. Trans. for 1809, p. 385; and that an hypothesis somewhat similar to Sir E. Home's had been advanced by Dr. Young in his lectures; Med. Lit. p. 110, 1.

[2] Sur la Vie, p. 214, 5.

[3] Phil. Trans. for 1814, p. 102 et seq.

[4] Inquiry, p. 119..125.

[5] This controversy was carried on principally in the Quarterly Journal, v. vii. p. 161 et seq. and 349 et seq.; v. ix. p. 197, 8; v. x. p. 292 et seq.; v. xi. p. 45 et seq. and p. 320 et seq.; v. xii. p. 17 et seq. and p. 96, 7; it was conducted by Dr. Philip and Dr. Hastings on the one side, and by Sir B. Brodie and Mr. Broughton on the other. The whole series of papers is well worth perusal, both as containing a number of curious facts, and still more as suggesting many important reflections on the mode in which philosophical discussions ought to be conducted. The experiments have been since repeated at Paris, by Breschet, Milne Edwards, and Vavasseur; when Dr. Philip's fundamental positions were fully confirmed; see Archives Générales

I have already referred, was discovered, that the mere division of the nerves, and even the retraction of the divided ends, for the space of one-fourth of an inch, does not prevent the influence from being transmitted along them to the stomach, but that if a portion of the nerve be actually removed, or the ends folded back, the digestive process is suspended in the manner that was described by Dr. Philip[1]. The original proposition of Dr. Philip is therefore established, that if the par vagum be divided in such a manner as effectually to intercept the passage of the nervous influence, the digestion is suspended, and as this operation is supposed to be brought about by the action of the gastric juice, which is secreted from the surface of the stomach, it was concluded by him, that secretion is necessarily connected with the nervous influence, and cannot be performed without its intervention.

It was in connexion with the inquiry respecting the effect produced upon the stomach by the division of the par vagum, that Dr. Philip made his curious discovery concerning the power of galvanism, in supplying the place of the nerves. The influence which the electric fluid, as excited by the galvanic apparatus, appeared to exercise over the muscles, as well as other circumstances, which seemed to point out a connexion between this agent and certain of the animal functions, induced Dr. Philip to inquire whether the analogy could not be extended, and whether, not only the contractility of the muscles, but the more characteristic effects, which he supposed to be necessarily dependent upon the nervous influence, might not be produced by galvanism. The secretion of the gastric juice

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de Médécine, for Aug. 1823, also Edwards de l'Influence, &c. p. 527. We have, however, a still later series of experiments performed by Breschet, and Milne Edwards, in which they somewhat modify their former results. They suppose that the complete division of the par vagum, even with the precautions employed by Dr. Philip, although it very materially affects the process of chymification, does not entirely destroy it; they conceive that this diminished effect does not depend upon the absence of the gastric juice, but upon a paralysis being induced upon the muscular fibres of the stomach, in consequence of which the different parts of the alimentary mass are not duly brought into contact with the coats of the stomach, so as to be exposed to the action of its secretions, that the effect of the galvanic influence is to restore the due action of the fibres; they inform us that a mechanical irritation applied to the lower end of the divided nerves produces a similar kind of change upon the food introduced into the stomach; from which they finally conclude, that the use of the par vagum, as connected with the functions of the stomach, is to bring the alimentary mass into sufficient contact with the gastric juice: Arch. Gén. de Méd. The experiments are related with a sufficient degree of minuteness, and bear every mark of accuracy; but upon being repeated in London by Mr. Cutler, under the inspection of Dr. Philip and Sir B. Brodie, so far as the main point was concerned, the effect of mechanical irritation of the lower part of the divided nerve, the results did not correspond with those of Breschet and Edwards; Med. Chir. Rev. v. iii. p. 589, 0.

[1] Phil. Trans. for 1822, p. 22, 3; Quart. Journ. v. xi. p. 325..7, and v. xii. p. 19, 0.

appeared to offer an unexceptionable mode of putting his hypothesis to the test of experiment. For this purpose, after dividing the par vagum, a portion of the lower end of the nerves was coated with tin foil, and a silver plate placed over the stomach of the animal; the tin and silver were then respectively connected with the opposite extremities of the galvanic apparatus. It was found, by this arrangement, that the animal seemed to be entirely free from the various distressing symptoms which always, in a greater or less degree, attend the division of the nerves, and upon examining the contents of the stomach, after the death of the animal, the food appeared to be perfectly digested, affording a complete contrast to what was contained in the stomach of a similar animal, in which the nerves had been divided, but which had not been subjected to the galvanic influence[1].

The singularity of the result, and the important consequences which were deduced from it, caused this experiment, like that of the division of the nerves, to be repeated by various physiologists, and although the facts were, in the first instance, warmly controverted, they appear to be now fully confirmed. Dr. Philip then proceeded to examine how far the other effects which he ascribed to the nervous influence, could be imitated by galvanism, and with this intention he made the experiments of which I have given an account in the last chapter[2], on the evolution of heat from arterial blood by this agent. From the whole of his observations and experiments, taken in conjunction with each other, he conceived himself warranted in the conclusion, that every effect of the nervous influence might be produced by galvanism, and, in short, that the two agents are, strictly speaking, identical[3].

According to Dr. Philip's view of the subject, we have two positions, the truth of which appears to be involved in the hypothesis of secretion which he supports, first, that the nervous influence is essential to this function, and second, that the nervous influence is identical with galvanism. As, however, these positions are in themselves completely distinct, it may be desirable to examine them separately, in order that we may more clearly comprehend the nature of the evidence on which they each of them rest. With respect to the first of these points, the dependence of the function of secretion upon the nerves, it appears to be the direct inference from the result of the division of the par vagum, and it must be admitted that the argument has been very forcibly urged by Dr. Philip, and that the reasoning which he employs, and the facts which he adduces in its support, are very impressive, and appear at first view almost unanswerable. Yet, perhaps, upon reflection, we shall be induced to think that the conclusion does not so inevitably follow from the premises, as has been supposed to be the case. The clear and legitimate inference from them is, that the nervous influence has a necessary connexion with the action of the glands, but in the particular case of the stomach it is not very easy to trace out the series of changes which takes place, in such a manner as to show how far the connexion is essential, or how far it is only what may be deemed incidental. In order to prove that the latter is the case, it is only necessary to bring forwards one unequivocal example of a secretion being produced, where there can be no intervention of nervous influence, but of this we have numerous instances in many of the classes of the lower tribes of animals, in which no nervous system has yet been detected[1]. This I conceive to be so direct an argument, as to be sufficient to counteract the force of any individual facts or experiments, however striking, which may be urged in favour of the contrary opinion To assert that these animals have a nervous system, because they exercise those functions which have been generally supposed to be affected by means of the nerves, when no organs of this kind can be detected, and when the animals are of such magnitude as that their structure can be distinctly examined, is a mode of reasoning, which I conceive to be so palpably incorrect, as to require no formal refutation[1]. And, indeed, we can require no more striking illustration of a secreted fluid being produced without the intervention of the nerves, than the experiment itself which we are now considering; for we find that after the complete division of the nerves, when the action of the gland is either suspended, or at least altogether deranged, it may be restored, and maintained, by the intervention of a physical agent, as far as appears, in its perfect and natural condition.

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[1] See the original experiments in the "Inquiry," p. 223..237; ex. 70..4. Many experiments on the subject will be found in the references to note (8). A digested summary of the hypothesis is contained in a paper in the Quarterly Journ. v. viii. p. 72 et seq.

[2] See page 470.

[3] For the correct conception of this hypothesis it is necessary to bear in mind, that Dr. Philip supposes what he styles the nervous functions, to be confined to the four following operations, "stimulating the muscles, both of voluntary and of involuntary motion, conveying impressions to and from the sensorium, effecting the formation of the secreted fluids, and causing an evolution of caloric from the blood;" Inquiry, p. 220. Perception and volition he denominates sensorial functions, supposing them to be more immediately connected with, or dependent upon the brain, as distinguished from the nerves. See the 10th chapter of the Inquiry; also Quart. Journ. v. xiii. p. 97, and v. xiv. p. 91..3; and Phil. Trans. for 1833, p. 68 et seq., where we have Dr. Philip's most matured view of the subject.

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[1] Cuvier, Leçons, t. ii. p. 361..3; Lamarck, Anim. sans. Vert. t. i. p. 24, 5. Perhaps we may adduce, in this place, the cases that are on record of monstrous or deformed fœtuses being born, with many of their organs fully developed, in which the nervous system was entirely wanting; one of the most remarkable of these is that detailed by Clarke in the Phil. Trans. for 1793, p. 154 et seq.; the author fairly infers that this system cannot be essential to the functions of the fœtus, and to secretion among the rest. We may, however, conceive that the fœtus may possess some supplementary action derived from the mother. I have already had occasion to refer to Mr. Lawrence's valuable paper "on monstrous productions," where other cases are related analogous to the above; Med. Chir. Tr. v. v. p. 165 et seq.; in all these instances the secretions appeared to be produced as if the fœtus had possessed the natural and perfect structure. Several cases of this description are also related in the different volumes of Mém. Acad. Scien.; in the one for 1711, p. 26, there is a notice of a fœtus by Fauvel, where the cerebrum, cerebellum, and spinal cord were wanting; the writer remarks, that this case affords "une terrible objection aux esprits animaux," and we may add an equally forcible one to any hypothesis which supposes the nervous system to be essential to what are usually termed the vital and natural functions. There is a notice of a case by Mery in the vol. for 1712, p. 38; a more ample account of another by the same, in 1720, p. 8 et seq.; and one by Winslow in 1724, p. 586 et seq., who also subjoins other cases to his paper. The first of Mery's cases, which was without either the brain or the spinal cord, is said to have lived for 21 hours, and to have taken food; the act of deglutition must of course have been performed. We have another remarkable case by Le Cat, of which there is a translation in the Phil. Trans. for 1767, p. 1 et seq.; although the whole of the upper part of the body was deficient, there appear to have been an imperfect spinal cord and cerebellum. There is a well

And, indeed, should it even be proved, that the nerves are essential to the secretion of the gastric juice, it would only tend to establish this particular fact, while we have so extensive a range of operations, in which secretion is always going forwards in situations or under circumstances where it appears impossible that the nerves can have any influence. The question, therefore, respecting the influence of the nerves in secretion, appears to be precisely analogous to that respecting the intervention of the nerves in muscular contraction, which was discussed in the fourth chapter[2]. That the nerves are very frequently concerned in both the operations, is admitted, but the point to which we are to direct our inquiry is whether there are any cases in which muscular motion and secretion can be performed without the

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narrated case of this kind in the Phil. Trans. for 1775, p. 311 et seq. by Cooper; Hewson appears to have assisted at the examination; we are told, p. 314, that "upon a careful inspection there is evidently no brain or spinal marrow." I may remark, that Sir Ev. Home, in opposition to the authority of Cuvier and Lamarck, as referred to at the commencement of this note, asserts, "that in the most simple animal structures endowed with life, large enough to admit of dissection, brain and nerves are met with." Phil. Trans. for 1825, p. 257. Béclard has published a valuable memoir on acephalous fœtuses in the Journ. de Méd. pour 1815; it contains a very copious list of well authenticated cases of this description. In addition to these we have a curious case by Sue, Recher. Physiol., and two cases by Breschet, in Magendie's Journ. t. ii. p. 269 et seq. The subject is treated in a very ample and judicious manner by St. Hilaire, in the 2d volume of his Philos. Anat. § 4. p. 77 et seq.; and by Breschet, Dict. de Méd., Art. "Acephalie," t. i. p. 255 et seq.; see also the art. "Acephalie" by Blandin, Dict. Méd. Chir. prat. t. i. p. 178 et seq.; to this article is appended a copious list of references.

[1] The analogy of the vegetable kingdom may, I conceive, be fairly applied to the illustration of this question; for although we may regard its powers and functions as essentially different from those of animals, yet on this particular point they may admit of comparison.

[2] P. 171 et seq.

co-operation of the nerves; a single unexceptionable example of this kind is sufficient to decide the controversy[1].

We are now brought to the second of Dr. Philip's positions, the identity of the nervous influence and galvanism, and I think it must be admitted, that if we could prove this identity, some of the most formidable objections against the nervous hypothesis would be removed. And here, as in the former case, although I acknowledge that Dr. Philip's arguments are very clearly and ingeniously stated, and very happily enforced by his experiments, yet I conceive that there are some points in which they are essentially defective. The argument is, that we are able to imitate all the operations which properly belong to the nerves, by galvanism, and that therefore the two agents must be identical. But a moment's reflection will convince us, that the apparent similarity of two effects is not sufficient to prove the identity of the causes. The coagulation of albumen is produced by heat, by galvanism, and by various chemical re-agents, yet we are not warranted in asserting that heat, galvanism, and all the chemical re-agents in question are identical. The sensation of sight is excited by light, by galvanism, and by a blow on the eye, but we do not suppose that light, galvanism, and mechanical violence are identical. We conclude, in these cases, that the first set of causes act in some way upon the albumen, and the second set upon the optic nerve, so as ultimately to affect the albumen and the optic nerve in the same manner. The mere similarity in the effect is therefore no proof of the identity of the causes, employing the term in its ordinary acceptation, the means which we make use of to produce the change in question, but of the minute and intimate operation of which we are frequently altogether ignorant.

But in the case under consideration it will be said, that we have not merely a single coincidence of an apparent cause and effect, but that there are two agents, one of which seems to produce all the characteristic effects of the other, so as to increase in a very high degree the probability of their identity. The force of the argument will therefore depend upon the number of these coinciding circumstances and the accuracy of the resemblance. In the case of the nervous influence, there are four of its characteristic effects pointed out by Dr. Philip[1], all of which are said to be capable of being imitated by galvanism, a circumstance which must add very great weight to the reasoning, provided the fact be clearly made out in each individual instance. In the first case, that of the production of the gastric juice, the resemblance of effect is undoubtedly very remarkable; but even here, I do not think that we are warranted in applying the same reasoning to all the other secretions, by an inference from that of the gastric juice alone. We are very little acquainted with the nature of the gastric juice itself, or of the mode of its operation upon the food, and although it is, upon the whole, highly probable, that the stomach is furnished with glands, which secrete a fluid that acts somewhat in the manner of a solvent upon the aliment, yet it is rather by inference than by direct observation that we arrive at this conclusion.

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[1] I may refer the reader in this place to a judicious essay by Dr. Alison, Quart. Journ. v. ix. p. 106, and to the remarks in his Physiol. p. 71..3, the object of which is to controvert Dr. Philip's hypothesis of secretion. The line of argument which I have employed in the text is somewhat similar to Dr. Alison's, but he has not always used the term nervous action or nervous influence in the same sense with Dr. Philip, which I have thought it necessary to do in discussing this question, whatever opinion I might be induced to form of its propriety or utility. See also Bichat, "Sur la Vie," p. 244..7, and some very judicious remarks by Mr. Shaw, Lond. Med. Journ. v. xlix. p. 454 et seq. The question of secretion is considered by Adelon, in his Physiol. t. iii. p. 461 et seq., and is referred to the vital action of the vessels. On the general question of the galvanic theory of nervous action, see Dr. Alison, Physiol. p. 149, 0. I will also beg to refer to the appendix to this chapter, which I have retained from the former edition.

And although we find that when the par vagum is divided, the food remains undigested, so as to render it probable that the gastric juice is not duly secreted, still we find that the surface of the stomach and the lungs pours out a fluid, indeed probably in a greater quantity than natural, although its physical and chemical properties are not the same as when the nerves are entire. The very circumstance of the anatomical structure of the par vagum, which renders them so well adapted for the experiment, seems to point out that there is something peculiar in their nature or their action, and that they either perform other functions besides that of secretion, or that there is some peculiarity in their secreting power, which is connected with their peculiar anatomical disposition[2].

If we succeed in weakening the inference which is deduced from the first case of coincidence, that of the secretion of the gastric juice, I think the others will not be found sufficiently powerful to support the argument. I have already made some remarks on the experiments in which heat appeared to be evolved from arterial blood, by the application of galvanism[3]; and with respect to the power of this agent in exciting muscular contraction, when we consider in how many ways this effect is produced, we shall think it quite sufficient to abide by the old opinion, that galvanism, in these cases, acts as a subtile stiimulus, without venturing to decide upon the nature of the operation.

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[1] Inquiry, p. 116, 7; Quart. Journ. v. xiv. p. 91, 105.

[2] Analogy would induce us to suppose that if nervous action were necessary for secretion, it would be derived rather from the ganglionic, than from the cerebral or spinal nerves. See the remarks of Mr. Shaw, as referred to in the last page. So far as the question can be elucidated by anatomy, we are informed that although numerous nerves go to the glands, they evidently pass through, or by them, to be ultimately distributed upon other parts: see particularly Haller, El. Phys. vii. 2. 2. He likewise informs us, as the result of his experiments, that little sensibility is manifested by the glands when different stimuli are applied to them; Mém. Sur les Part. Irrit. et Sens. t. i. p. 39. 59.

[3] P. 457.

With respect to the power of galvanism in conveying impressions to and from the sensorium, which it is stated to possess in common with the nervous influence, I do not perceive that Dr. Philip has advanced any direct facts in proof of this point. The power of exciting muscular contraction, by transmitting the galvanic influence from the origin of a nerve to its extremity, or of producing an impression on the sensorium, by transmitting the influence in the opposite direction, if these be the circumstances from which the conclusion is derived, would appear to prove no more than that galvanism is a stimulus both to the contractility of the muscles, and the sensibility of the nerves, a circumstance which is by no means peculiar to it, or characteristic of its effects.

Although the supposed improbability of an opinion should not be allowed to stand in opposition to the evidence of decisive and unequivocal facts that may be adduced in its favour, yet, in cases of this kind, we naturally require stronger evidence and more decisive facts than under ordinary circumstances. On this account, it is necessary to inquire into the antecedent reasonableness of any physiological doctrine, independently of the experiments or observations that are brought forwards in support of it, to ascertain how far it coincides with our ideas of the other operations of the animal œconomy[1]. The doctrine of the identity of the nervous power and galvanism necessarily leads us to the conclusion, that the use of the nerves, as distinguished from the brain and spinal cord, is to conduct electricity, that all the operations which are supposed to be produced by the nervous communication of the different organs must be resolved into the conveyance of electricity from one to the other, and that when an impression is made upon the extremity of a nerve which is communicated to the sensorium, whatever may be its nature, it is effected by the intermedium of the electric fluid, and in like manner the same agent must be the medium of communication from the brain to the extremity of the nerves. When, for example, mechanical violence, or a chemical acrid irritates the surface of the body, when light acts upon the retina, or the undulations of the air upon the nerve of the ear, the pain excited, the visible impression produced, and the perception of sound, are all conveyed to the sensorium by galvanism; and when we form a volition, and by means of it produce the contraction of a muscle, the will must act upon the galvanism which resides in the nerve that belongs to the part, and cause it to contract, while the same agent must again pass up from the muscle along the nerve to the brain, and communicate to it the perception or consciousness that the volition has been executed.

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[1] Dr. Philip, in speaking of the distinction between the sensorial and nervous powers, uses the following expressions; "However blended the organs of the sensorial and nervous powers may appear to be, we are assured that they are distinct organs, by the fact, that while the organs of the nervous power evidently reside equally in the brain and spinal marrow, those of the sensorial power appear to be almost wholly in man, and chiefly in all the more perfect animals, confined to the former." Quart. Journ. v. xiv. p. 93. Upon the hypothesis of the identity of the nervous power and galvanism, the preceding remark must, I conceive, imply that there is in the brain and spinal cord, an apparatus provided for the accumulation or evolution of electricity, from which it may be transmitted, when required, along the nerves, and to which it again returns, when it conveys impressions from the extremities to the centre. This idea seems to be farther countenanced by another expression in the same essay, p. 91, that there is "no evidence that impressions are ever communicated from one nerve to another, independently of the intervention of one of these organs" (the brain and spinal cord). I think it would be difficult to point out any structure in either of these parts, which can be supposed to be appropriated to this purpose, nor will it, I apprehend, be easy to show, why the currents should not as readily pass from one nerve to another, as in the course which we observe the nervous power to follow. It is often curious to observe some crude indications of the most recent, and apparently original doctrines, among the older authors. Sauvages says, that many experiments prove that the nervous influence is nothing more than electrical fluid, charged with some particles of extremely attenuated lymph; Œuvres Div. t. ii. p. 233.

There is certainly a clear foundation for the distinction which Dr. Philip has laid down between the nervous and the sensorial powers, or the functions of the nerves and of the brain, although, perhaps, there are certain cases in which we may doubt, whether the line which he has traced out be, in every instance, quite correct. But admitting of the division in its fullest extent, it seems scarcely reasonable to conclude, that the actions of these organs depend upon totally different principles, that the nerves operate merely through the intervention of electricity, while it must be supposed that the brain can have no farther connexion with this agent, than to receive the impressions which it makes upon it. It would appear more natural to suppose that the mode of operation of the nervous system should be similar in all its parts[1], although, with certain modifications or additions, each of them, as we presume, possessing every power or property of the other, together with what may be necessary for the exercise of those functions which belong to it exclusively. This community of properties might be expected to exist more particularly with regard to those nerves which are immediately connected with the brain or the spinal cord, which parts, from their connexion with each other, it is natural to suppose must possess, to a certain extent, the same properties. It has been found that the power of transmitting the galvanic influence, if not confined to certain nerves, is at least much more remarkable in some of them than in others, and this difference exists in so great a degree, that many eminent physiologists have been unable to excite any contractions in the nerves that are connected with the ganglia, and which are not under the control of the will. Yet if the nervous power be identical with galvanism, there appears to be no assignable reason, why these nerves should not be at least as sensible to this stimulus, as the nerves that belong to the voluntary organs, since they cannot be supposed to be deficient in the mere nervous functions, although they may not be possessed of those which are confined to the sensorium.

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[1] In making this observation, it is to be understood, that I do not mean to refer to the intellectual functions which are attached to the brain, but those which it possesses as a mere vital agent.

We have frequently had occasion to remark upon the great diversity in the nature of the substances which act as stimulants to the muscles; and although I have endeavoured to establish the doctrine of their independent contractility, yet, at the same time, it was shown that, in a great number of instances, the stimulating substances act through the intervention of the nerves; but if we are to regard the nervous power as identical with galvanism, it will follow that all these agents, mechanical, chemical, and vital, must operate through the medium of electricity, a supposition which appears quite repugnant to our ideas of their physical relations, and of the nature of electricity itself. And I may observe that the difficulty, with respect to animals that are without a nervous system, is scarcely, if at all diminished, by supposing that the nervous power is identical with galvanism; for we have the effects that are ascribed to galvanism produced without the existence of the nerves, which are supposed to be the channels through which this agent is conveyed. Should we go a step farther, and maintain that galvanism is capable of being transmitted through every part of the body, as well as through the nerves, we may indeed elude the present difficulty; but we become involved in the contradiction of supposing that nerves are necessary for the performance of a certain function, because they serve to convey the galvanic influence, yet that other parts of the body, as well as the nerves, are adequate to this conveyance[1].

Upon reviewing the subject of the theory of secretion, the inquiry, when considered in the abstract, may be stated as follows. We have a fluid possessed of certain properties and consisting of certain components, from which various other substances are produced, the greatest part of them composed of the same elements with the primary fluid, but in different proportions; by what means are these secondary substances formed? we may conceive of both chemical and mechanical agencies being concerned in these operations; if the substances produced are identical with any of the constituents of the primary fluid, or even very similar to them, it may appear probable that the operation is principally mechanical, whereas if the secondary substance differs considerably from any of the constituents of the primary fluid, we should naturally suppose that it has been produced by a chemical affinity, or by the combined effect of chemical and mechanical action. We must next inquire, how far the different modifications of chemical and mechanical actions, which may be conceived to exist, are sufficient to produce all the effects which we actually observe to take place, and to this inquiry we can only reply, that the present state of our knowledge on the subject of animal chemistry does not allow us to go farther than to say, that the changes which have been actually produced are very numerous, and that those which may be supposed possible are still more so, and that if there be any which we cannot explain, they will probably be equally difficult to account for upon any other principle. On this view of the subject, therefore, the question will rather be an appeal to our ignorance, than to any principle which can direct our judgment in deciding upon this point.

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[1] On the theory of secretion generally, and on Dr. Philip's hypothesis in particular, I shall refer my readers to the judicious remarks of Dr. Prichard, in the 8th section of his Essay on the Vital Prin., with note No. 2.

In the fourth place we must ask, in what manner are these supposed chemical or mechanical changes connected with the operations of the living system; which of the vital powers are called into action, and through what medium are they excited? When we consider the mere act of secretion, to which our present inquiry extends, I should say that contractility is the only vital power which is essential to the operation. The action of the heart, in the first instance, propels the blood into the capillaries; it is transmitted through these with different degrees of velocity, and subjected to various modifications of compression, so that its constituents are more or less intimately mixed together; some of its finer parts are transmitted into vessels too minute to admit of those that are more viscid or tenacious, while at the same time it may be supposed to experience various alterations from changes of temperature, from the action of the atmosphere, or from the mixture of the different secretions with each other. Then, although for the reasons stated above, I conceive that the action of the nerves is not essential to secretion, it is sufficiently obvious that the organs of secretion, in the higher orders of animals, are very much under the influence of the nerves, and are, in many cases, materially affected by them, so that we are in possession of an additional agent, by which we may multiply the number of possible combinations of elements, and produce a corresponding number of new substances. Still, however, we are to bear in mind, that we can form no clear conception of any mode in which the nerves can act upon the organs of secretion, except through the medium of the circulating system, so that here again we reduce the primary operation to the contractility of the muscular fibre, notwithstanding the share which the nervous system may have in the effects, considered as a secondary agent.

A great difficulty which remains to be obviated, respects the formation of the saline secretions, or of those substances, the elements of which are not to be found in the blood, or at least not in sufficient quantity to account for the great accumulation that takes place in certain parts of the system, and where we are unable to point out any means by which they can have access to it. This is a difficulty which, I confess, appears at present insurmountable, and it may be said, that it attaches equally to every hypothesis that has been proposed; for it is at least as difficult to say in what manner the action of the nerves should produce lime from the blood, as how it should be produced by any operations of chemical affinity. To suppose that we are affording any real explanation of the phenomenon by ascribing it to the operation of the vital principle, or to any vital affinities, which is merely a less simple mode of expressing the fact, is one of those delusive attempts to substitute words for ideas, which have so much tended to retard the progress of physiological science.

APPENDIX I. TO CHAPTER IX.

Chemical Constitution of the Bile.

The chemists who have lately turned their attention to the analysis of bile are Thenard and Berzelius; the former published an elaborate dissertation on the subject in 1805, in which he announced the existence of a peculiar proximate principle, which gives the bile many of its specific properties, and composes a large proportion of its solid contents; to this he gave the name of picromel; Mem. d'Arcueil, t. i. p. 23; also Traité, t. iii. p. 547, 8. He gives the following as the composition of ox bile:

Water	700·
Picromel and resin	84·3
Yellow matter	4·5
Soda	4·
Phosphate of Soda	2·
Muriate of do.	3·2
Sulphate of do.	0·8
Phosphate of lime	1·2
Oxide of iron	trace
	800·0

Mem. d'Arc. t. i. p. 38.

The constitution of human bile is considerably different, the picromel not being found in it, but its place partly supplied by what is termed resin.

Water	1000·
Yellow insoluble matter	2· to 10
Albumen	42·
Resin	41·
Soda	5·6
Salts, the same as in ox bile	4·5;—p. 57.

Berzelius, however, calls in question the accuracy of Thenard's analysis; he does not admit of the existence of the resin as described by Thenard, but attributes the peculiar characters of bile to a substance which he simply denominates biliary matter; his analysis of bile is as follows:

Water	907·4
Biliary matter	80·0
Mucus of the gall bladder, dissolved in the bile	3·0
Alkalies and salts, common to all secreted fluids	9·6
	1000·0

Ann. Chim. t. lxxi. p. 220; Ann. Phil. v. ii. p. 377..9; Med. Chir. Tr. v. iii. p. 241.

Dr. Thomson gives us rather a different statement of the result of Berzelius's analysis, which is quoted from his Swedish work.

Water	908·4
Picromel	80·
Albumen	3·
Soda	4·1
Phosphate of lime	0·1
Common salt	3·4
Phosphate of soda, with some lime	1·0
	1000·0

Chemistry, v. iv. p. 522.

Dr. Davy's analysis in Monro's Elem. v. i. p. 579, is as follows:

Water	86·0
Resin of bile	12·5
Albumen	1·5
	100·0

For a farther account of the opinions that have been entertained respecting the chemical constitution of bile, the reader may consult Baglivi, Diss. 3. circa Bilem; Haller, El. Phys. xxiii. 3. 2..20; Fourcroy, System, v. x. p. 17 et seq.; Thomson, Chem. v. iv. p. 518 et seq.; Thenard, Chim. t. iii. p. 608 et seq.; Henry, Elem. v. ii. p. 412 et seq.; Plenk, Hydrol. p. 110 et seq.; Blumenbach, Inst. Physiol. sect. 25. For an account of the liver, and its secretion, see Sœmmering, Corp. Hum. Fab. t. vi. § 84, p. 163.

Dr. Thomson has analyzed picromel by means of the peroxide of copper, and found it to consist of the following ingredients:

Carbon	54·53
Oxygen	43·65
Hydrogen	1·82
	100·00

The substance upon which he operated was procured by precipitating bile with sulphuric acid; the acid was separated by carbonate of barytes, and the fluid evaporated. What he obtained would therefore be "the biliary matter" of Berzelius; Ann. Phil. v. xiv. p. 70. The experiments of Tiedemann and Gmelin on bile appear to have been conducted with great attention to accuracy; they are principally on the bile of the ox and the dog, but there are a few on human bile. The result of the examination is that 91·51 per cent. is water, and the solid contents consist of 12 proximate animal principles and 10 neutral or earthy salts. The proximate animal principles are some of them in small quantity, and not very well defined; of those that are more so, and exist in greater proportion, we have cholesterine, biliary resin, what is termed biliary asparagin, picromel, osmazome, and mucus. The salts are principally combinations of soda, but we have no uncombined soda, as is commonly supposed; Recherches, t. i. p. 83. The bile of the dog did not differ very essentially from that of the ox, although the number of ingredients which entered into its composition was smaller; it contained cholesterine, resin, picromel, and mucus, with various salts; p. 88, 9. As far as the confessedly incomplete analysis which was made of the human bile allowed them to form an opinion, it appeared to contain the same proximate principles; p. 90. Hence we learn that the analysis of Tiedemann and Gmelin approaches more nearly to that of Thenard than of Berzelius.

M. Raspail considers bile to be essentially a saponaceous substance with a trace of soda; New System, § 1148 et seq.; see also the art. "Bile," in the Cyc. of Anat. by Mr. Brande.

APPENDIX II. TO CHAPTER IX.

Chemical Constitution of the Urine.

Dr. Henry has announced the following list of substances, as having been "satisfactorily proved to exist in healthy urine;" Elements, v. ii. p. 435, 6.

Water.
Free phosphoric acid.
Phosphate of lime.
Ditto of magnesia.
Fluoric acid.
Uric acid.
Benzoic acid.
Lactic acid.
Urea.
Gelatin.
Albumen.
Lactate of ammonia.
Sulphate of potash.
Ditto of soda.
Fluate of lime.
Muriate of soda.
Phosphate of soda.
Ditto of ammonia.
Sulphur.
Silex.

According to Berzelius, the constitution of the urine is as follows; Ann. Phil. v. ii. p. 423.

Water	933·00
Urea	30·10
Sulphate of potash	3·71
Ditto of soda	3·16
Phosphate of soda	2·94
Muriate of soda	4·45
Phosphate of ammonia	1·65
Muriate of ammonia	1·50
Free lactic acid Lactate of ammonia Animal matter soluble in alcohol Urea not separable from the preceding	17·14
Earthy phosphates with a trace of fluate of lime	1·00
Lithic acid	1·00
Mucus of the bladder	0·32
Silex	0·03
	1000·00

In some minute points these two accounts do not coincide, but they sufficiently agree respecting the general nature of the urine, and especially in the great number of its ingredients, a circumstance which is a strong confirmation of its excrementitious nature, as a general outlet for whatever is not required in the system. It must be confessed, however, that there is a fact in comparative physiology, which is adverse to the excrementitious nature of urine; I allude to the observation of Dr. Davy, Phil. Trans. for 1818, p. 305, that the urine of serpents and lizards consists of nearly pure lithic acid. A substance was examined by Dr. Prout, which, according to the information that he received, was the only species of excrement that was discharged by the boa constrictor; but the remarks of Dr. Davy, in the paper referred to above lead us to consider it probable, that it was, at least in a great measure, derived from the kidney. Dr. Prout's analysis is as follows; 100 parts were found to consist of

Lithic acid	90·16
Potash	3·45
Ammonia	1·70
Sulphate of potash, with a trace of muriate of soda	·95
Phosphate of lime Carbonate of ditto Magnesia	·80
Animal matter, consisting of mucus and a little colouring matter	2·94
	100·00

Ann. Phil. v. v. p. 415.

From some late observations by Dr. Davy, we have reason to suppose that the urine of toads and frogs differs from that of serpents and lizards, and more resembles the urine of the mammalia, in containing urea; Phil. Trans. for 1821, p. 98. The author adduces various considerations which lead to the conclusion, that the nature of the urine depends more upon the specific action of the kidney than upon any peculiarity in the diet; Phil. Trans. for 1810, p. 99. We have, however, the observations of Dr. Wollaston on the urine of birds, which leads us to the opposite conclusion; for he found the proportion of lithic acid in the excrements of different birds to vary from a $\frac{1}{300}$ part, in a goose which fed entirely upon grass, until, in birds which took animal food alone, it composed the greatest part of the whole mass. Besides the above substances, which Dr. Prout conceives to be essential to healthy urine, he enumerates the following, as occasionally found in certain morbid conditions of the system: "Nitric acid, various acids formed from the lithic, oxalic acid, benzoic acid, and carbonic acid, xanthic oxide, cystic oxide, Prussian blue? sugar, bile, and pus;" Inquiry into Diabetes, &c. p. 5.

APPENDIX III. TO CHAPTER IX.

Dr. Philip's Hypothesis of Secretion.

The arguments by which Dr. Philip supports his hypothesis may substantially be reduced to four; they are to be met with in the 13th chapter, entitled "On the Nature of the Vital powers." 1. He commences by the observation to which I have already alluded, that in comparing the sensorial and the nervous functions, the latter (using the word in the restricted sense in which it is always employed by Dr. Philip) are found to bear a strong resemblance to the physical, or, as he styles them, the inanimate powers of nature, while no resemblance of this kind can be traced with respect to the former. The acts of secretion and of calorification are analogous to many chemical processes, the transmission of impressions through the nerves to both chemical and mechanical processes, while the excitement of the muscular fibre is the effect of various physical agents. But, as the author remarks, we can trace no resemblance or analogy between sensation (perception) or volition, and the operation of any physical agents. Hence the deduction is made, that the nervous influence must depend upon a physical agent, as it appeared a necessary consequence, that where the phenomena bore a clear analogy to the effects of physical action, they could not be referred to a power which exclusively belongs to a living system.

2. The author next remarks that the vital functions must be supposed to be necessarily confined to the organization of the organs which are their peculiar seat, for that a power which is independent of the organ in which it resides cannot be regarded as a vital power. If it can exist in any unorganized or inanimate body, it must be regarded as a mere physical property. To apply this principle to the case under consideration, it is argued, that if the nervous influence be a vital power, it must be necessarily confined to a substance similarly organized with the nerves, and be incapable of existing in a part possessed of any other kind of structure. "If the nervous power can be conveyed by other parts, it is not a vital power, but one that may reside in unorganized bodies." Dr. Philip then proceeds to relate the experiments that were performed on the division of the par vagum, in which it was found, that when the division had been complete, and the divided ends of the nerves had even retracted for a small space, still the secretion of the gastric juice was continued, and consequently, according to his hypothesis, the nervous influence must have passed through the interval, and of course have been conveyed through this space by the moisture or some other interposed body; and hence, as it can exist attached to this body, it is deduced that it cannot be a vital agent. The author observes, as a circumstance that, at first view, caused some surprise, that this transmission of nervous influence between the divided extremities of a nerve can only take place with those that are attached to the ganglions, for that he was unable to produce the same effects with the nerves from the spine. This apparent anomaly he endeavours to account for upon the principle that the action of secreting surfaces is increased by whatever produces an unusual determination of blood to them, which "solicits towards them a corresponding supply of the influence of the nervous power." But nothing of this kind takes place with respect to the cerebral or spinal nerves.

3. The third argument which Dr. Philip adduces in support of his hypo-

thesis, is derived from the fact, that "we can substitute for the nervous power a variety of inanimate agents." The muscles can be excited by various mechanical and chemical stimuli, and also by the nervous influence, but as we here perceive that the nervous influence only acts the same part, or produces the same effect which may be produced by physical agents, it is concluded that this must likewise be a physical agent.

4. Dr. Philip having thus rendered it probable, or rather, as he conceives proved, that the nervous influence is a physical agent, he proceeds to inquire, whether it consists in something which is peculiar to the animal body, or whether it be an agent which operates in the production of other natural phenomena. It was supposed that electricity, or rather that modification of it which has been termed galvanism, was the most likely to possess the necessary requisites, and for the purpose of ascertaining how far this conjecture was sanctioned by the phenomena, the experiments were performed, of which an account has been given above. They consisted in applying galvanism so as to produce by means of it a secreted fluid, viz. the gastric juice, and to evolve heat from arterial blood: and as galvanism appears to have the property of acting as a stimulus to the muscular fibre and of conveying impressions along the nerves to the sensorium, it is supposed that we are able to produce by means of it, every effect of the nervous power, and hence Dr. Philip deduces an argument in favour of their identity.

The above remarks, as I conceive, afford a clear and faithful account of Dr. Philip's hypothesis, and of the arguments by which it is supported. I fully admit the force of many of the facts on which it is founded, and the ingenuity with which the author has deduced his conclusions from them, yet I must confess that they do not bring conviction to my mind. I have little to urge in favour of my own opinion, in addition to what has been stated in the preceding pages; but I was desirous that an hypothesis of so much importance in physiology, and one brought forward by so powerful an advocate, should be fairly presented to my readers; and having done this I leave it to be decided by future investigations.

CHAPTER X.

OF DIGESTION.

The last chapter contained an account of the means by which certain substances are separated from the blood, for the purpose of contributing more or less directly to the preservation of the constituents of the body in their perfect and healthy condition; I am now to describe the mode by which the loss thus occasioned is repaired, by which fresh materials are, from time to time, received into the system, and assimilated to it. This is accomplished by the function of digestion, which, when considered in its most extensive sense, may be defined the process by which aliment is made to undergo a succession of changes, so as to adapt it for the purposes of nutrition. Perhaps, in strict propriety, we ought to regard this process as consisting of several subordinate processes, each of which might be considered as a distinct function; but as they all appear to be different steps of the same operation, and subservient to one ultimate object, that by which food is converted into blood, it will be more convenient to describe the whole of them in connexion with each other[1].

The first change which the aliment experiences is entirely of a mechanical nature, and consists in reducing it into that state of minute division, which may prepare it for the future changes which it is to undergo. In man and the mammiferous quadrupeds this is accomplished by the teeth. After the food is sufficiently masticated, it is received into the stomach, where both its physical and its chemical properties are changed, and it becomes converted into a uniform pultaceous mass termed chyme. From the stomach it passes into the duodenum, where it is detained for some time, and undergoes a farther change in its properties, and where it is converted from chyme into chyle[2]. The process of chylification may be regarded as the ultimate result of the action of the digestive organs; there are still, however, certain changes to be effected before the complete assimilation is accomplished. The next step consists in the separation of the chyle from the refuse matter with which it is combined, and the transmission of this separated matter through the lacteals into the blood vessels. It is poured into the trunk of the great veins, near their termination in the right auricle of the heart, and being mixed with the blood, is finally assimilated to it in all its properties. The subjects of this chapter will be arranged under five heads; I shall first give a general description of the form and structure of the digestive organs; in the second place I shall make some remarks upon the nature of the various substances that are used in diet; next I shall examine the successive changes which they experience, from their first reception into the stomach, until they are deposited into the blood vessels; in the fourth place, I shall give an account of the hypotheses that have been invented to explain the nature of the operations; and lastly, I shall notice some affections of the digestive organs, which are indirectly connected with their functions.

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[1] See Cullen's, Inst. § 201. The term digestion, in its primary technical import, was intended to express the operation by which the aliment is macerated in the stomach, by a process supposed to be analogous to the digestions which are carried on in the laboratory; see Castelli, Lexicon, "Digestio." Perhaps the most valuable and elaborate part of Magendie's work is that which treats upon digestion; he conceives it to be made up of 8 subordinate actions; 1. reception of the food, 2. mastication, 3. insalivation, 4. deglutition, 5. action of the stomach, 6. of the small intestines, 7. of the large intestines, 8. expulsion of the fæces; of these the 5th and 6th may be regarded as the essential operations; Physiol. t. ii. p. 33.

[2] The terms chyme and chyle are generally employed by the modern physiologists in the way that is stated above, but it does not appear that there is any thing in their etymology which would lead to this distinction, nor was it

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recognized by the older authors, or even by some of those of the last century, who appear to have used the words indifferently, or to have considered them as synonymous. The following examples may be adduced among the older physiologists, where chyle is spoken of as formed by the stomach; Willis de Ferment. C. 5. p. 16; Sylvius, Disput. Med. 1. Op. p. 1, 2. et Prax. Med. lib. 1. C. 7. Op. p. 117; Fabricius de Ventriculo, Op. p. 113, 4. and p. 139; Gulielmini, de Sang. not. § 37; Charleton, Œcon. Anim. Exerc. 2. de Chylif. § 4; Lower, de Corde, p. 204; Pitcairne, El. Med. C. 5. de Œcon. Anim. § 1; Riolan, Ench. An. lib. 2. C. 23. p. 118; Bartholin, de Lacteis Thorac. Cap. 1. p. 3; see also Castelli, "chymus" and "chylus." The distinction between chyme and chyle is not recognized by Boerhaave; see Prælect. § 78..95; he appears, indeed, not to contemplate any essential difference between the contents of the stomach and the duodenum, except what depends upon the mixture of bile and pancreatic juice with the latter. Haller's opinion upon this point I shall notice more particularly hereafter; but I may remark in this place, that he does not uniformly employ the terms in their modern acceptation; see Boerhaave, Prælect. not. 12, ad § 83, not. 9. ad § 87; Prim. Lin. § 635, 638, 717, 8 et alibi. I have not been able to ascertain who it was that first assigned to the words their present signification. Sœmmering applies the term chyme to the alimentary matter when it arrives at the small intestines; Corp. Hum. fab. t. vi. p. 306..9. § 216. Magendie employs chyme to signify the substance formed in the stomach; Physiol. t. ii. p. 72, 81, 2. Sir B. Brodie employs the terms in their ordinary acceptation; Quart. Journ. v. xiv. p. 342; and this is also the case with Adelon and Chaussier, Art. "Digestion," Dict. des Scien. Méd. t. ix. compare p. 406 and 429; this article although written in a diffuse style contains much valuable information. See also Dumas, Physiol. t. i. ch. 10. p. 350 et seq. Raspail makes the distinction between Chyme and Chyle; he farther observes that the chyme is generally acid and the chyle alkaline; "New System", § 879, 0 et alibi. The distinction between the two operations of chymification and chylification is clearly laid down by Dr. Roget: Bridgewater Treat. Ch. 7. and 8, and by Dr. Prichard, on the Vital Prin. Sect. 8.

§ 1. *Description of the Organs of Digestion.*

The digestive organs[1], as they exist in man and the higher orders of animals, may be conceived to consist of three orders of parts, each of which serves a distinct and appropriate purpose. The operation of the first is entirely mechanical, constituting the means by which the food has its texture broken down, or is sufficiently comminuted to admit of the full operation of the next process, which is more of a chemical nature. At the same time, however, that this mechanical operation is going forwards, the food is mixed up with the various mucous secretions that are found in the different parts of the mouth, fauces and gullet; these tend to soften the alimentary mass, and render it more easily divisible, while they may, perhaps, have some effect in promoting the subsequent changes which it is to experience. In man and the mammiferous quadrupeds, this mechanical process is affected by mastication, as performed by the teeth. I shall not think it necessary to enter into any description of these organs, farther than to remark, that although there is a general resemblance between their form and situation, in the different classes of animals, yet upon a more minute inspection, we find that they differ very considerably from each other in these respects; and upon examining these differences, in connexion with the habits of the various animals, we shall find in all cases, that they are adapted to that species of alimentary matter, which is the best suited to the digestive organs and other functions of the individual. In some animals the teeth are so disposed as to be evidently intended for seizing and lacerating animal food; others, on the contrary, are better fitted for cropping and triturating the parts of vegetables; in short so great a correspondence has been traced between the disposition and structure of the teeth, and the general habits of the animal, that naturalists have not unfrequently assumed these organs, as among the most characteristic features, by which to form the basis of their systematic arrangements[1].

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[1] The existence of a stomach, or of some organ equivalent to it, has been supposed, by most naturalists, to be necessary to animal organization, and even to afford a definite character by which animal may be distinguished from vegetable life; see Smith's Introd. to Botany, p. 5. Dr. Willis remarks that nothing resembling a stomach has been found in any vegetable; Cyclop. Anat. v. i. p. 132. It is accordingly stated by writers on comparative anatomy, that no organs are so generally present, in all kinds of animals, as those which serve for digestion; and it is indeed self-evident, that every being possessed of life, must have the means of receiving and assimilating to itself the matter which is subservient to its growth and nutrition; Blumenbach, Comp. Anat. § 82. Sœmmering expressly says, "animal ventriculi expers innotuit plane nullum;" Corp. Hum. fab. t. vi. p. 229. According to Dr. Grant, "the internal alimentary cavity is the most universal organ of animals;" Cyclop. of Anat. v. i. p. 107. It appears, however, that there are certain animals of the inferior orders, and some of no inconsiderable size, which are not furnished with any receptacle for containing food, and which, therefore, like vegetables, must be supposed to imbibe their nutriment from the surface of the body; see Lawrence's Blumenbach, note 1. p. 129; Roget's Bridgewater Treat. v. i. p. 72, note. In the 4th chapter of this work, v. i. p. 74 et seq., we have an interesting account of the organs of digestion in the lower orders of animals, and the mode in which they receive and prepare their nutriment. Mr. Abernethy, in his 6th lecture, p. 193, 4, gives a list of the animals, of various classes, in which Hunter had examined the digestive organs.

The food, after a due degree of comminution in the mouth, is transmitted, by the act of deglutition[2], down the œsophagus, into the stomach, a bag of an irregular oval form, which lies across the upper part of the abdomen, to which it gives the name of the epigastric region. The structure of the stomach may be considered physiologically as three-fold. A large part of its substance is composed of membranous matter, which determines its form and capacity; it is plentifully furnished with muscular fibres, constituting what is termed its muscular coat, and it is lined internally with a mucous membrane, which appears to be more immediately connected with its secretions. Anatomists have indeed differed considerably in the account which they have given of the number of what are termed the coats of the stomach, but the difference is, in a great measure, verbal. Those who have made the most numerous divisions have pointed out as many as eight distinct textures; first, the peritoneal covering, below which are two strata of muscular fibres, one longitudinal, and the other circular; a cellular coat connects this with the more dense membranous expansion, called, according to the phraseology of the older anatomists, the nervous coat; below this is another cellular coat, within which is the villous or innermost coat. Considered physiologically, these may be all reduced to the muscular strata, and the internal mucous lining, with the membranous matter which lies between them. It would appear that the ultimate termination of the nerves and vessels of the stomach, so far as they can be actually traced, is on the external surface of the mucous or innermost coat, and it seems probable that this is the seat of the glands[1].

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[1] Linnæus, Sys. Nat. t. i. p. 16 et alibi; Shaw's Zoology, v. i. Introd. p. vii et alibi. The late researches of Ehrenberg have enabled him to extend this principle of classification to the most minute animalcules; Ann. Sc. Nat. t. ii. 2 ser. p. 266 et seq. For an account of the human teeth it will be sufficient to refer to the treatises of Hunter, Blake, Bell, and Fox; to Sœmmering, Corp. Hum. Fab. t. i. p. 177..207; Monro (Tert.) v. ii. p. 8..28; Blandin, notes to Bichat, t. iii. p. 107 et seq.; Cuvier, Dict. Sc. Méd. t. viii. p. 320; and to Serres, Sur l'Anatomie et la Physiologie des Dents, and Mém. Soc. d'Emul. t. viii. p. 113 et seq. For the comparative anatomy of the teeth to Cuvier. Leç. d'Anat. comp. No. 17. t. iii. p. 103 et seq.; St. Hilaire, Syst. Dent.; Rousseau, Anat. Comp. du Syst. Dent.; and to Roget's Bridg. Treat. v. ii. passim. For the soft parts connected with the process of mastication, to Boerhaave, Prælect. t. i. § 58..64; Monro's Elem. v. i. p. 495..508; Bichat. Anat. Des. t. ii. p. 663 et seq.; and to Magendie, Physiol. t. ii. p. 46 et seq.

[2] There is, perhaps, no part of the human frame which exhibits a more beautiful specimen of mechanism than the organs that are concerned in deglutition. Simple as the process may appear, it is in reality very complicated, and consists of a succession of individual actions, each of which produces an independent specific effect, yet so connected with the rest as to attain the object in view in the most perfect manner. After the aliment has been sufficiently comminuted by the teeth, it is moulded into a suitable form by the muscles of the mouth and the tongue, and is transmitted by them to the pharynx, which is, at the same time, so disposed as to be put into the best position for receiving the mass; while the same action of the parts also causes the epiglottis to close the passage into the larynx. The food being now received into the top of the œsophagus, its muscular fibres commence their contraction, which proceeding progressively from the higher to the lower part, gradually propels the mass into the stomach. For a more minute account of the process of deglutition and of the parts concerned in it, see Boerhaave, Prælect. t. i. § 70..2; he concludes his description with the following remark: "tam operosa fit arte deglutitio, tot conspirantes organorum adeo multiplicium et concurrentium actiones huc requirentur." Haller, Prim. Lin. ch. xviii. § 607..621; El. Phys. xviii. 3. 21 et seq. and xviii. 4. 1 et seq. According to Morgagni, we are indebted to Valsalva for much of our knowledge respecting the muscles which are concerned in deglutition; see Valsalva, Op. t. i. epist. 9. tab. 5 and 6. For a description of the œsophagus and its appendages, see Bell's Anat. v. iv. p. 40..4; Sœmmering, Corp. Hum. Fab. t. vi. p. 201..8. § 109..126; Monro (Tert.) on the gullet, p. 1..5; and Elements, v. iii. p. 508..512; Bichat, Anat. Descrip. t. iii. p. 379..397. Dumas, Physiol. t. i. p. 341..353, divides the act of deglutition into four stages: during the first, the alimentary mass is propelled towards the gullet; during the second, the cavity dilates and receives it; during the third, the mass passes into the pharynx; and during the fourth, it is transmitted down the œsophagus into the stomach. Magendie, Physiol. t. ii. p. 54..67, marks three stages; by the first, the mass passes from the mouth to the pharynx; by the second, it enters the œsophagus; and by the third, it is transmitted

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to the stomach. See also Roget's Bridgewater Treatise, v. ii. pt. 2. chap. 6. "Preparation of Food," in which the different steps of the process are described previous to chymification. A view of the muscles concerned in deglutition, is contained in Albinus, Tab. 10, 11, 12; also in Santorini, pl. 6; which, although entitled to but little commendation as a work of art, is probably a correct delineation of the organs which it represents, and this commendation may be justly bestowed upon Watt's "Anatomical Chirurgical Views," the execution of which is stiff and harsh.

[1] For a description of the stomach, its form, situation, structure, &c., the student may be referred to the following works; Fabricius de ventric. Op. p. 99..149; Willis, Pharm. Rat. sect. 2; Winslow, sect. 8. § 2. 43..71; Boerhaave, Prælect. t. i. § 73 cum notis; Haller, Prim. Lin. ch. xix. § 622..6, and El. Phys. xix. I. 6..11; Blumenbach, Inst. Physiol. § 352..4; Fordyce on Digestion, p. 6..12; Sœmmering, Corp. Hum. Fab. t. vi. p. 209..228. § 127...147; Bertin, Mém. Acad. pour 1760, p. 58 et seq.; Sabatier, Anat. t. ii. p. 288..2; and Boyer, Anat. t. iv. p. 332, who enumerate four coats, the membranous, the muscular, the nervous, and the villous; Bell, Anat. v. iv. p. 44 et seq.; Bichat. Anat. Descrip. t. iii. p. 397..415; Buisson, the editor of this volume, reduces the coats of the stomach to three, the serous, the muscular, and the mucous; Monro (Tert.) Elem. v. i. p. 515; and Outlines, v. ii. p. 115, 6; Fleming's Zool. v. i. p. 314 et seq.; in Bell's Dissect. pl. 4, we have an excellent view of the stomach, and its contiguous parts; and in Cloquet, Anatomie, pl. 260..5; we have also a characteristic drawing of the stomach by Mr. Bauer, Phil. Trans. for 1821, pl. 4. With respect to Willis's description of the coats of the stomach, it is not a little remarkable to observe the influence of words upon opinions; although a person of much knowledge and judgment, and one who had particularly attended to the nervous system, he subscribes to the opinion of the old

Besides the mucous fluid which is poured out by the internal membrane, in the same manner with all other bodies of a similar texture, the stomach has been supposed to possess glands that secrete the peculiar fluid called gastric juice, which acts so important a part in the process of digestion; the existence, however, of any distinct glands for this purpose is rather inferred from the effects which it is supposed that they produce through the intervention of their secretion, than from our being able to demonstrate their existence[1]. The membranous substance of the stomach appears to be peculiarly distensible, so as to admit of having its capacity much increased, while its muscular fibres give it a high degree of contractility, by which means its bulk is capable of being diminished as occasion requires, and is thus always exactly adapted to the quantity of its contents.

The muscular fibres being connected to the membrane either individually, or in small separate groups, not only enable the stomach to contract in its whole extent, and in all directions, but bestow upon its separate parts the power of successively contracting and relaxing, so as to produce what is termed its peristaltic, or, perhaps, more appropriately, its vermicular motion[2]. The action of these fibres appears to be so directed as to produce two mechanical effects; in the first place, the successive contraction of each part of the stomach, by producing a series of folds and wrinkles, serves to agitate the alimentary mass, and by bringing every part of it in its turn to the surface, to expose it to the influence of the gastric juice, while, at the same time, the whole of the contents are gradually propelled forwards from the orifice, which is connected with the œsophagus, to that by which they are discharged. These fibres, like all those which compose the muscular coats, are not under the control of the will.

There are few parts of the body which are more copiously

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anatomists, that the sensibility of the stomach, and the other parts of the digestive organs, is seated in what was called the nervous coat; see Pharm. Rat. p. 6, 13. Among the older physiologists or anatomists who have given us characteristic views of the stomach, and the parts connected with it, we may select Vesalius, De Corp. Hum. Fab. lib. v. fig. 10..19; Eustachius, Tab. Anat. No. 10. fig. 1, 2, 3; Ruysch, Thes. Anat. 2. tab. 5; Santorini, tab. 11.

[1] See Haller, El. Phys. xix. 1. 14; Bell's Anat. v. iv. p. 58. Winslow says that the glands are situated on the internal surface of what is termed the nervous coat, and that there are perforations in the innermost coat, which afford a passage to the excretory ducts; Sect. 8. § 2; but it may be questioned whether this was the result of actual observation.

[2] Haller, El. Phys. xix. 4. 9. 10; Boyer, Anat. t. iv. p. 333..5; supposes the muscular fibres of the stomach to be disposed in three layers, the first being a continuation of those of the œsophagus, the next the transverse or circular fibres, and lastly, two large muscular bands, which are situated obliquely to each other. One of the first accurate descriptions that we have of the muscular coat of the stomach is by Bertin, Mém. Acad. pour 1760, p. 58 et seq.

provided with blood vessels than the stomach, a circumstance which is evidently connected with the great degree of vitality possessed by this organ[1]. Its nerves are likewise very numerous, and are remarkable for the variety of sources whence they are derived. It not only partakes of the ganglionic nerves, which are thickly dispersed over it, in common with the neighbouring viscera, but it likewise derives a supply of nerves from the spinal cord, and is distinguished from all the other parts of the body, except what are termed the organs of sense, by having a pair of cerebral nerves, almost entirely devoted to it, although it is situated at so great a distance from the brain[2]. The specific uses of all these nerves will be more fully considered hereafter; but I may remark, in this place, that the stomach appears to possess, in a very high degree, many of the powers which are ascribed to the nervous influence, it is exquisitely sensitive, while it partakes remarkably of the general actions of the system, and sympathizes with all its changes, so that it may be regarded as a kind of common centre, by which the organic functions are connected together, and their motions regulated.

The extremity of the stomach, by which the food is received, is termed the cardia, that by which it is discharged the pylorus; the latter is furnished with a fold of the membranous coat, and also with a number of muscular fibres, possessing in some degree the property of a sphincter, so as to retain the food until it is in a proper state for being discharged[3], and thus resisting the vermicular action of the muscular coat, when it would tend to propel the aliment through the pylorus, before it had undergone the requisite preparation. The food is also prevented from being too quickly discharged by the relative situation of the cardia and the pylorus; as the stomach lies across the abdomen, these two orifices are nearly on the same horizontal line, and in consequence of the form of the organ, and the connexion of the neighbouring parts, whenever it is distended with food, a large part of its contents will be below the level of the pylorus[1], so that it must require a considerable force of muscular contraction in the stomach to discharge its contents, in which it is probably aided by the diaphragm and the abdominal muscles[2].

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[1] Haller, El. Phys. xix. 4. 19; Blumenbach, Inst. Physiol. § 356; Bell's Dissect. p. 19..25, pl. 3, 4.

[2] Winslow's Anat. v. ii. sect. 8. § 2. par. 78, 9; Haller, El. Phys. xix. 1. 21; Blumenbach, Inst. Phys. § 355. p. 201; Bell's Anat. v. iv. p. 64; he observes that the par vagum is distributed principally over the cardia, and that this appears to be the most sensitive part of the stomach. Of Walter's plates, Nos. 3 and 4, it is impossible to speak too highly; the accuracy of the drawing, and the clearness of the engraving, are equally deserving of admiration.

[3] The peculiar office and functions of the pylorus is one of those subjects that was considered by the older anatomists as something singularly wonderful or mysterious. The delicate sensibility of the stomach was conceived to reside chiefly in this part, and it was also thought to produce some specific effect in the process of digestion, which could only be explained by supposing it to be endowed with certain extraordinary powers and qualities. Vanhelmont conceived it to be the peculiar seat of the soul, an opinion to which Willis gives a degree of support. Richerand ascribes to it something like intelligence, when he says that it has a peculiar tact, which enables it to select from the contents of the stomach what is proper to pass through, while it rejects the remainder; Physiol. § 23. p. 111, 2.

The intestinal canal[3], which receives the aliment when it leaves the pylorus, is a long winding cylindrical tube, varying much in its different parts, as to its form and diameter, but which, like the stomach, may be considered as essentially consisting of three structures, the membranous, the muscular, and the mucous, each of which, like the corresponding parts of the stomach, serves respectively to give the organ its general form, to impart to it a degree of contractility, and to furnish the appropriate secretions. The intestines have been divided by anatomists into the two great classes of small and large, a division which refers entirely to the diameter of the parts, but which may also be connected with certain specific differences in their form, structure, and situation. Each of the two portions is then subdivided into three parts, which, commencing with the stomach, have received the names of duodenum, jejunum, and ilium, for the small, and cœcum, colon, and rectum, for the large intestines.

The division into the small and large intestines, may be considered as founded upon their physiological nature as well as upon their anatomical structure, for it appears to be in the former alone that any part of the digestive process is carried on, the latter being solely intended to remove from the system the refuse matter, which is incapable of undergoing the process of chylification. With respect to the small intestines, there are no definite characters, either anatomical or physiological, by which the jejunum and ilium can be distinguished from each other;

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[1] It has, however, been remarked by anatomists, that although it is the great curvature which principally becomes distended when food is received into the stomach, and that we are in the habit of considering this as the lower part of the organ, yet by distention this part is protruded forwards as well as downwards, so that, perhaps, there may be no greater portion of the contents below the level of the pylorus than in the more contracted state of the organ. But it is probable that the mere power of gravity is but little concerned in the transmission of the food through the stomach. See Haller, El. Phys. xix. 4, 5.

[2] Haller, El. Phys. xix. 4. 2. 3.

[3] For a description of the form and structure of the intestinal canal, it may be sufficient to refer to Haller, El. Phys. lib. xxiv; Monro (Prim.) Ed. Med. Essays, v. iv. p. 76..92, a paper which contains much important information respecting the minute anatomy of the parts; Blumenbach, Inst. Phys. sect. 28; Bell's Anat. v. iv. p. 70..83; Monro (Tert.) Elem. v. i. p. 533..550, and Cloquet, Anatomie, p. 675..685, pl. 261, 2, 7, 8, 9. We have an interesting account of the comparative anatomy of these organs in Blumenbach, Comp. Anat. note A, p. 177, and in Sœmmering, Corp. Hum. Fab. t. vi. p. 281 et seq.

but the case is different with respect to the duodenum, the structure and functions of which are sufficiently appropriate[1]. It appears, indeed, to be the part which is subservient to the important process of chylification, while the office of the jejunum and ilium is principally confined to abstracting the chyle from the residual mass; this is accomplished by its being gradually transmitted along their cavity, thus permitting the lacteals to absorb the nutritive part, as it is brought into contact with their orifices. The means by which the absorption is effected will be considered in the following chapter.

In the course of this work, I have confined my attention for the most part, to the functions as they exist in man, and in the animals which the most nearly resemble him, with only occasional observations on comparative physiology. There are, however, some remarkable deviations from the ordinary form and action of the digestive organs, even among the higher classes of animals, of which I shall give a more particular description, not only in consequence of the interest which may be attached to them considered individually, but more especially, from the information which they afford us concerning the function of digestion generally, by noticing the peculiarities of their structure, and observing the relation which their several parts bear to the operations of the human organs. I refer to the compound stomachs of the ruminating animals, and to the strong muscular stomachs of certain birds[2].

Many of the mammalia possess a stomach of a much more complicated structure, and possessed of a much greater variety of distinct parts, than that of man. These animals feed principally on the leaves or stalks of plants, which they take in large quantity; the food is swallowed, in the first instance, without much mastication, and is received into a capacious cavity, called venter magnus, or paunch, where it remains for some time, as if for the purpose of being softened or macerated. Connected with this is a much smaller cavity, which, in consequence of its internal coat being drawn up into folds, that lie in both directions, so as to leave between them a series of angular cells, has obtained the name of reticulum, or honey-comb. From this second stomach the food is again brought up into the mouth, in the form of a rounded ball, and is then masticated by the animal, until it is sufficiently comminuted, constituting the process of rumination, or chewing the cud. The mass, when duly prepared, is again swallowed; but it now passes by the first and second stomach, and is conveyed into the third cavity, called omasum, or maniplies, distinguished by the broad folds or ridges of the inner membrane, which are disposed longitudinally, and differ from those of the reticulum, in not being crossed by others in the contrary direction; it is also of smaller size than any of the other cavities. From this the food is sent into the fourth stomach, named abomasum or read[1], which is of a large size, although much less than the paunch, is of an irregular conical form, the base being turned towards the omasum, lined with a mucous or villous coat, which is disposed into rugæ like those of the third, and appearing, in its structure and functions, to be most analogous to the simple stomach of man and the other mammalia[2].

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[1] Haller proposes to denominate the whole of that portion of the small intestines, which lies behind the mesocolon, the duodenum, and to apply the term small intestines, intestinum tenue, to the remaining part, as the jejunum and ilium entirely agree in their structure and functions, and are obviously different from the duodenum in these respects. It is only partially covered by the peritonæum, it is firmly attached to the spine, not merely connected to it by a loose membrane, as is the case with the other parts of the small intestines, its diameter is larger, it is more vascular and glandular, its muscular fibres are stronger, and it has larger folds; the pancreatic and biliary ducts open into it. See Winslow's Anat. v. ii. sect. 8. § 3. par. 108 et seq.; Haller, Prim. Lin. ch. 24. § 719. not. * * ad § 96. in Boerhaave, Prælect. and El. Phys. xxiv. 1. 4. 5; also Bell's Anat. v. iv. p. 65.; Fordyce on Digestion, p. 15..9; Monro (Tert.) Elem. v. i. p. 534, where we have the description of the duodenum by the elder Monro, copied, although with some variations, from the Ed. Med. Essays, v. iv. p. 66..8; this essay is accompanied by a plate; see also Sandifort, Tab. Duod.; Sœmmering, Corp. Hum. Fab. t. vi. p. 283..5. § 182..5; Richerand, Physiol. § 25. p. 115, 6; Bichat, Anat. Des. t. iii. p. 416..421; Santorini, C. 9. § 7. p. 166, 7. The duodenum has been named by some anatomists the ventriculus succenturiatus, or accessory stomach, as being the organ in which the process of chylification appears to be perfected; Claussen, de Duodeno, in Sand. Thes. t. iii. p. 273. § 19, 0; see Sabatier, Anat. t. ii. p. 302, 3; Boyer, Anat. t. iv. p. 345; Chaussier, in Dict. des Scien. Méd. t. ix. p. 429..4; and Dumas, Physiol. t. i. ch. 10.

[2] For an interesting account of the organs of digestion in various classes of the mammalia, see Carus's Comp. Anat. by Gore, v. ii. p. 72 et seq.

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[1] The corresponding terms in the French language are Panse, Bonnet, Feuillet, and Caillette.

[2] We have a very complete account of the digestive organs of ruminant animals by Peyer in his Mericologia; they are described, as it appears, with great minuteness, accompanied with coarse, but expressive engravings. We have excellent views of the parts by Daubenton, in Buffon's great work, Nat. Hist. des. Anim. t. iv. pl. 15..8, and by Sir Ev. Home in Phil. Trans. for 1806, p. 362..5, pl. 15 and 16; and Lect. Comp. Anat. v. ii. pl. 21..5. See also Haller, El. Phys. xix. 1, 2; and xix. 4. 15; and Cuvier, Leç. Anat. Comp. t. iii. p. 363..6. Among the older physiologists we have a good description of the parts by Fabricius, in his treatise "De Varietate Ventriculorum:" Op. p. 128 et seq. The reader may consult with advantage Grew's work on the Compar. Anat. of the Stomach, a treatise which, in a short compass, contains many valuable, and probably original observations, respecting the comparative anatomy of the digestive organs; also Glisson, de Ventriculo, Ch. i. § 9..15, p. 123..7. There are certain animals which appear to possess a kind of intermediate stomach, between the simply membranous receptacle, and the complicated structure of the ruminants. This is particularly the case with the horse, in which the two halves of the stomach possess an obviously different structure, the left side seeming to be intended merely as a reservoir for the food, while the right half is provided with the villous coat and the glandular apparatus to adapt it for the purpose of chymification; Bertin, Mém. Acad. Scien. pour 1746, p. 23 et seq., fig. 2; Blumenbach, Comp. Anat. § 87, p. 133. and note C. p. 153. From the remarks of Prof. Monro it appears somewhat doubtful how far this structure exists in the human stomach, as has been supposed by some physiologists; *Outlines*, v. ii. p. 111..5. Hunter informs us that the whale possesses four

There is some doubt as to the effect which is produced by the different parts of this complicated apparatus, and as to the use which they serve in the œconomy of the animal. It is, however, pretty clear that the object of the first stomach is principally that of maceration, which is still further completed in the reticulum, that this cavity as well as the omasum contain secretions which are mixed with the aliment, which it may be presumed are more or less similar to the saliva, while it is in the abomasum that the proper digestive operation, that of chymification, is conducted[1]. There has been much discussion concerning the final cause of this arrangement, or concerning the cause why the maceration and mastication of the food is effected in a different manner in these animals from what it is in those that, in other respects, the most nearly resemble them. The popular opinion is, that, from the nature of their food, the large quantity of it which these animals require for their support, and the consequent length of time which is necessary for its complete mastication, it was requisite that it should be more completely macerated, and be mixed with a greater proportion of the different mucous secretions, than is the case in the ordinary process[2]. It has, however, been doubted how far this hypothesis can be maintained, as there are some of the ruminant animals, where the organs of mastication, as well as the general habits of the animal, would appear to be adequate to the preparation of the food by means of a simple stomach[3]. When animals that possess ruminant stomachs take in liquids, they are conveyed in the first instance into the second stomach, where they serve to macerate the food as it passes from the paunch, so as to prepare it for the process of rumination[4]. While the young animal is nourished altogether by the mother's milk, it passes directly through the third into the fourth stomach, and it is not until they begin to eat solid food that rumination is established. It has been supposed that the act of rumination is under the control of the will, and that the animals possess a voluntary power of conveying the food at pleasure either into the first or the third stomach, and of returning it from the second stomach into the mouth[5].

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stomachs, which in their structure and appearance bear a considerable resemblance to the digestive organs of the ruminants; but it appears that they do not correspond in their uses, as in this class of animals the second cavity seems to be that in which chyme is produced; Phil. Trans. for 1787, p. 410, 1.

[1] Hunter on the Animal Œconomy, p. 212, 3.

[2] It was supposed by some of the ancient anatomists, as it appears by Galen and Aristotle, that the use of this particular organization of the stomach, was to compensate for the deficiency of the incisor teeth, the materials of which are applied to the formation of the horns. See remarks upon this opinion by Fabricius, de Variet. Ventr. Op. p. 131, 2.

[3] Blumenbach, Comp. Anat. by Lawrence, p. 134..8.

[4] Home in Phil. Trans. for 1806, p. 363.

[5] Grew, Comp. Anat. of the Stomach, &c. Ch. v. p. 26; Ray's Wisdom of God, &c. p. 275; Blumenbach, Comp. Anat. § 90, 1. p. 137, 8. The mechanism of these parts, as connected with each other, and their relative

The other animals to which I alluded, as possessing a peculiar kind of stomach, are certain tribes of birds. Birds, although not provided with teeth, or any other organ of mastication, many of them feed upon hard grains or other substances, which the gastric juice does not seem to be capable of dissolving while in their entire state. To supply this deficiency, the birds who employ a diet of this description are provided with two peculiar organs, the crop or craw, ingluvies, and the gizzard, ventriculus bulbosus. The crop is a large membranous cavity, attached to the lower end of the œsophagus, in which the food is received when it is first swallowed, and where it appears to be softened by the secreted fluids of the part. After a due degree of this kind of maceration, it is transmitted to the apparatus called the gizzard. This is a cavity of a moderate size and flattened spherical form, composed of four strong muscles. Two of these, which constitute the greatest part of its bulk, are of an hemispherical shape, of a pecularly dense and firm texture, and lined internally with a thick callous membrane, of the nature of cartilage. Attached to these, forming, as it were, the ends of the cavity, are two other muscles, of much smaller dimensions,

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actions, are so curious, and exhibit so remarkable an example of mechanical contrivance, that I shall quote the account which is given of it by Blumenbach. "The three first stomachs are connected with each other, and with a groove-like continuation of the œsophagus, in a very remarkable way. The latter tube enters just where the paunch, the second and third stomachs, approach each other; it is then continued with the groove, which ends in the third stomach. This groove is therefore open to the first stomachs, which lie to its right and left. But the thick prominent lips, which form the margin of the groove, admit of being drawn together so as to form a complete canal: which then constitutes a direct continuation of the œsophagus into the third stomach. The functions of this very singular part will vary, according as we consider it in the state of a groove, or of a closed canal. In the first case, the grass, &c. is passed, after a very slight degree of mastication, into the paunch, as a reservoir. Thence it goes in small portions into the second stomach, from which, after a further maceration, it is propelled, by a kind of antiperistaltic motion, into the œsophagus, and thus returns again into the mouth. It is here ruminated and again swallowed, when the groove is shut, and the morsel of food, after this second mastication, is thereby conducted into the third stomach." Flourens has lately published two interesting memoirs on the digestive organs of ruminant animals and the functions of their respective parts. In order to elucidate this latter point he made openings into the different stomachs, and in this way he found, that when the food is taken in small quantity and is much comminuted, a considerable part of it passes directly into the third stomach. He found, that when the action of the abdominal muscles was destroyed, by the division of the spinal cord, that rumination could not be performed. It would appear, that the relative position of the œsophagus and the semi-circular canal, which connects the two first stomachs with the third, is the immediate agent in the deposition of the food in the different stomachs and its return into the mouth. Flourens' observations lead to the conclusion, that the fourth stomach is the proper organ of chymification, the three first being merely preparatory to this operation; Ann. Sc. Nat. t. xxvii. p. 34 et seq. In Phil. Trans. for 1830, p. 85, is a brief account of the stomach of the zariffa, by Sir E. Home; it is accompanied by some interesting plates, representing magnified portions of the interior surface of the different stomachs, illustrating their peculiar structure.

but of the same structure and consistence[1]. There is an orifice which suffers the food to pass in small successive portions from the crop into the cavity of the gizzard, and the effect of the contraction of the two large muscles of this part is to move them laterally and obliquely upon each other, so that whatever is placed between them is subjected to a very powerful combined action of friction and pressure. The force of trituration which these muscles exercise is almost inconceivably great, so as not only to break down the hardest grains, and reduce them to a complete pulp, but even to grind to powder pieces of glass, and to act upon siliceous pebbles, and masses of metal, while, at the same time, the cuticular lining is so dense and impenetrable, as not to be injured by the introduction of lancets or other bodies with sharp cutting edges, which have been introduced into the cavity by accident or for the sake of experiment[2].

The action both of the crop and the gizzard must be considered as essentially mechanical, the latter being equivalent to the teeth, and the former appearing to serve merely for the purpose of maceration. We always observe a strict connexion between the food of birds and the nature of their stomachs, those alone possessing the gizzard who employ substances which the gastric juice would not be able to dissolve in the entire state. The stomachs of carnivorous birds are termed membranous, in opposition to the strongly muscular organs which have been described above; these, however, are plentifully furnished with muscular fibres, and possess the same kind of peristaltic and vermicular motion with the human stomach and those of the non-ruminant mammalia.

Most anatomists, in describing the muscular stomachs of granivorous birds, speak of the gizzard as analogous to the digesting stomach of man, or of the non-ruminant quadrupeds, that is, the organ by which chyme is produced, whereas, strictly speaking, it is merely a substitute for the organs of mastication. Grew, whose remarks on these parts are very judicious, although he seems to have considered the action of the gizzard as entirely mechanical, does not point out any provision for the production of chyme, probably because he considered trituration as alone competent to the process. He aptly describes the gizzard as a part, "wherein the meat, as in a mill, is ground to pieces, and then pressed by degrees into the guts in the form of a pulp. For which purpose the deductor serves to deliver the meat from the echinus to the laboratory, as a hopper to a mill. The four grinders, or chief operators, are the mill-stones." He then goes on to explain very correctly the mode in which the muscles act[1]. The same remark applies to Peyer, who gives a full and correct account of the structure of the parts, and seems to consider the sole office of the crop and gizzard to be for maceration and trituration[2]. Spallanzani, after proving, in the most decisive manner, that the action of the muscular stomachs is essentially mechanical, and that grains and other hard bodies are not digested when they are protected from the effects of trituration, proceeded to inquire how the triturated matter is converted into chyme, and seems to have established, that this, or any other soft substance, is acted upon by the gastric juice, as in membranous secreting stomachs. This fluid cannot be furnished by the gizzard itself, as its structure is evidently not adapted for secretion, but by a glandular apparatus named echinus, ventriculus succenturiatus, bulbus glandulosus, or infundibulum, which is situated at the lower end of the œsophagus[3].

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[1] Blumenbach's Comp. Anat. § 99. Grew describes the gizzard as consisting of six muscles; four large ones which compose its principal substance, and two that are much smaller; Comp. Anat. of the Stomach, p. 34. These two latter are, however, only appendages to the gizzard, and serve to conduct the food into its cavity from the bulbus glandulosus. We have a good view of the whole apparatus, as it exists in the turkey, by Mr. Clift, accompanying a paper of Sir E. Home's in Phil. Trans, for 1807, pl. 5. fig. 1. See also Lect. on Comp. Anat. v. 2. pl. 49, 62. For some valuable observations on the action of the gizzard we are indebted to J. Hunter; Anim. Œcon. p. 198, 9.

[2] For facts on this subject, see Acad. del Cimento, p. 268, 9; Borelli, de Motu Anim. t. ii. prop. 189; Redi, Esperienze intorno a diverse cose, p. 89 et seq.; and Spallanzani, Dissert. i. § 5..8 and 10..22. Prof. Kidd has noticed a remarkable analogy between the digestive organs of the mole cricket, gryllus gryllotalpa, and the stomachs of granivorous birds; Phil. Trans. for 1825, p. 222..5, fig. 6, 7, 8. See Roget's Bridgewater Treatise, v. ii. p. 210 et seq., where we have an account of a gizzard or something analogous to it, as existing in various orders of insects; also the articles "Annelida," by Dr. M. Edwards, and "Arachnida," by M. Adouin, in the Cyclop. of Anat. v. i. in loco, on the digestive organs of these animals respectively.

We may remark that birds, which have no organs of mastication, have no proper salivary glands, the secretions that are provided by the appendages to the stomach supplying the necessary fluids[4]; the echinus is probably, in this case, the supplementary part[5]. In this instance as in others of an analogous kind, besides the birds which have the stomach of a decidedly muscular, or of a decidedly membranous structure, there are many which have what may be termed intermediate stomachs; of these a copious list is given by Haller[6].

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[1] Compar. Anat. of the Stomach, Ch. ix. p. 40, 1.

[2] Anat. Ventr. Gall. in Manget, Bibl. Anat. t. i. p. 172. See also Haller, El. Phys. xix. 1. 7, and Fordyce on Digest. p. 172.

[3] Diss. N°. i. § 3..8, 39..45, 52. In Home's Comp. Anat. v. ii. pl. 56, are a series of figures, exhibiting the various forms of these glands.

[4] Spallanzani, Exper. § 47..52, 79. Blumenbach's Comp. Anat. note 1, p. 159.

[5] Blumenbach, p. 142; see also Grew Comp. Anat. &c. Ch. 8; the Art. "Birds" in Rees; and "Aves," by Mr. Owen, in Cyc. Anat. v. i. p. 319, 0.

[6] El. Phys. xix. 1. 2. It is well known that granivorous birds are in the habit of swallowing small pebbles, a fact which seems to have been first noticed by the members of the Acad. del Cimento; Saggi de Esper. p. 268. Notwithstanding the experiments of Spallanzani, § 27, 8, it appears that the

It would appear probable that all the anatomical varieties in the structure of the stomachs of different animals may be resolved into their mechanical effects upon the aliment, as it seems that whatever be the nature of the food which is employed, if it be sufficiently comminuted or triturated, it is equally acted upon by the gastric juice. We find, indeed, that certain animals naturally confine themselves to certain kinds of food, and we must therefore conclude that such food is better adapted to the nature and constitution of the individual. But numerous examples are familiar to every one, where, either for the purpose of experiment, or from necessity, a total change has taken place, as for example, from an animal to a vegetable diet, or the reverse, without any apparent injury to the functions being produced, provided the mechanical texture of the food admits of its solution or minute division in the stomach.

SECT. 2. *An Account of the Articles employed for Food.*

The articles employed in diet may be classed under the two great divisions of animal and vegetable, each of them competent to the support of life, probably in all kinds of animals, although it would appear that, in most cases, one or the other is better adapted to the different species of them. From what has been stated above, it may be conceived, that this greater competency depends principally upon the mechanical properties of the substances, but they likewise differ considerably in their chemical nature, and this both with respect to their proximate principles and their ultimate elements. The ultimate elements of animal substances are oxygen, hydrogen, carbon, and nitrogen; vegetable substances contain oxygen, hydrogen, and carbon; but the proportion of carbon is generally greater, and of hydrogen less, while, for the most part, they are either without nitrogen, or contain it in small quantity only.

Although there is reason to believe that every article of food

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food is not equally well digested without them, and we may easily conceive that they may contribute to the mechanical effect of the gizzard. Borelli, de Mot. Anim. par. ii. prop. 102, 4, formed the extravagant idea, that these stones directly contributed to nutrition, an opinion which was opposed by Redi, who was aware of their real use; see Esperienze, p. 84, where he expressly says, "Quelle pietruzze sono come tante macinette raggirare da quei due forti et robusti musculi de' quali e composto ventriculo.. ;" also Osserv. p. 91, 2. Blumenbach, Comp. Anat. note 19. p. 145, 6, supposes that their especial purpose is to kill the grains, which, while alive, would resist the action of the gastric juice; but it is scarcely necessary to have recourse to this supposition. It has, however, been thought to receive some confirmation from the circumstance of the Pangolin, Manis pentadactyla, swallowing pebbles; for as its food consists of insects, which are not masticated, the pebbles have been supposed to be necessary for the purpose of crushing them, and thus depriving them of life, so as to render them more easily acted upon by the digestive fluids; p. 139. On the subject of these pebbles see also Hunter on the Anim. Œcon. p. 196, 8; Fordyce on Digestion, 23, 4; Blumenbach, Spec. Physiol. Comp. p. 17.

which is received into the stomach, must experience a complete decomposition, and be assimilated into the state of chyme, before it can serve for nutrition, yet the successive steps of the change or the length of the process which it has to undergo, depends, in some measure at least, upon the similarity which there is between the alimentary matter and the materials of which the body is composed. We therefore find that carnivorous animals, in general, have less bulky and less complicated organs than the herbivorous, and that among the latter, those that feed upon seeds or fruits, with the exception of the ruminants, have them less so, than those which live upon leaves or the entire vegetables. The stomach and intestines of man assimilate him, in regard to the nature of his diet, more to the herbivorous than to the carnivorous animals[1], yet we find, as a matter of fact, that either kind of diet is perfectly competent to his nutrition and support, and that probably the best state of health and vigour is procured by a due admixture of the two classes of substances.

We find, indeed, that mankind are principally guided in the choice of their food, with respect to its animal or vegetable origin, by the facility with which they are able to procure either the one kind or the other[2]. The inhabitants of the northern regions, where, at least during a considerable part of the year, vegetables could not be obtained, live almost entirely upon animal food, while in the warmer climates, where fruits and vegetables of all kinds are abundant, the diet is chiefly composed of these substances. We may remark, however, that this arrangement, although more a matter of necessity than of choice, is, on other accounts, the best adapted to their respective situations. An animal diet is probably better fitted for producing the vigour and hardihood of frame, which is requisite to brave the rigour of an arctic climate, while at the same time we may presume that it is more suited to the evolution of heat.

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[1] Cuvier, Regne Anim. t. i. p. 86. Lawrence's Lect. p. 217 et seq. The reader who is disposed to pursue this inquiry, may peruse the learned dissertation of Richter "De Victus Animalis Antiquitate et Salubritate," where he will find the subject treated in the true spirit of German research.

[2] See Haller, El. Phys. xix. 3. 3. The third section generally contains much useful and curious information respecting the different kinds of substances that have been employed in diet, either by nations or individuals; it is, however, liable to the imputation, from which many parts of this great work are not exempt, of the references being rather numerous than select. See also Lorry, Essai sur les Alimens; Plenk, Bromatologia; Richerand, El. Phys. § 3. p. 83; Sœmmering, Corp. Hum. fab. t. ii. p. 241, 250. § 157..161; Parr's Dict. Art. "Aliment;" Pearson's Syn. part 1; Lawrence's Lect. p. 201, 9; Thackrah's 2d Lect. on Diet, p. 54 et seq., and Paris on Diet, part 5. Dr. Stark collected a series of facts respecting individuals, who had lived for a considerable length of time on some peculiar kind of diet; Works, p. 94, 5. His experiments on the effect produced by different kinds of aliment upon his own system, which he pursued with unexampled perseverance, afford a number of very curious results, but it would be impossible to give any synoptical view of them, consistent with the elementary nature of this work; see Journal, p. 96..168. See the art. "Aliment," by Dr. Kellie, in Brewster's Enc., for a good account of the various articles employed in diet; also Elliotson's Physiol. p. 65, 6. Roget's Bridgewater Treatise, part 2. Ch. 3. § 1. The following articles may also be consulted with advantage; Londe, "Aliment," Dict. de Méd. et de Chir. t. ii. p. 1 et seq.; Rostan, "Aliment," Dict. de Méd. t. i. p. 523 et seq.; Rullier, "Nutrition," Ibid. t. xv. p. 161 et seq.; Adelon and Chaussier, "Digestion;" Dict. Sc. Méd. t. ix. p. 360 et seq.

The proximate principles, or primary compounds of animal origin that are employed in diet, are fibrin, albumen, jelly, and oil, to which we may add sugar, osmazome, and some others of less importance. The animals that are employed in diet are taken principally from the mammalia, from birds, fish, the testacea, and the crustacea. The flesh of the mammalia and of birds consists chiefly of fibrin, together with a quantity of jelly united to it, especially in young animals. Milk, which from its destination as the food of the young animal immediately after birth, may be regarded as peculiarly adapted both for digestion and nutrition, consists of an emulsion of albumen, oil, and sugar, suspended in a large quantity of water. In the formation of cheese and butter, we abstract the greatest part of the water, and obtain the albumen and oil respectively in a state of greater or less purity according to the exact nature of the process which is employed. The eggs of birds, which likewise contain a peculiarly nutritive species of food, consist chiefly of albumen with a quantity of oily matter. Fish consist of a much greater proportion of albuminous and gelatinous matter, in some cases united with a considerable quantity of oil, and the same would appear to be the case with the testacea and the crustacea that are employed in diet. It is scarcely necessary to observe that the different kinds of soups consist nearly of the same proximate principles with the materials of which they are composed, a portion of the firm and dense substances being rejected, while the more soluble parts are dissolved, or, perhaps, rather suspended in the water, consisting therefore of fibrin, albumen, jelly, or fat, according to the age of the animal, or the part of it which is employed.

The vegetable products, which compose any considerable portion of our diet, are fruits, seeds, roots, tubers, seed-vessels, stalks, and leaves. The most important of the proximate principles are gluten, farina, mucilage, oil, and sugar[1]. In all those nations which have arrived at any great degree of civilization, the main bulk of the vegetable food is derived from seeds of various kinds, and particularly from some of the cerealea; of these wheat has always been held in the highest estimation. In some countries rice composes a large proportion of the food of the inhabitants, and in many of the warmer climates maize is largely employed.

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[1] Haller attempts to reduce all nutritious substances to one principle, jelly: El. Phys. xix. 3. 2; Cullen thinks, that the matter of nutrition, is, in all cases, either "oily, saccharine, or what seems to be a combination of the two." Physiol. § 211. In his treatise on the Mat. Med. v. i. pt. 1. ch. 1. p. 218 et seq., he endeavours to show that acid, sugar, and oil, contain all the principles which contribute to compose the animal fluids. An account of the various articles employed in diet, is contained in the second chapter, p. 240 et seq. Fordyce also makes an unfortunate attempt at generalization, in reducing all the nutritious matter to mucilage; Treatise on

Gluten has been considered as the best adapted for the purposes of nutrition of any of the vegetable principles, both in consequence of its being of easy digestion, and of its containing, in proportion to its bulk, the greatest quantity of nutriment. This circumstance depends upon its being the substance, the elements of which the most nearly resemble those of the animal kingdom, hence termed the most animalized of any of the vegetable principles, and this chiefly in consequence of the large quantity of nitrogen which it contains. It exists in the greatest proportion in wheat, while it is found in small quantity only in the other kinds of seeds, or in the parts of plants generally [1].

Next to gluten, in point of importance as an article of nutrition, comes the farina; this is also found copiously in wheat and the other grains, and it likewise forms a considerable proportion of the nutritive parts of the various kinds of pulse and

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Digest. p. 84; but as he admits of a farinaceous mucilage, a saccharine mucilage, &c., it is rather a verbal, than an actual inaccuracy; p. 91..107. In like manner he says that all the animal solids consist of mucilage and water; p. 86. The proximate vegetable principles mentioned in the text, as serving for nutrition, are those which are generally employed by the human species; it appears, however, that certain species of animals can extract nourishment from parts which are not capable of being digested by the organs of man; the beaver, for example, can digest the bark and wood of trees; Blumenbach's Comp. Anat. p. 139. Richerand attempts to show that the alimentary principle is, in all cases, either gummy, mucilaginous, or saccharine; El. Phys. § 3. p. 82. Dumas is disposed to regard mucus as the "principe éminemment nutritif," because, as he says, it forms the basis of our organs and of our humours; Physiol. t. i. p. 187; we may conclude, therefore, that by the term mucus he means albumen. Sir H. Davy, in the third of his lectures on agricultural chemistry, gives a concise view of the proximate principles of the vegetables that are ordinarily employed in diet; p. 73 et seq. M. Magendie classes all alimentary substances under the heads of farinaceous, mucilaginous, saccharine, acidulous, oily, caseous, gelatinous, albuminous and fibrinous; Physiol. t. ii. p. 3, 4. Dr. Prout reduces the stamina or ground-work of all organized bodies to these principles, the saccharine, the oleaginous, and the albuminous; Abs. of his Gulstonian lectures, p. 9.; Dr. Elliotson, ubi supra, adopts the same view of the subject.

[1] We have a detail of the chemical relations of gluten, in Thomson's System, t. iv. sect. 19; Henry's Chem. v. ii. sect. 10; Thenard, Chim. t. iii. p. 392 et seq.; Turner's Chem. p. 902 et seq. It has been resolved by Sig. Taddie, into two proximate principles, which he has named gliadine and zimome; see Ann. Phil. v. xv. p. 390, 1. and v. xvi. p. 88, 9. An account of the original observations of Beccaria will be found in Bonon. Acad. Com. t. i. p. 123 et seq. Vogel examined two species of wheat which are cultivated in Bavaria, the triticum hibernum, and spelta; the former was found to contain 24 per cent. the latter 22 per cent. of gluten; Journ. Pharm. t. iii. p. 212.

of tubers[1]. The nutrition of leaves, stalks, and of seed-vessels, and the green parts of plants, resides in the mucilage which they contain, although, in most cases, this is united with a portion of saccharine matter, which materially contributes to their nutritive powers. Most fruits contain a basis of mucilage or farina, which is combined either with sugar or with oil. In the pulpy fruits, with the exception of the olive, the former chiefly prevails; they generally also contain a quantity of acid, in addition to their other ingredients, but it may be doubted whether the acid serves directly for the purposes of nutrition, or whether it should not be rather considered as indirectly promoting digestion, by its effect upon the stomach or the palate. The principal ingredients of the chestnut, which, in many countries, composes a large share of the diet of the inhabitants, are farina and sugar, while many of the nuts are composed of a basis of albumen, united to a quantity of sugar and oil[2].

Sugar enters into the composition of many vegetable substances that are employed in diet, and although it is generally regarded rather as a condiment, than as a direct source of nourishment, yet it has been supposed to be the most nutritive of all the vegetable principles. Nearly the whole of the sugar that is consumed in Europe is produced from the sugar cane, the juice of which contains it in large quantity and in a state of comparative purity. Sugar is also procured from the root of the beet in considerable quantity, and in some parts of America from the sugar maple[3]. Oil, either animal or vegetable, is commonly employed, more or less, in diet, and is likewise conceived to be highly nutritive; in the warmer climates vegetable oil is principally used, whereas in the colder regions animal oil is employed, as procured from milk, in the form of butter.

There are two points of view in which the articles of diet may be contemplated, either as they are nutritive, or as they are digestible; the former respects their capacity of affording the elements of chyme; the latter, the power which the stomach has of causing them to undergo the necessary change. Between these there is an essential difference, and they do not, by any means, bear an exact proportion to each other. There are many substances, which appear to contain the largest proportion of the elements that constitute chyme, but which are by no means easily digested, and which are rendered more so by being mixed with other substances that are less nutritive[1]. It seems probable that a certain quantity of what may be considered as merely diluting matter, tends to promote digestion, and that in this way, various articles which when taken into the stomach alone are not competent to afford nourishment, become useful when combined with other substances.

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[1] For an account of the chemical relations of farina, see Thomson's Chem. v. iv. sect. 17; Henry's Elem. v. ii. sect. 9; Thenard, Chim. t. iii. p. 211.. 226; and Turner's Chem. p. 851 et seq. According to Braconnot, Ann. Chim. et Phys. t. iv. p. 383, rice contains 85 per cent. of farina; Vogel, Journ. Pharm. t. iii. p. 214, conceives the farina in rice to be as much as 96 per cent.; see also the analysis of rice by Vauquelin, ibid. p. 320.

[2] See Boullay and Vogel on the analysis of the almond; Journ. de Pharm. v. iii. p. 337, 344.

[3] Dr. Thomson gives a list of the plants which contain sugar; Chem. v. iv. p. 81. There was a good deal of discussion among the older writers, respecting the nutritive properties of sugar; Lewis, Mat. Med. v. ii. p. 289, observes that in consequence of its property of uniting oily and watery bodies, it has been supposed by some to enable the unctuous part of the food to unite with the animal juices, while others have conceived, that, from the same cause, it will prevent the separation of the oily part of the food, and thus prevent it from contributing to nutrition; the author properly remarks, that experience has not shown that sugar can produce either of these effects.

. When animals are in their natural state, we find that most of them uniformly adhere to the same kind of food, and this in a very remarkable degree. Thus we observe carnivorous animals feeding only on certain kinds of flesh, and among the herbivorous animals, only on certain plants, or even on certain parts of them, and there are many of the insects which would appear to be exclusively attached to single species of plants. This is not, however, the case with man; for within certain limits, his digestive organs are supposed to be kept in a more healthy state, by a due admixture of different kinds of food, than by any one article taken singly. Stark, who performed a series of experiments on the effect of different kinds of food upon the stomach, which he pursued with remarkable perseverance and apparent accuracy, seems, in the results which he obtained, to have very clearly established the fact, that those substances which afforded the most nutrition, could not be used as the sole article of diet, for any length of time, without the stomach being deranged.

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[1] This distinction is very clearly pointed out by Adelon and Chaussier, in the art. "Digestion," Dict. de Scien. Méd., an article which, although very diffusely written, contains much useful information. In the introduction to Spallanzani's work by Sennebier, we have a detail of a set of important experiments by Goss, on the comparative digestibility of different kinds of alimentary substances. He had acquired the habit of swallowing air, which acted upon the stomach as an emetic, and thus enabled him to bring up its contents at pleasure, and to examine their state in the different stages of the digestive process; p. CXXXI .. CXL. The experiments that have been adduced by Magendie, to prove that a proportion of azote is necessary for the support of animals, in which it was found that animals cannot live, for any length of time, upon pure sugar, oil, or gum, Physiol. t. ii. p. 390, and Ann. de Chim. et Phys. t. iii. p. 66 et seq., I conceive prove no more than that the stomach is not capable of digesting these substances without some addition. Haller observes, that certain animals are destroyed by the use of sugar, although to others it proves highly nutritive and salutary; El. Phys. xix. 3. 12. In Stark's experiments we have many examples of the indigestible nature of a diet composed of a single article, which was easily digested when mixed with other substances; Exper. on Diet, in his works, p. 89 et seq. Magendie, Ann. de Chim. et Phys. t. iii. p. 76, refers to Stark's experiments on sugar, as conceiving them to prove the incompetency of substances that contain no nitrogen to the nutrition of the body. But, although Stark found that his digestion was deranged by the excessive use of sugar, I conceive that his experiments show, that his health was as much affected, by the exclusive or excessive use of other articles which contain a sufficient proportion of nitrogen. In order to render Magendie's experiments unexceptionable, it would be necessary to employ a diet which should be composed of a mixture of substances, all of them without nitrogen, as farina, mucilage, or gum, mixed with sugar or oil. Dr. Prout, in the essay to which I have already referred, observes that the most perfect diet is composed of a mixture of the three principles which he supposes to form the basis of all organized bodies, the saccharine, the oily, and the albuminous.

It is found also that there are great differences in the digestive powers of different individuals among the human species, some stomachs requiring a preponderance of animal, and others of vegetable food. There are many substances, which, although nutritious and salutary to certain individuals, appear incapable of being digested by others, and this is often the case, when it is very difficult to conceive to what ingredient we are to assign this peculiarity of effect. Much may often be ascribed to the effect of habit and accidental association, or even of mere caprice, but, upon the whole, we may conclude, that there are original variations in the powers of the stomach, which cannot be accounted for upon any other principle, either moral or physiological.

That this difference exists with respect to animals of different species is sufficiently obvious. Besides the two great divisions of carnivorous and herbivorous, we find decided differences in each of the two classes. Among the carnivorous animals, we observe some feeding entirely upon the flesh of quadrupeds, some of birds, and others of insects. Among those that live upon vegetables, we likewise find that they have each their peculiar partialities for certain parts of plants; the seeds, the fruits, the leaves, &c.; and we can frequently trace a manifest connexion between the substance on which they feed, and the structure of the teeth, so as to show that the selection is not the effect of accident or arbitrary choice, but that it is connected with the conformation of the body, and depends upon the permanent structure of the organs. With respect to the teeth; we meet with some that are manifestly adapted for seizing and biting, others for tearing or lacerating their prey; some that are more proper for cropping the succulent and delicate parts of plants, others for the protracted mastication of those that are more firm and dense in their texture. The beaks of birds are infinitely diversified in their form and structure: some long and pointed, some broad and flat, and others hooked or curved, so as to be most clearly fitted for the reception of certain kinds of food only; and we find, in all cases, that the nature of the stomach, whether membranous, muscular, or ruminant, whether simple, as consisting of one cavity only, or compounded of several, precisely corresponds to that of the teeth, and to the other organs and habits of the individual.

Liquids of various kinds constitute an important part of diet. These may be arranged under two heads[1]; first, the different decoctions or infusions, either animal or vegetable, where various substances are dissolved or suspended in water, and where the specific properties of the liquid depend almost entirely upon that of the substance which is added to the water, or where we employ liquids for the mere purpose of quenching thirst, or enabling the stomach to act more readily upon the aliment. On the first class of substances, which are, as it were, intermediate between solids and fluids, I have already had occasion to offer some remarks. Their properties may be considered generally as very similar to those of the substances which compose them; being necessarily formed of the most tender and succulent parts alone, they are, in most cases, proportionably nutritious and digestible, yet there are certain constitutions or states of the system, in which the quantity of the fluid necessarily introduced appears unfavourable to the process of digestion. This may be conceived to operate in various ways; it may act by unduly distending the stomach, so as to interfere with the vermicular motion, or, in consequence of its bulk, it may stimulate the muscular fibres to contract too rapidly, and expel the food before it has undergone its appropriate change; it may, perhaps, dilute the gastric juice, or from the consistence and temperature of the alimentary mass, a tendency to fermentation may be induced, or to some other chemical change, which may interfere with the regular and healthy action of the organs. The same remarks, with respect to its action upon the stomach, apply to milk, which, strictly speaking, should be classed among the nutritive fluids; but the farther prosecution of this inquiry belongs rather to the province of the physician than the physiologist.

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[1] Boerhaave, Prælect. t. i. § 56; Haller, El. Phys. xix. 3. 20 .. 6; Sœmmering, Corp. Hum. Fab. t. vi. p. 250 .. 2. § 162; Magendie, Physiol. t. ii. p. 6, arranges drinks under the four classes of water, vegetable and animal infusions, fermented liquors, and alcoholic liquors.

Among drinks, properly so called, the most important is water, which is, perhaps, not only the best substance for quenching thirst, but is, with a few exceptions, the vehicle of all the rest. These may be classed under the two heads of vegetable infusions and fermented liquors. Of the former, those that are the most commonly used in Europe, are coffee and tea, both of which afford a useful and salutary beverage, when not taken to excess, which, although not themselves directly nutritive, seem to render the stomach more capable of digesting its contents.

Fermented liquors, of some kind or other, we find to be employed by all people that have made any considerable advance in the arts of life, and in civilization; in this country, they are principally made from barley, by means of the sugar which is evolved during the process of germination; in the other parts of Europe, the juice of the grape is principally employed; and in many parts of the world, various saccharine and mucilaginous juices are used for the same purpose. These liquors, like the vegetable infusions, if not taken of immoderate strength, or in undue quantity, are grateful and salutary, and seem to promote digestion, while they are likewise themselves, to a certain extent, capable of affording nutrition, in consequence of the portion of undecomposed sugar and mucilage which they generally contain. I think, however, we can scarcely extend this indulgence to distilled spirits, for, although they are occasionally valuable as medical agents, they must be always more or less pernicious, when made habitual articles of diet.

A third class of substances remains to be noticed, such as are not in themselves nutritive, but which are added to our food for the purpose of giving flavour to it, styled condiments. These are very numerous, and derived from very different sources, but they may be all reduced to the two heads of salts and spices. Their selection appears to depend upon very singular habits or even caprices, so that those substances which are the most grateful to certain individuals and classes of people, are the most disagreeable, or even nauseous, to others. It may be laid down as a general principle, that such articles as are, in the first instance, disagreeable to the palate, are those for which we afterwards acquire the strongest partiality, and which even become necessary for our comfort; whereas the frequent repetition of flavours that are originally grateful, is very apt to produce a sense of satiety, or even of disgust. The examples of tobacco, garlic, and assafœtida, on the one hand, and of such substances as possess simple sweetness on the other, may be adduced in proof of this position.

There is such a very general relish for sapid food, among all descriptions of people, and in all states of civilization, as to induce us to suppose that besides the mere gratification of the palate, some useful purpose must be served by it, and that it must contribute, in some important manner, to the digestion of our food. This peculiar kind of taste, with a few exceptions, does not seem to exist among the inferior animals, who generally prefer the species of food, which is best adapted to their organs, in a simple state. Perhaps, this may be, in some measure, explained by the consideration that man differs from other animals in his capacity of existing in all climates and in all situations, and that, in order to enable him to do this, it was necessary that he should be omnivorous, as it is termed, that is, able to digest any substance which affords the elements of nutrition. But, as in the process of chymification, it appears that all the aliment received into the stomach must be reduced to a mass nearly uniform in its constitution, it is reasonable to suppose that some assisting or correcting substances may be requisite to reduce the various species of aliment to one uniform standard. Vegetable substances, for example, when reduced to a soft pulp, and macerated at the temperature of the stomach, would probably have a tendency to the acetous fermentation, which we may presume will be corrected by the addition of aromatics and spices, whereas a mass of animal matter may be prevented from degenerating into the putrid state by salts or acids. Both these articles seem to be originally agreeable to the palate; and as a general rule, we shall find that they are naturally produced in the regions where they are the most required. The different species of brute animals exist in those countries alone where there is a supply of food adapted to their digestive organs, and as they are guided in their choice of the articles of diet by instinct, they do not require the aid of these correctives.

There is, however, one remarkable exception to this general rule, in the relish which many animals of the higher orders seem to have for salt. Besides the various kinds of fish that inhabit the sea, and the birds that feed upon marine animals, we find that many quadrupeds and land birds clearly indicate their fondness for salt, and we have daily examples presented to us of its salutary operation, when either incidentally or intentionally mixed with the food of animals. Many singular instances are mentioned of the extraordinary efforts which they often make to procure it, when it is otherwise difficult to obtain. The beasts of prey that inhabit the central parts of the African and American continents, are known to travel immense tracts for the purpose of visiting the salt-springs that are occasionally met with, and it is said that these springs have been in some instances discovered by means of their footsteps, and by the hovering of birds over them[1]. At the same time that we thus find animals to be led by instinct to the use of salt, we perceive that the human species are induced to employ it from its grateful effect upon the palate; for it may be remarked, that among all the singular diversities of tastes that exist among nations and individuals, there are no people, from the most barbarous to the most refined, who do not relish a certain portion of salt in their food[2]. It may, perhaps, be thought not an unreasonable conjecture, that as salt always exists in the blood and the other fluids, and must therefore be supposed to be of some essential use in the animal œconomy, there is a provision made for a regular supply of it in the constitution of our organs, and in our natural propensities and instincts.

There is a numerous class of substances, which are somewhat analogous to the condiments in their effects upon the stomach, although very different in their action upon the palate, the various medicaments. These do not afford nutrition, but they many of them tend to put the stomach into a state which adapts it for the digestion of aliment, and they produce upon the system generally, or upon some of its organs, certain changes, of which we take advantage in correcting its diseased actions and restoring its powers, when perverted or weakened. The farther prosecution of this subject does not fall within the province of the physiologist, but I may observe that the operation of medicines affords us many interesting examples of the nature of the vital functions, and conversely, that a correct knowledge of these functions must very materially contribute to guide us in the selection and administration of these substances. I may remark, that condiments and medicines differ in one essential circumstance from the articles of diet; that whereas the latter are always resolved into their ultimate elements, before they contribute to nutrition, the former act in their entire state, and when decomposed, cease to produce their appropriate effects[1]. Some of the substances which possess the most powerful action over the system are derived from the vegetable kingdom, and are therefore composed of the same elements with our ordinary articles of food, only combined in different proportions; and even the most active mineral or metallic substances become inert when they are resolved into their elementary constituents[2].

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[1] We have some curious facts of this description in Mr. Hodgson's interesting Letters from N. America; v. i. p. 240, 1. note.

[2] Haller, El. Phys. xix. 3. 11; Fordyce on Digestion, p. 55. I believe that the opinion which appeared to be established by the experiments of Pringle, that although salt is powerfully antiseptic under ordinary circumstances, yet that it promotes the decomposition of alimentary matter, when added to them in small quantity, Appendix, p. 351, 2, is now generally supposed to be without foundation.

There is perhaps no substance whose operation on the animal œconomy is more violent than the hydrocyanic acid, yet this is entirely composed of carbon, hydrogen, and nitrogen. The acrid extracts and the narcotic alkalies that are procured from vegetables, all consist of different proportions of oxygen, hydrogen, and carbon, to which, in some cases, a quantity of nitrogen is added. The pure metals appear to exert no action upon the system, although many of their oxides and salts are so acrid. Phosphorus differs from other bodies of the same class, in being more active in its simple, than in its compound state. The three elementary bodies, chlorine, iodine, and fluorine, which agree in many of their chemical relations, resemble each other also in their powerful action on the living body, and this violence of action they retain both in their simple and in their compound form.

What we commonly term poisons, are so denominated in consequence of the popular conception of their effect upon the system, but in reality, they do not essentially differ from medicaments. The very powerful operation which they produce, when under due regulation, is, perhaps in every instance, capable of being converted to some salutary purpose, and is only noxious when carried to an excessive degree.

SECT. 3. *Changes which the Food undergoes in the Process of Digestion.*

I proceed, in the third place, to describe the successive changes which the food experiences, from the time when it is taken into the mouth, until it is converted into perfect chyle[1]. The first necessary step is a due degree of mechanical division, in order to prepare the aliment for the chemical changes which it is subsequently to undergo; this division, as has been observed above, is accomplished in various ways, according to the structure of the parts concerned, either by mastication, trituration, or maceration, or by a union of the three operations. After the requisite mechanical change, the food is transmitted into the proper digestive stomach, and is there reduced into the soft pultaceous mass, termed chyme. It has been stated in a general way, that the specific odours, and other sensible properties of the alimentary matters employed, are no longer to be recognized in the chyme, but that whatever species of food be taken into the stomach, the resulting mass is always the same[2].

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[1] Adelon, Dict. des Sc. Méd. t. ix. p. 358.

[2] Fordyce observes, that certain insects live entirely upon cantharides, yet their fluids are perfectly mild; On Digestion, p. 86; also that the poison of the rattle-snake is perfectly innocent when taken into the stomach; p. 119.

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[1] Dr. Prout, in a very valuable paper, which is inserted in the 13th and 14th vols. of Ann. Phil., describes in succession the different stages which the aliment experiences, from its reception into the stomach until it is finally converted into blood. He divides the whole process into the four stages of digestion, chymification, chylification, and sanguification, which he conceives are performed respectively by the stomach, the duodenum, the lacteals, and the pulmonary blood vessels. Many of Dr. Prout's observations are original and of undoubted accuracy, while the remarks which he offers upon the facts are highly ingenious and deserving of great attention; but I think it is to be regretted, that he has occasionally employed a different nomenclature from that generally adopted, and one from which I do not perceive that any particular advantage is to be derived. He, for example, employs the word chyme, not to signify the mass into which the aliment is converted in the stomach, but "that portion of the alimentary matter found in the duodenum, which has already, or is about to become albumen, and thus to constitute a part of the future blood." It must be observed, that the term albumen is likewise employed after the example of Berzelius in a somewhat unusual sense, to designate the substance which may be regarded as the first rudiments of the blood, being applied collectively to its three principal ingredients, the serum, fibrin, and globules, in their incipient state. A minute, and as it would appear, a correct description of the appearances which the aliment presents from the action of the gastric juice is given by Dr. Philip; Inq. ch. 7. sect. 1. p. 140..155. The principal facts which he states are that the new and old food are always kept distinct from each other, the former being in the centre of the latter; the food is more digested the nearer it is in contact with the surface of the stomach; it is the least digested in the small curvature, more at the larger end, and still more in the middle of the great curvature; the state of the food in the cardiac differs from that in the pyloric portion; in the latter it is more completely digested and more uniform in its consistence; it is also more compact and dry in this part; it appears that the act of digestion is principally performed at the large end of the stomach, that the mass is gradually moved forwards to the small end, becoming more digested as it advances; we may presume that the secretion of the gastric juice principally goes on at the large extremity, and that its chemical action on the aliment takes place in this part, whence it is slowly propelled to the pylorus. It is accordingly the great end of the stomach which is found to be digested after death by the action of the gastric juice upon it. See Magendie's remarks on the formation of chyme; Physiol. t. ii. p. 81, 2; also Mr. Mayo's Physiol. ch. 7. sect. 4, for many useful observations on the action of the gastric juice on the contents of the stomach.

[2] Haller, El. Phys. xix. 4. 24. 31; see the remarks of Tiedemann and Gmelin on this subject, in the 3d section of their researches.

As far as respects the same kind of animal, when the nature of the food employed is not very dissimilar, and all the functions are in a perfectly healthy and natural state, the position may be admitted, but it is not true to the extent in which the assertion has been made, for we learn from experiment that the chyme produced from vegetable matter differs sensibly from that of animal origin, and it can scarcely be doubted, that in the different states of the stomach of the same individual, even where the same kind of food has been used, the chyme does not always possess precisely the same qualities. But this remark may probably apply to the stomach only, when its actions are deranged, in which case these deviations must be regarded as partaking of the nature of disease, and consequently affording no indication of the natural state of the function.

Although the properties of chyme have been frequently examined, and various experiments performed upon it, still there is considerable obscurity respecting the nature of the process by which it is formed, and we are not able to account satisfactorily for the effect which is produced. The operation may be considered as analogous to the effect of a proper chemical action, where the body is not merely divided into the most minute parts, and has its aggregation completely destroyed, but, at the same time, acquires new chemical properties. That this kind of solution of the food takes place is proved by the experiments of Reaumur[1], Stevens[2], and Spallanzani[3]. They inclosed different alimentary substances in balls, or in metallic spheres, or tubes, that were perforated with holes, or in pieces of porous cloth; these were introduced into the stomach, and after being suffered to remain there for a sufficient length of time were withdrawn, when it was found that the inclosed substances were more or less dissolved, while the substance containing them, whether metal or cloth, was not acted upon, thus proving that the effect was not of a mechanical, but entirely of a chemical nature.

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[1] Mém. Acad. pour 1752, p. 266 et seq., and p. 461 et seq.

[2] De Alimentorum Concoctione. Stevens took advantage of a man who had been in the habit of swallowing stones, and afterwards rejecting them from the stomach. He caused this individual to swallow hollow metallic spheres, perforated with numerous orifices, and filled with different kinds of alimentary matters; c. 12. ex. 1..9; he afterwards pursued the experiments upon dogs; he caused these animals to swallow the perforated spheres, and after some time destroyed the animals and examined the state of the aliment; Ex. 11..23.

[3] Expér. sur la Digest. The experiments of Spallanzani on this subject are quite decisive, and so varied and multiplied, as to meet every objection that could be urged against them. He was perfectly indefatigable in the pursuit of truth, and he presents us with the rare example of a philosopher who errs rather from the excess than the deficiency of the experiments which he adduces in order to establish a point, which might often have been as satisfactorily proved by a smaller number. The principal object of the first of his dissertations is to show, that in granivorous birds, the action of the gizzard is necessary in order that the gastric juice may act upon the hard and unmasticated food which usually composes their diet. In the second he illustrates the action of the gastric juice in animals that possess what he styles intermediate stomachs; in the third, fourth, and fifth, he extends his observations to the membranous stomachs of the amphibia, fish, various quadrupeds, and lastly of man. See also Blumenbach, Inst. Physiol. § 358; and Monro (Tert.) Elem. v. i. p. 532.

The results of these experiments have been confirmed by some remarkable facts, which bear still more directly upon the point under investigation; where certain individuals have had preternatural openings made into the stomach, either from accident or disease, while the functions of the part appear to have been but little affected. By this means, the operation that is going forward in this organ may be minutely watched in all its stages, and we are enabled to observe the changes which the food undergoes, from the time that it enters the stomach, until it passes from the pylorus, and to compare the changes which different kinds of food experience during its progress.

A case of this kind is related by Circaud[1], where an individual lived many years with a fistulous opening into the stomach; but a much more remarkable case of the same description has been lately detailed by Dr. Beaumont. The individual in question was wounded early in life by a shot in the epigastric region, which perforated the stomach. After some time the wounded part healed, with the exception of an aperture two inches and a half in diameter, which communicated with the stomach. He lived many years in this state, in perfect health and vigour, so as to be capable of following a laborious occupation, while the fistulous opening still remained. Under these circumstances he was made the subject of experiment by Dr. Beaumont, who, for the space of eight years, continued his observations with great assiduity and accuracy, both on the action of the stomach in its ordinary state, and when subjected to different conditions for the immediate purpose of the experiment.

We may remark generally, that the results of these experiments confirm those of Spallanzani, in their most essential particulars, and at the same time enable as to decide upon some points which he left imperfect[2]. Among the most important points respecting the formation of chyme, which appear to be confirmed by the observations of Dr. Beaumont, are the following: that the different kinds of aliment all require to undergo the same process by means of the gastric juice, in order to be reduced into chyme; that the rapidity of the process differs considerably according to the delicacy of their natural texture, or the degree of their mechanical division; that animal substances are more easily converted into chyme than vegetables, that oily substances, although they contain a great quantity of nutriment, are comparatively difficult of digestion, and that the saliva is of no specific use in the conversion of aliment into chyme[1].

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[1] Journ. Phys. t. liii. p. 156, 7.

[2] Beaumont on the gastric juice, sect. 1, 5.

The opinion which is commonly entertained respecting the production of chyme, and the one which appears to be sanctioned by our experiments is, that the glands of the stomach secrete a fluid of a peculiar kind, which is named gastric juice[2], that this acts as a solvent for the food, and that the chyme is a solution of the alimentary matters in this juice. This has been supposed to be proved by direct experiment. For, besides the facts that were ascertained by Spallanzani, respecting the solution of the alimentary substances that were inclosed in the tubes, he procured the gastric juice from the stomachs of various animals, sometimes by causing them to vomit after fasting, and, perhaps still more effectually, by introducing sponges, which after some time were withdrawn, and the fluid pressed from them. Although in this way the glands of the stomach would probably experience an unusual excitement or irritation, which might be supposed to affect the nature of the secretion, yet the fluid which was procured by this means was found to act upon various alimentary matters very much in the manner which we conceive the gastric juice to do. When they were digested with it for some time, at a temperature equal to that of the human body, a kind of imperfect chymification was found to be produced, perhaps as nearly resembling the natural process as could be expected from the imperfect manner in which the experiment was necessarily performed. This imperfection attaches both to the mode in which the gastric juice was procured, and also to the want of the vermicular action which belongs to the living stomach, by which, during the whole period of the process of digestion, the aliment is continually in motion, fresh portions of it being exposed to the action of the solvent, and the whole gradually pushed forwards from the cardia to the pylorus.

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[1] Page 275..8 et alibi. In connexion with this opinion of Dr. Beaumont's respecting the saliva, I may mention the speculation of Tiedemann and Gmelin, that the sulpho-cyanate of potash, which they detected in this secretion, served to destroy "la faculté vitale de se contracter" in the alimentary matter; Recherches, t. i. p. 330.

[2] For an account of the gastric juice, see Boerhaave, Prælect. § 77 et seq.; Haller, El. Phys. xix. 1. 15, and xix. 4. 20; in the former of these authors we have a copious list of physiologists and chemists, who have procured it and examined its properties. Among others, the student may consult Réaumur, Mém. Acad. pour 1752, p. 480, 495; Fordyce on Digestion, p. 62; Hunter on the Anim. Œcon. p. 214, 5; Blumenbach, Inst. Phys. § 357; Bell's Anat. v. iv. p. 58 et seq.; Richerand, El. Phys. § 20. p. 105..8; Sœmmering, Corp. Hum. Fab. t. vi. p. 266. § 173; Cuvier, Leçons, t. iii. p. 362..6; Magendie, Phys. t. ii. p. 199; Monro (Tert.) Outlines, v. ii. p. 119..122, and El. v. i. p. 527 et seq. Spallanzani made it the subject of very numerous experiments, Expér. § 81 et seq., 145, 185, 192; the only properties which he detected in it were that it was slightly salt and bitter; see also the analysis of Scopoli in the same work, § 244, which seems to have been made with much accuracy; his conclusion is, that the gastric juice contains water, an animal substance, "savoneuse et gélatineuse," muriate of ammonia, and an earthy matter, similar, as he says, to what is found in all animal fluids. See also Dr. Prout, in Ann. Phil. v. xiii. p. 13; and Phil. Trans. for 1824, p. 45 et seq. Senebier gives us an account of various experiments that were performed by Jurine and others on the use of the gastric juice in healing wounds and ulcers; Journ. de Phys. t. xxiv. p. 161 et seq.; but it is unnecessary to remark upon the uncertainty of such experiments. We have in the same paper an account of Carminati's analysis of the gastric juice; he found it salt and bitter, and frequently acid; he points out its antiseptic properties, which are the most powerful in carnivorous animals, and appear to be connected with its acidity; it is also said to contain a little volatile salt, and a larger quantity of a muriatic salt; p. 168 et seq. The latest analysis which we have had of the gastric juice is that of Tiedemann and Gmelin, which forms part of their elaborate researches on digestion, to which I have already had occasion to refer. The dog and the horse were the animals employed in these experiments. We learn from them, that not only the quantity, but the nature of the fluid secreted by the stomach is considerably affected by the vital actions of the part, either as excited by food or even by mechanical irritation. In this latter case it always exhibits acid properties, and contains the muriatic and acetic acids in the uncombined state. The existence of the first of these acids, it is well known was announced by Dr. Prout, and the authors assure us, that they made the same discovery in February, 1824, a month before they had read Dr. Prout's paper. They conceive that the acetic acid is evolved during the process of digestion, and to this is referred the lactic acid, which is said to have been detected in the stomach, this acid being, as they suppose, merely a modification of the acetic; this, however, would appear not to be the case, from the recent experiments of Guy-Lussac, and Pelouze; Ann. Chim. et Phys. t. lii. p. 410 et seq. An acid, which is named the butyric, is said to have been occasionally found in the stomach of the horse; Rech. t. i. p. 166, 7. Here again, as we found to be the case with the saliva and the pancreatic juice, we meet

It is, however, not a little remarkable, that when the gastric juice has been examined with relation to its chemical properties, nothing has been detected in it which appears adequate to the effects we observe to be produced. As far as we can judge, it nearly resembles saliva, or the ordinary secretions of the mucous membranes, substances which indicate no active properties, and which we are disposed to regard as altogether very inert in respect to their action upon other bodies. From the

with the same variation between the experiments of Tiedemann and Gmelin, and those of Leuret and Lassaigne. While the German professors agree with Dr. Prout, that muriatic acid is found in the contents of the stomach, the French chemists suppose, that the uncombined acid is the lactic; they inform us that they have frequently repeated their observations, and always with the same result; they particularly enter into the consideration of the process employed by Dr. Prout to ascertain its presence, and they announce it to be erroneous; Recherches, Art. 4. p. 94 et seq. Independently, however, of the well known accuracy of this chemist, the fact of the presence of muriatic acid in the contents of the stomach is now so fully confirmed by other experimentalists, as to leave no reasonable doubt of its existence in the ordinary and healthy state of the digestive process. See especially Braconnot, Ann. Chim. et Phys. t. lix. p. 356, 7. Dr. Prout has, moreover, replied to the animadversions of Leuret and Lassaigne, and as I conceive, satisfactorily refuted them, in Ann. Phil. v. xii. p. 405 et seq.

experiments of Dr. Prout we learn, indeed, that a quantity of muriatic acid is present in the stomach during the process of digestion[1], but there does not appear to be any evidence of the existence of this acid before the introduction of the food into the stomach, so that we may rather infer that it is in some way or other developed during the process of digestion, than that it is the efficient cause of it. After it is developed we may indeed conclude that it essentially contributes to the completion of the process, although we are not, I think, able to explain the mode in which it operates[2].

But notwithstanding this uncertainty about the nature and operation of the gastric juice, many circumstances prove that the stomach, or rather the fluid which it secretes, possesses a proper chemical action, because its effect upon bodies submitted to it, bears no proportion to their mechanical texture or other physical properties[3]. Thus the secretion from the stomach acts upon dense membrane and even upon bone, reducing them to a pulpy mass, while, at the same time, many bodies of comparatively delicate texture, as the skins of fruits and the finest fibres of flax or cotton, are not, in the smallest degree, affected by it. This selection of substances so exactly resembles the operation of chemical affinity, and is so directly contrary to what would be the effect of mere mechanical agency, as to prove beyond a doubt, that a chemical action takes place in the stomach, and we are only induced to hesitate in giving our assent to this opinion, because the qualities of the agent appear to be so unlike what we might expect to find in a body capable of producing such powerful effects.

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[1] Phil. Trans. for 1824, p. 45 et seq.

[2] We have many accounts among the earlier physiologists of acid being detected in the stomach during digestion; it was one of the essential doctrines of Vanhelmont, that an acid is developed by the action of the stomach upon the aliment, in what he styled the first digestion; Ortus Med. p. 164..7 et alibi; also Willis de Ferment. Op. t. i. p. 25; this acid, however, was generally supposed to depend upon a morbid condition of the organs. Haller says, that in its recent and healthy state, the chyme is neither acid nor alkaline; not. ad § 77. Boer. Præl. t. i. p. 138; and El. Phys. xix. 1. 15; but he afterwards informs us, that in some experiments, which were made at his request by Rastius, a tendency to alkalescency was detected in it. He classes the acescency of the stomach, along with fermentation, putrescence, and rancidity, under the title of "Corruptio Varia;" Ibid. xix. 4. 29. Fordyce also states, as the result of his own experiments, as well as of Hunter's, that the contents of the stomach are not necessarily acid; on Digest. p. 150, 1. We have a well written anonymous essay in Duncan's Med. Com. v. x. p. 305 et seq., to show that the stomach does not contain any acid. Spallanzani makes the presence of acid during digestion a distinct object of inquiry, and decides, that although it is occasionally present, it is not essentially so; Exper. § 239..245; See Hunter's remarks, in Anim. Œcon. p. 293 et seq. In the case related by Circaud, which was referred to above, of an individual who lived many years with a fistulous opening into the stomach, the contents appear to have been generally acid. Dumas relates a series of experiments which he performed upon this subject, the results of which led him to conclude, that the gastric juice is not necessarily acid, but that it occasionally becomes so by the use of certain aliments that are disposed to go into the acid state; El. Phys. t. i. p. 278..0. Those curious cases of individuals, who have been in the habit of swallowing large pieces of iron, a remarkable example of which is related by Dr. Marcet, in Med. Chir. Tr. v. xii. p. 52 et seq., indicate the presence of an acid in the stomach, by the corrosive effect produced on the metal. Another case of this description has been lately published by Dr. Harrison; Dublin Med. Journ. v. viii. p. 1 et seq. Dr. Carswell conceives that acidity is an essential property of the gastric juice, and that upon this property its specific action mainly depends; he styles it the gastric acid; Pathol. Anat. fas. 5. I have stated above the results of the experiments of Tiedemann and Gmelin on this point, and have also referred to the decisive experiments of Dr. Prout.

[3] Montegre has performed a series of experiments which appear to have been executed with care and assiduity, but which lead us to an opinion respecting the nature of digestion very different from what has been generally

Besides the property of dissolving the aliment and reducing it to the state of chyme, the gastric juice produces two other effects which are decidedly of a chemical nature, the coagulation of albuminous fluids[1] and the prevention of putrefaction. It is as difficult in this case as in the former to explain how these properties can be attached to such a substance as chemical analysis would indicate the gastric juice to be. It is upon the coagulating power of the gastric juice, as residing on the surface of the stomach, that the method of making cheese depends. What we term rennet consists of an infusion of the digestive stomach of the calf, and by adding this to milk, we convert the albuminous part of it into the state of curd, which is formed into a dense mass by pressing out the more fluid part from it. But here again the difficulty occurs which was alluded to above, that we are at a loss to conceive, by what property in the gastric juice it is that this coagulation can be effected. The quantity of the rennet which is sufficient

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adopted. He possessed the faculty of being able to vomit at pleasure, and in this way obtained the fluid from his stomach for the purposes of examination and experiment. He conceives that what has been supposed to be the gastric juice is in fact nothing more than saliva, that it possesses no peculiar power of acting upon alimentary matter, that the principal use of the gastric juice is to dilute the food, and that the only action of the stomach consists in "une absorption vitale et élective," in which the absorbent vessels, in consequence of their peculiar sensibility, take up certain parts of the food and reject others; Expér. sur la Digestion, p. 20 et seq.; see the general conclusions in p. 43, 4. That the conclusion of Montegre respecting the action of the stomach cannot be maintained, is correctly observed by Berthollet, in his report upon the memoir, because, until the food has undergone a certain chemical change in the stomach, the substance does not exist which is taken up by the absorbents; and further, that whatever may be our opinion respecting the nature of the gastric juice, or the mode in which it operates, it has been unequivocally proved that it can act upon substances which are not in contact with the stomach, provided they are exposed to its secretions; p. 53, 4. The conclusion of Montegre is, however, implicitly adopted by Adelon and Chaussier; Dict. des Sc. Méd. t. ix. p. 422, 3.

[1] Fordyce, on Digest. p. 57, 9; 176 et seq. Fordyce, to show the extent of the coagulating power, remarks, "the six or seven grains of the inner coat of the stomach infused in water gave a liquor which coagulated more than a hundred ounces of milk." See also Prout. Ann. Phil. v. xiii. p. 13 et alibi.

to produce the effect is so extremely small that it is difficult to imagine, how so minute a portion of a substance which seems to possess no properties of an active nature, can produce such very powerful effects. I think that the present state of our knowledge will not enable us to explain the difficulty, but it must be remembered that the process of coagulation is itself one of the most mysterious operations in chemistry, that it is produced by a variety of substances, the action of which we are not able to reduce to any general principle, and the nature of which we are unable satisfactorily to explain.

The other peculiar effect of the gastric juice, its antiseptic power, is no less difficult to explain than its coagulating property, yet there is no doubt of its existence, for it was distinctly ascertained by Spallanzani and others[1], that in carnivorous animals, who frequently take their food in a half putrid state, the first operation of the stomach is to remove the fœtor from the aliment that is received into it. The same power of resisting putrefaction is also manifested in experiments that have been made out of the body, in which it was found equally efficacious in resisting putrefaction, or even in suspending the process when it has commenced. The antiseptic power of the gastric juice is perhaps still more extraordinary and inexplicable than its coagulating property; we can only say concerning it, that it is a chemical operation, the nature of which, and the successive steps by which it is produced, we find it difficult to explain, at the same time that we have very little in the way of analogy which can assist us in referring it to any more general principle, or to any of the established laws of chemical affinity.

The conclusion then to which we are reduced is, that when the food has undergone a sufficient degree of maceration and mastication, or other mechanical process by which it is reduced to a state of sufficiently minute division, the fluids that are secreted from the surface of the stomach act upon it, so as to produce a complete change in its properties. This is analogous to a chemical change in all its relations, but it is remarkable from the difficulty which we have in conceiving how so considerable an effect can be produced by so apparently inert a substance. During the process of chymification heat is occasionally extricated, and not unfrequently gas is evolved[2]; both of these, however, would appear not to be necessary steps in the process; but the consequence of a morbid state of the function. Pre-

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[1] Expériences, § 250..3 et alibi; Hunter on the Anim. Œcon. p. 204. Montegre does not admit the existence of this antiseptic property; Sur la Digestion, p. 21 et alibi; his experiments would likewise deprive it of its coagulating power, of which, I conceive, it is impossible to doubt. The experiments of Dr. Thackrah have led him to deny the antiseptic property of the gastric juice; Lect. on Digest. p. 14; but with every feeling of respect for this author, I may remark, that it would require a considerable number of negative results to set aside the force of the positive statements which we possess on this subject.

[2] The nature of the gases that are found in the different parts of the alimentary canal has been examined by Jurine and Chevreul. Jurine's results show that as we recede from the stomach, the proportion of oxygen and car-

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bonic acid decreases, while that of nitrogen increases; and that the proportion of hydrogen is greater in the large than in the small intestines, and less in these than in the stomach; Mém. Roy. Méd. Soc. t. x. p. 72 et seq. Chevreul analyzed the gas in the stomach, the small, and the large intestines, with the following results:—

In the Stomach.

Oxygen	11
Carb. acid	14
Hydrogen	3·55
Nitrogen	71·45
	100·0

In the Small Intestines, in different Subjects.

Oxygen	0·0	0·0	0·0
Carb. acid	24·39	40·0	25·0
Hydrogen	55·53	51·15	8·4
Nitrogen	20·08	8·85	66·6
	100·00	100·00	100·0

Gas in the Great Intestines of the first of the three Subjects.

Oxygen	0·0
Carbonic acid	43·5
Carburetted hydrogen with a trace of sulphuretted ditto	5·47
Nitrogen	51·03
	100·00

Of the second.

Oxygen	0·0
Carbonic acid	70·0
Hydrogen and carburetted hydrogen	11·6
Nitrogen	18·4
	100·0

In the Cæcum of the third.

Oxygen	0·0
Carbonic acid	12·5
Hydrogen	7·5
Carburetted hydrogen	12·5
Nitrogen	67·5
	100·0

In the Rectum of the same.

Oxygen	0·0
Carbonic acid	42·86
Hydrogen	0·0
Carburetted hydrogen	11·18
Nitrogen	45·96
	100·00

Magendie, Phys. t. ii. p. 85; 104, 5, 112, 3.

We have a long and elaborate essay on this subject by Chevillot, Magendie's Journ. t. ix. p. 287 et seq.

vious to the experiments of Dr. Prout, the generation of acid in the stomach was regarded in the same point of view, but we are now led to conceive that it is a necessary part of the digestive process, and that it contributes, in some way or other, to the formation of chyme. It still remains to be inquired, whether in all cases of what is termed acidity of the stomach, the acid formed be the muriatic, in what exact stage of the process it is produced, from what source the acid is immediately derived, and how it is ultimately disposed of.

I have already given some account of the vermicular motion of the stomach, or that alternate contraction and relaxation of the muscular fibres, by means of which its different parts are successively brought into action, which is propagated in a regularly progressive manner along the whole of the organ. This motion is evidently intended to mix all the parts of the alimentary mass intimately together, and to apply each portion in succession to the surface of the stomach, so as to bring it into contact with the gastric juice. It is principally produced by the contraction of the circular or transverse fibres, while it is probable that the longitudinal fibres have more effect in propelling the contents of the stomach from the cardia to the pylorus.

When the contents of the stomach enter the duodenum they are subjected to a new action, and undergo a farther change in their constitution and physical properties, by which the chyme is converted into chyle[1]. The nature and properties of chyle have been examined with more minuteness and accuracy than those of chyme, although with respect to the mode of its production we are still less able to offer any satisfactory information. We seem to be in possession of little more than the general fact, that the contents of the stomach, soon after they pass into the duodenum, begin to separate into two parts, a white substance which constitutes the chyle, being detached from the mass, while this, which consists of the residual matter, is converted into fæces, and finally rejected from the system. At the same part of the intestinal canal where this separation takes place, the duodenum receives the secretions from the biliary and pancreatic ducts; and it is stated, that the chyle assumes its most perfect form, or is produced in the greatest quantity, about the orifices of these vessels, so as to indicate some connexion between the process of chylification, and the action of the bile and pancreatic juice. We have, however, no very direct proof of any action taking place between these fluids and the chyme, so as to convert it into chyle, nor are we able to offer any explanation of the nature of the effect which they might be supposed to produce upon it. The pancreatic juice appears to be, in all respects, similar to the saliva, and although

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[1] From the remarks that were made above, p. 539, it will appear that the older physiologists were not thoroughly aware of the distinction between *chyme* and *chyle*, and still less were they acquainted with the exact part of the digesting organs in which the latter made its appearance. From the expressions which are employed by Boerhaave; Prælect. § 90..5; it may be inferred that he supposed the chyle was formed in the stomach, and was merely separated in the duodenum from the residual mass. Haller's opinion on this point is not stated with his usual clearness and precision; although, upon the whole, it appears probable, that he supposed the process of chylification is not perfected in the stomach, but he does not make that distinction between the contents of the stomach and the duodenum which have been pointed out by later physiologists. See Prim. Lin. § 635, 8. 717. v. ii. p. 160, 1. 221, 2; also El. Phys. xviii. 4. 24, 31. and xxiv. 2. 1. Juncker very accurately discriminates between chyme and chyle; he says that the aliment is reduced to chyme in the stomach, and is then propelled into the duodenum, where it is converted into chyle, by the mixture of other substances with it; Conspect. Physiol. Tab. 11. De Secretione, and Tab. 25. De Nutritione. Vanhelmont complains that "Usus duodeni neglectus in scholis," and makes the following observation: "Mirum dictu, quod aridus cremor in duodeno, salis saporem confectim acquirat, suumque salem acidum in salem salsum, adeo libenter commutet." Ortus Med. p. 167, 8. On this, as on many other occasions, we shall find the opinions of Baglivi more nearly correct than those of his contemporaries; Diss. 3. Circa Bilem; Op. p. 429, 0. Nor have the moderns been always sufficiently correct on these points. See particularly Hunter on the Anim. Œcon. p. 213. Fordyce, indeed, distinctly states, "that chyle is not formed in the stomach," yet in many parts of his treatise he argues as

if the process of chylification was completed in the stomach. See Bell's Anatomy, v. iv. p. 65 et seq., and Monro's (Tert.) Elem. v. i. p. 552; where the fact is correctly stated, that the chyle is not perfected until the alimentary mass arrives at the duodenum. Richerand observes that the essential part of the process does not take place in the stomach, yet his expression would almost lead us to suppose that he conceived the action of the duodenum was merely to separate the nutritive from the excrementitious part of the aliment; El. Phys. § 14. p. 98. Sir E. Home, in his observations upon the purposes which are served by the different cavities in the whale, speaks unequivocally of chyle being formed in the proper digesting stomach, and appears not to be aware of the distinction between chyme and chyle; Phil. Trans. for 1807. p. 98, 9; the same remark applies to the paper in the second part of the same volume. With respect to this latter paper I may remark, that it contains a very interesting account of the formation of the stomach, which is traced through the various classes of animals, the object of which is to show that, in all cases, the cardiac and the pyloric ends differ in their structure, and consequently in their functions, the former serving, as would appear, more for maceration, the latter for chymification; the whole being illustrated by a series of excellent engravings. Sir E. Home states, that this diversity in the structure of the two parts of the stomach is marked by a visible contraction in the external form of the organ, owing to a transverse muscular band; p. 170 et seq. It is a little singular that he should suppose that it had not been noticed before, as we learn, by a copious list of references in Monro's Elem. v. i. p. 519, 0; that it was described by Cowper, and has since been frequently referred to; to these references we may add Haller; El. Phys. xix. 4, 5; where it is expressed in the clearest manner, as having been seen both by himself and others. We have a description of this sacculated structure of the stomach, as existing in the Semnopitheci, a genus of the Simiæ, by Mr. Owen; Zool. Tr. v. i. p. 65 et seq. Dr. Prout, in his valuable paper, to which I have so frequently referred, very clearly points out the specific operation of the duodenum; Ann. Phil. v. xiii. p. 12 et alibi. Boer. Præl. § 105; Haller, El. Phys. xxiv. 2, 3.

the bile might be conceived to possess more active properties, in consequence of the resinous matter and the soda which it contains, we have no data by which we are able to ascertain what their operation would be upon the chyme. It has been supposed by some physiologists, that the duodenum secretes a specific fluid, which converts chyme into chyle, as the gastric juice converts aliment into chyme; but, as we have no proof of the presence of this fluid, except its supposed utility in the case under consideration, it would be incorrect to assume its existence, without more direct proof. We appear then to be reduced to the conclusion that the change is probably effected either by the intervention of the bile and pancreatic juice, or the action of the constituents of the chyme upon each other; both these causes may operate, although perhaps the former is the more efficient of the two[1].

The chyle, when thus procured from chyme, and separated from the residual mass, is a white opake substance, considerably resembling cream in its aspect and physical properties[2]. When removed from the body it soon begins to concrete, and finally separates into two parts, a dense white coagulum and a transparent colourless fluid, an operation which appears to be very analogous to the spontaneous separation of the blood into the crassamentum and the serum. The chemical properties of the constituents of chyle appear also to be considerably analogous to those of the corresponding parts of the blood, and the chyle is likewise found to resemble the blood in the nature of its salts: but on the other hand, it differs from it essentially in containing a quantity of oil or fatty matter, an ingredient which is only occasionally found in the blood[3]. Hence it appears, that in its chemical properties, as well as in its physiological relations, we may regard the chyle as a kind of intermediate substance between the chyme and the blood.

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[1] Dr. Prout conceives, that when the bile is brought into contact with the contents of the duodenum, the chyle is separated by a kind of precipitation; he was able to imitate the process, to a certain extent, out of the body, by mixing together chyme and bile. It is probable that the alkali, which is a constituent of this latter secretion, may perform some important part in this operation, but the subject still requires further examination. We are indebted to Sir B. Brodie for a series of experiments which he performed on living animals, in order to ascertain the effect of bile upon the process of chylification, which are so simple, that, at first view, they appear decisive in favour of the opinion that the formation of chyle is the immediate result of the mixture of bile with chyme. They consisted merely in applying a ligature round the duct which conveys the bile from the liver into the duodenum, the result of which is stated to have been, that the process of chylification was suspended. But although the experiments must be regarded as highly interesting, I think we should be scarcely justified in giving our unqualified assent to the conclusion which the author draws from them, without a more full detail of the circumstances attending them than we at present possess; Quart. Journ. v. xiv. p. 341 et seq.

[2] Fordyce on Digestion, p. 121 ; Young's Med. Lit. p. 516, fr. Berzelius ; Dumas, Physiol. t. i. p. 379..1 ; Magendie, Physiol. t. ii. p. 154..8. Dr. Hodgkin, in the appendix to his trans. of Edwards, gives us an account of his microscopical observations on chyle ; p. 440, 1.

[3] The experiments of Dr. B. Babington and of M. Lecanu referred to above, p. 494, lead us to conclude, that an oleaginous substance is one of the ordi-

For the chemical analysis of chyle, we are principally indebted to Vauquelin, Marcet, and Prout [1], who, in succession, examined it with much minuteness. Vauquelin employed the chyle of a horse, as obtained both from the thoracic duct, and from one of the principal branches of the lacteals. The chyle from the thoracic duct, when it had spontaneously coagulated, contained a clot which was of a light pink colour, its colour being deeper than that of the serous part, while the clot from the lacteals was almost white. The properties of the liquid part were very nearly similar to those of the serum of the blood; like this it contains uncombined alkali, but it differs from it in containing a considerable quantity of an oily or fatty matter, which is also found, although in smaller quantity, in the coagulum. The coagulum contained a basis of a substance considerably resembling fibrin, so as to indicate that the coagulum of chyle is of an intermediate nature between albumen and perfect fibrin [2]. The principal object of Marcet's experiments was to compare the chyle, as produced by vegetable and animal food in the same kind of animal; for this purpose he procured it from the thoracic duct of dogs. In all the essential points his results agreed with those of Vauquelin. The chyle consisted of a coagulum of a pinkish appearance, containing fibres or filaments, and of a fluid part very similar to the serum of blood, except that, in the animal chyle, there was the oily or fatty matter, which floated on its surface like cream. The vegetable chyle generally bore less resemblance to blood than that derived from animal food; the latter was more disposed to become putrid, and upon the addition of potash, it evolved a quantity of ammonia, which was not the case with the vegetable chyle, while the oily matter was found in the animal chyle alone. The two species were of the same specific gravity, and contained the same weight of saline matter, but the solid residuum of the animal chyle, as obtained by evaporation, was considerably greater than from the vegetable chyle. When they were both submitted to destructive distillation, the vegetable chyle produced three times as much carbon as the animal chyle, whence we may conclude that the latter contains a much greater proportion of hydrogen and nitrogen [3]. It is not improbable that in this case, the vegetable chyle was less completed or assimilated, in consequence of the animal being fed upon a diet, which was not

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nary constituents of the serum; but it exists in much greater quantity and in a more obvious state in chyle.

[1] Emmert made some experiments on chyle, which he procured from the lacteals soon after it enters these vessels, but we do not obtain much precise information from them; Ann. Chim. t. lxxx. p. 81 et seq.

[2] Ann. Chim. t. lxxxi. p. 113 et seq.; Ann. Phil. v. ii. p. 220 et seq.

[3] Med. Chir. Tr. v. vi. p. 618 et seq.

natural to its digestive organs; for we observe that the chyle of the horse, as examined by Vauquelin, which must have been derived from vegetable food, was in a more animalized state than the vegetable chyle in Marcet's experiments [1]. Dr. Prout's results, for the most part, agree with those of Vauquelin and Marcet. The chyle was found to consist of a coagulum and a fluid part, which bore a general resemblance to the corresponding ingredients of the blood. In addition to these there was the oily or fatty matter, which appears, however, to have been in less quantity than in the animal chyle which was examined by Marcet. Dr Prout likewise compared the chyle as produced from vegetable and from animal food, and found the former to contain more water and less albuminous matter, while the fibrous part and the salts were nearly the same in both; they are both said to have exhibited a trace of the oily matter; upon the whole he found less difference between the two kinds of chyle than had been noticed by Marcet. Dr. Prout has given us an interesting account of the successive changes which the chyle experiences in its passage along the vessels, having examined it when it first enters the lacteals, when it has arrived nearly at their termination, and when it is finally deposited in the thoracic duct. Its resemblance to the blood, as might be expected, was found to be increased in each of these successive stages of its progress [2].

The chyle, as it is formed or separated, is taken up by the lacteals, a set of vessels, the appropriate office of which is to convey this substance from the duodenum to the thoracic duct. Upon examining the contents of the different parts of the alimentary canal, we observe that the chyle first makes its appearance soon after the chyme leaves the pylorus, that the greatest quantity of it appears to be formed at a short distance from this part, more especially, as it is said, near the orifice of the biliary duct, and that it gradually occurs in less and less quantity, as we pass along the small intestines, until it is no longer to be met with, and that, except in certain morbid states, where the contents are propelled with undue rapidity, no chyle is ever found beyond the small intestines.

The obvious and essential use of the large intestines is to carry off from the system the refuse matter, after the separation of the chyle from it; we may, however, suspect that in this, as in all other analogous instances, some secondary purpose of utility is served by them. This opinion is farther rendered probable by their anatomical structure, for besides their length, which although less than that of the small intestines, is still considerable, there is evidently a provision in them for retaining their contents, and preventing them from passing too rapidly through them. It is moreover observed, that there is an obvious change in the physical properties of the contents of the intestines from the time when they enter the cœcum until they arrive at the rectum. Although they no longer contain chyle, and are therefore not furnished with lacteals, they have a number of lymphatic vessels connected with them, which absorb the more fluid parts of the fæces, and thus extract from them what may ultimately contribute to nutrition. That this is the case is rendered probable by the effect of nutritive matter injected into the rectum, which, in cases of mechanical obstructions of the œsophagus, when food cannot be received into the stomach, has supported life for a certain length of time, proving the capacity of the organs to extract any portion of nutriment which may be mixed with their contents. Probably, however, the most important object to be gained by the structure of the large intestines, is to retain, for a certain length of time, the fæcal matter, which is gradually transmitted to them by the upper part of the canal, and to allow it to be evacuated at certain intervals only; a temporary detention of the contents being thus rendered necessary, advantage was taken of this circumstance to produce other beneficial effects in the system [1].

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[1] In some late experiments, performed by MM. Macaire and F. Marcet, on the two species of chyle, the proportion of nitrogen in each was found to be nearly similar; Ann. Chim. et Phys. t. li. p. 371.

[2] Ann. Phil. v. xiii. p. 22..5; see also Magendie, Phys. t. ii. p. 154..8. Magendie observes, that the opake white matter, which is observed in the serous part of chyle, is more abundant when the animal has used any considerable proportion of fat or oil in its diet; Ibid. p. 157.

There are two of the abdominal viscera, which, from their connexion with the stomach, have been generally supposed to be subservient to the process of digestion; the pancreas and the spleen. The former of these, both from its intimate structure, and from the nature of the secretion which it furnishes, appears to be very similar to the salivary glands, and its office has accordingly been supposed to be that of providing a quantity of fluid resembling the saliva, which may contribute to the completion of the process of chylification [2].

The structure and function of the spleen is more obscure, and they have given rise to many hypotheses and conjectures which appear to be altogether unfounded, wholly unsupported either by any well ascertained facts, or by the analogies of the animal œconomy[1]. We are indebted to Sir E. Home, for what he conceives to be a more consistent account of the nature and use of this organ. He supposes that the spleen serves as a reservoir or receptacle for any fluid that is received into the stomach, more than what is sufficient for the purposes of digestion; that this excess of fluid is not carried off by the intestines, but is transmitted directly to the spleen by the communicating vessels, and is lodged there until it is gradually removed, partly by the veins and partly by the absorbents. He illustrated his opinion by numerous experiments upon living animals, in which coloured infusions were injected into the stomach, and were afterwards discovered in the spleen, while it appeared that they had not passed through the absorbents of the stomach[2].

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[1] Sœmmering. Corp. Hum. Fab. t. vi. p. 334..8. § 241. In the paper of Dr. Prout's, to which I have referred above, we have a series of very interesting observations on the successive changes which the alimentary mass experiences in its progress along the intestinal canal, both in different animals, and in the same kind of animal, when fed upon different kinds of food. It appears, as a general principle, that the process of digestion is more complete when animal food is employed, but we find that in most cases, the fluids of the intestines continue to coagulate milk, even as low down as the rectum; Ann. Phil. v. xiii. p. 15..22. I have already offered some observations upon the hypothesis of Sir Ev. Home, in p. 495. Dr. O'Bearne has lately published an essay on the process of Defecation, to which I shall refer my readers, as containing some new and apparently correct views on this subject. We are indebted to Berzelius for an analysis of the fæces, which appears considerably more minute than any that had been previously made; Chimie, par Esslinger, t. vii.

[2] See Santorini's fig. 1, in tab. 13; also references in p. 489.

Sir E. Home has more recently investigated the structure of the spleen, and is led to conclude, that it consists entirely of a congeries of blood-vessels and absorbents; he conceives that there is no cellular membrane interposed between them, but that there are interstices which are filled with blood that exudes through certain lateral orifices in the veins, which are rendered pervious when these vessels are much distended[3]. Notwithstanding the importance which we must attach to these observations, we may remark concerning them, that as the investigation of the intimate structure of the spleen appears to have been effected merely by successive macerations, it may be questioned how far this operation was the best adapted for elucidating the natural state of the organ. The conclusion which is drawn respecting the function of the spleen is nearly similar to the one referred to above, although, perhaps, rather more vaguely expressed. It is said that, "the spleen, from this mechanism, appears to be a reservoir for the superabundant serum, lymph, globules, soluble mucus, and colouring matter, carried into the circulation immediately after the process of digestion is completed."

We have a still later account of the structure and functions of the spleen by the Professors Tiedemann and Gmelin, derived from a series of experiments which they performed on this organ, and which tend still farther to elucidate this intricate subject. With respect to the former of these points, the authors conceive, that the structure of the spleen essentially resembles that of the lymphatic glands, and that it is in fact to be regarded as an appendage to the absorbent system. Its specific function is to secrete from the blood a fluid, which is of a reddish colour, and possesses the property of coagulating, and which is carried to the thoracic duct, and being there united with the chyle, converts it into blood. Their opinion is founded upon the appearance of this peculiar fluid, which has been seen by other physiologists, as well as themselves, and which has led to an opinion, that has been pretty generally adopted, that the spleen essentially contributes, in some way or other, to the process of sanguification. They also adduce in favour of their doctrine the relation which the vessels of the spleen bear to each other. The artery is unusually large, so as to render it probable, that it must serve some purpose besides the mere nutrition of the part, while we have, at the same time, an extraordinary number of lymphatic vessels. Hence it is inferred, that the great quantity of arterial blood which is sent to the spleen must have some immediate connexion with the numerous lymphatics that pass off from it; and they accordingly found, agreeably to the observations of preceding anatomists, that injections of various kinds are readily transmitted from the branches of the splenic artery to the lymphatic vessels, showing an actual communication between the arterial and absorbent systems[1]. There is undoubtedly much valuable information contained in the researches of the Professors Tiedemann and Gmelin, and it is impossible not to admit that their opinions evince an accurate acquaintance with the operations of the animal œconomy. But their hypothesis appears to me to be obnoxious to one fatal objection, that animals have been known to live for an indefinite length of time after the removal of the spleen, without any obvious injury to any of their functions[2], which could not have been the case, if the spleen had been essentially necessary for so important an operation as that of chylification. It would seem from this circumstance, that the office of the spleen must be something of a supplementary or vicarious nature only, which, although occasionally useful, is not at all times essentially necessary to our existence[3].

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[1] See Haller, El. Phys. lib. xxi; also Sœmmering, t. vi. p. 149 et seq., where the various uses that have been assigned to the spleen by physiologists are enumerated; the author does not offer any opinion of his own upon the subject.

[2] Phil. Trans. for 1808, p. 45 et seq. and p. 133 et seq.

[3] Phil. Trans. for 1821, p. 35..42. pl. 3..8.

It has been made a subject of inquiry, whether by the conversion of aliment into chyle it is rendered soluble in water, because it has been supposed that no substance could enter the lacteals unless in a state of complete solution. Arguing from the chemical nature of chyle, as examined out of the body, we must conclude that it is not absolutely soluble, although reduced to that state of minute division, which renders it easily diffusible through water, and capable of forming with it a uniform emulsion. It is not, however, improbable, that the chyle may resemble blood in its relation to water, and that it may be soluble in this fluid while in the vessels, although only partially so when removed from them.

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[1] Ed. Med. Journ. v. xviii. p. 285 et seq. I must acknowledge my obligations to the editors of this work, for the valuable analyses which it frequently contains of the labours of the German physiologists.

[2] Baillie remarks that the spleen is occasionally wanting, and has been removed without apparent injury; Morb. Anat. p. 260, 1; Works by Wardrop, v. ii. p. 235. I consider it quite superfluous to adduce any additional proofs of a fact which is sanctioned by so high an authority.

[3] For further information on this subject, see Elliotson's Physiol. p. 108 et seq.; also an essay by Dr. Hodgkin, appended to his Trans. of Edwards, p. 448 et seq.

Another subject of inquiry has been, whether any part of the food which is received into the stomach, is taken up by the absorbent vessels unchanged, without having undergone decomposition, and entered into new combinations. To this I feel disposed to reply in the negative. It is obvious that it must be the case with vegetable food of all descriptions; and with respect to the animal food employed in diet, the specific properties of all the substances appear to be so entirely altered, as to indicate that none of them had escaped the action of the gastric juice. And indeed, were any thing to pass into the duodenum unchanged, we might conclude, that the power which the lacteals appear to possess of absorbing those substances only which are perfectly assimilated, would prevent them from taking up whatever had escaped previous decomposition.

But although we conceive that every thing which can be considered as properly alimentary, must be completely decomposed, and enter into new chemical relations before it can be converted into chyle, it appears that there are certain substances that are mixed with the food, or make a part of it, which pass into the system without experiencing any change; or even if they be in some respects changed, they are not assimilated with the chyle. This, we may presume, is the case with the saline substances that are taken into the stomach; and it appears that there are certain bodies which give the specific odours and flavours to various articles, both of animal and vegetable origin, which are taken up by the lacteals, while they still retain their sensible properties, and impart them to the solids and fluids of the animal. It is well known that the specific qualities of various medicines are communicated to the milk, so that it will affect the child in the same manner as if the substances had been directly taken into the stomach; and every one is aware that the flavour of the milk, and of the flesh of animals generally, is materially influenced by the nature of their food. The sensible properties of the plants on which bees feed are imparted to their honey; and we are informed by naturalists that it sometimes becomes poisonous, when procured from noxious vegetables, and the same is said to have been the case with the flesh of birds, in consequence of their feeding upon berries or seeds, which, although salutary to them, are injurious to the human constitution. Besides the substances which pass into the circulation unchanged, there are certain bodies, which although not completely assimilated with the chyle, yet seem to be partially decomposed, so as to acquire new sensible properties which they impart to the solids or fluids of the animal. Turpentine presents us with a remarkable example of this kind, which, when taken into the stomach, imparts to the urine an odour exactly resembling that of violets[1].

In the last chapter I offered some remarks upon the various salts which are found in the blood, and I discussed the question whether we are able to account for the quantity which exists in the different solids and fluids, by supposing them to be introduced along with the food. The facts of which we are in possession would lead us to the conclusion, that the quantity of these earths or salts that are introduced into the stomach ab extra, is not sufficient to supply the demands of the system, but that they must, in some way or other, be generated by the vital powers. Our next inquiry will be, what powers, or in what part of the system, are they produced; they may either be formed by the digestive organs, during the process of chymification or chylification, or by a process more analogous to secretion, by an organ or organs expressly appropriated to the purpose. The difficulties which exist upon both these suppositions are very great; we can indeed form no conception of the nature of the operation in either case, and it is only in consequence of the necessity that there is of attempting some explanation, that we have recourse to them. The subject is again introduced into this place for the purpose of inquiring whether there be any grounds of preference between these two opinions, and we may go so far as to remark, that if we find in the chyle all the salts that exist in any of the constituents of the body, we must conclude that they are produced in the process of digestion, and afterwards merely separated from the blood; a conclusion to which the results of our experiments would seem to lead us. The whole subject is, however, one that is so extremely obscure, and of such difficult solution, that I do not think it desirable to enter more particularly into the consideration of it, until we are in possession of a more firm basis of facts, on which to build our hypotheses.

SECT. 4. *Theory of Digestion.*

There are few subjects in physiology that have afforded a more fruitful subject for speculation than the theory of digestion, for in this, as in other parts of the science, we may remark, that the more obscure is the subject, and the less real information we possess concerning it, the more numerous have been the attempts to frame hypotheses to account for it. The opinion proposed by Hippocrates, and adopted by Galen and the ancients generally was, that the aliment is digested by what is termed concoction. But this is to be considered rather as another word expressive of the action, than as any explanation of it. It is, indeed, synonymous with the term digestion, and derived from the same analogy of the change which substances undergo, when they are exposed to a certain degree of temperature in close vessels[1].

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[1] Fordyce states that indigo and musk are both taken up by the lacteals, so far retaining their previous properties, as that the former possesses its specific colour, and the latter its peculiar odour; On Digest. p. 122. This power is, however, limited to certain substances, while others, although equally exposed to the action of the lacteals, are incapable of entering them, these vessels thus exercising what appears to be a kind of selection; p. 123; but upon this subject I shall have occasion to offer some further observation in the next chapter.

The next hypothesis was that of putrefaction, an hypothesis which was maintained by some of the earlier chemists, and was supported by various observations, and even experiments, that were supposed to be favourable to it. The food, when it is received into the stomach, was observed to have its texture broken down, and to have acquired an unpleasant odour, which the older physiologists, according to the loose mode of reasoning which they employed, regarded as a species of putrefaction. It is a sufficient refutation of this hypothesis to remark, that digestion and putrefaction are processes of a totally different nature; and that so far from their having any connexion with each other, one of the first effects of the gastric juice is to resist putrefaction, or even to suspend it, if it has actually commenced[2].

The mechanical physiologists endeavoured to account for all the phenomena of digestion by trituration, and they performed many curious experiments in proof of their opinion on those animals which possess what are termed muscular stomachs. But although the facts were correctly stated, the conclusions which were deduced from them were inaccurate in two respects. In the first place, they extended to all classes of animals an action which belongs to certain species only, and secondly, in considering the trituration of the muscular stomach as analogous to the process of chymification in membranous stomachs. I have already stated, that the operation of the gizzard is entirely mechanical, and is equivalent to the teeth of quadrupeds; the food is then brought into the same state as it is by mastication, and has still to undergo the action of the proper digesting stomach. If direct facts were wanting to confirm this opinion, they are abundantly furnished by the experiments of Stevens and Spallanzani, as they prove that chymification is effected under circumstances in which trituration could not possibly operate[1].

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[1] See Boerhaave, Prælect. not. ad § 86 t. i. p. 158, 160; Blumenbach, Inst. Phys. § 360. p. 202. By the following passage in Celsus, it appears that the hypothesis of attrition and of putrefaction, had also their defendants among the ancients; "Duce, alii, Erasistrato, atteri cibum in ventre contendunt; alii, Plistonico Praxagoræ discipulo, putrescere: alii credunt Hippocrati, per calorem cibus concoqui." Præf. p. 6. § 10. The student who is disposed to make himself acquainted with the doctrines of the earlier physiologists on the subject of concoction, a process which was supposed to be concerned in other functions besides that of digestion, may consult the treatise of Fernel, Physiol. lib. 6. c. 6. "De Concoctionibus." Fernel was one of the first of the moderns who wrote from his own observations, and who exercised his own judgment on subjects of physiology and pathology.

[2] This hypothesis has had its advocates even in modern times; Cheselden, Anat. p. 155, says "digestion is no other than corruption or putrefaction of our food." It would appear to have been invented by Plistonicus, of whom nothing more is known than that he was the author of this hypothesis; see Celsus, ut supra, and Le Clerc, Hist. de la Méd. part 2. liv. 1. c. 8. p. 326, 7. The hypothesis of Pringle and M'Bride, although nominally founded upon fermentation, ought really to be referred to putrefaction. M'Bride and most of his contemporaries thought that the saliva was an active promoter of this decomposition, Essays, p. 16, 7; whereas Pringle's experiments, Appendix, p. 362, led him to conclude that saliva resists this process. With respect to the gastric juice, the fact appears to be that it is decidedly antiseptic; Stevens, c. 9; Spallanzani, Exper. § 249..259.

In opposition to the mechanical doctrine of trituration, an opinion was advanced by the earlier chemists, that the action of the stomach consisted in a species of fermentation. This hypothesis appears to have been originally suggested by Vanhelmont[2], and was, at one period, embraced by the most celebrated physiologists of the age[3]. In order to estimate the value of their hypothesis we must bear in mind, that they employed the term fermentation to express any change which a body experiences in its mechanical or chemical properties, either by the action of its constituents upon each other, or by the addition of a foreign substance, in consequence of which the elements of the body were made to enter into new combinations. Various species of fermentations were supposed to exist, and a number of the most important changes, especially of those that occur among organized bodies, were supposed to be produced by this kind of operation. The correctness of this hypothesis will depend very much upon the exact sense in which the term is employed, for it must be admitted that, according to the mode in which it was used by the older writers, the change produced upon the aliment by the stomach appears to fulfil all the conditions that were supposed to be requisite to fermentation.

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[1] Physiological speculation was, perhaps, never carried to a greater excess than by Pitcairn, in the estimate which he makes of the mechanical force which the stomach exercises in digestion. After employing much learned and abstruse discussion to prove that no other power is competent to produce the requisite effect upon the aliment, he calculates that the power of the muscular fibres of the stomach is equal to 12,951 lbs.; Dissert. p. 72..95; also Elem. c. 5. p. 25..7; see the observations of Cheselden, Anat. p. 152..5; also of Hales, who estimates "that 20 lbs. would come nearer to the pressure of the aliments of a full stomach;" Statical Essays, v. ii. p. 174, 5. Haller very explicitly states the impossibility of trituration being effected by a membranous stomach; El. Phys. xix. 5. 1; yet he scarcely draws a sufficiently accurate line of distinction between the mechanical and chemical action of this organ. It is amusing to observe the learned and laboured arguments which Fordyce thought it necessary to adduce in order to prove that minute mechanical division alone cannot alter the chemical nature of a substance; On Digest. p. 124..138. See remarks on trituration by Stevens, de Alim. Concoct. cap. 10; also by Richerand, El. Phys. § 18. p. 100..2.

[2] The account which this singular writer gives of the action of the stomach upon the aliment, is contained in his treatise entitled, "Sextuplex Digestio Alimenti Humani." According to his doctrine, all the changes which the components of the body experience, in the different abdominal viscera, are to be ascribed to a series of fermentations: of these there are supposed to be six, the first being the conversion of aliment by the stomach, "in cremorem plane diaphonum in cavo stomachi." He goes on to observe, "Addo, id fieri vi fermenti primi, manifesti a liene mutuati." Vanhelmont, although a man of considerable acuteness and information, partook largely of the arrogance which is so characteristic of his sect. The first paragraph of the above treatise is entitled, "Misera Galenicorum jactura," (a censure which would apply with far more justice to the chemists,) and he begins by accusing Galen himself of having described parts which he never saw, and of writing without any knowledge, or any attempt to investigate the truth; Ortus Med. p. 167.

[3] Among the physiologists of eminence, during the last and preceding centuries, who have attributed the digestive process to fermentation, I may mention the names of Sylvius, Dissert. Med 1. et Prax. Med. lib. 7. C. 7; Willis, De Ferment. C. 1. p. 17; Boyle, Works, v. ii. p. 622; Grew, Comp. Anat. &c. p. 26; Charleton, Œcon. Anim. Exerc. 2. de Chylif; and Lower, De Cord. p. 204. Boerhaave's opinion was, that a commencement only of fermentation is excited by the gastric juice, Prælect. t. i. § 78, 87; and the same

The hypothesis of chemical solution, which is considerably analogous to that of fermentation, was derived from the experiments which have been so frequently referred to, on the effect of the gastric juice upon the aliment taken into the stomach, which was supposed to be similar to that of a chemical solvent[1]. In describing the successive changes which the food undergoes after it is received into the stomach, I have had occasion to remark upon the facts that have been adduced in favour of this doctrine, and upon the objections that have been urged against it. We appear to have sufficient evidence to prove that the stomach secretes a peculiar fluid, which acts chemically upon the aliment, and that nothing farther is necessary to produce this action than to bring the substances into contact[2]. That this is a case of mere chemical action, is espe-

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appears to be the case with Haller, Prim. Lin. Sect. 24. and El. Phys. xix. 5. 2. Parr inclines to the same doctrine; Dict. "Digestion." The objections to the doctrine, according to the opinion that was entertained at the time respecting the nature of fermentation, are well stated by Stevens, De Alim. Concoct. C. 11. Spallanzani devotes his sixth dissertation to the inquiry, whether fermentation is a necessary part of the process of digestion, which he decides in the negative; he, however, conceives that the absence of any gaseous product is a sufficient proof of the non-existence of fermentation, as Pringle and M'Bride thought that its presence was a sufficient indication of that process.

[1] It may not be uninteresting to observe the manner in which Grew's opinion on this subject corresponds with the modern doctrines; he published his lectures in 1681. "By the joint assistance of the glandulous and the nervous membranes, the business of chylification seems to be performed. The mucous excrement of the blood being supplied by the former, as an animal corrosive preparing; and the excrement of the nerves by the latter, as an animal ferment, perfecting the work;" Ubi suprà, p. 26.

[2] Reaumur, in two papers in Mém. Acad. pour 1752, p. 266 et seq. and p. 461 et seq., contrasts the process of digestion in birds with muscular and with membranous stomachs, in the first of which the great agent is supposed to be of a mechanical, and in the second of a chemical nature. He had considerable merit in pointing out this distinction, but he was not fully aware of the difference of mere trituration and of proper digestion. Reaumur's experiments were repeated and much extended by Spallanzani. See

cially proved by the experiments of Stevens[1], and still more of Spallanzani, where he produced a similar change out of the body, by gastric juice procured from the stomach[2], and also by the action of rennet upon milk. To a certain extent these experiments may be considered as establishing the fact; yet the hypothesis is still encumbered with serious difficulties. Not to insist upon the objection, that it only explains one part of the process, the formation of chyme, it must be confessed, as I have remarked above, that it is not easy to conceive how all the various articles that are taken into the stomach can be converted into a substance of the same, or nearly the same consistence, and this by an agent apparently so little active as the gastric juice.

In consequence of these objections, and of the difficulty which there seemed to be in accounting for digestion, either upon mechanical or chemical principles, many of the most eminent among the modern physiologists have ascribed it to the direct agency of the vital principle. It is said that the interior surface of the stomach is endowed with a specific property, unlike any other that exists in nature, which belongs to it as a living substance, and which enables it to digest the food. In proof of this position the curious fact is adduced, which was observed by Hunter, that in some cases of sudden death, the stomach itself is partially digested by the gastric juice which had been previously secreted[3]. A fact of a similar kind is stated

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also Blumenbach, Inst. Physiol. § 358, 9; Monro (Tert.) Elem. v. i. p. 532. Tiedemann and Gmelin, as the result of their elaborate researches, conclude, that the digestion consists in the solution of the aliment by the gastric juice, and that by this agent, all kinds of food, both those that consist of single proximate principles, and those that are more compounded, are dissolved. Water alone, they observe, at the temperature of the mammalia, is capable of dissolving many of the articles employed in diet, and a considerable number, which are not soluble in water, are so in the acids which are found in the stomach at the same temperature, and to this indeed they ascribe a considerable part of the effect produced; Richer. t. i. p. 363..7. But I may remark on this opinion, that a simple solution of the alimentary mass, either in water, or in the acids that are supposed to exist in the gastric juice, would differ very much from chyme.

[1] De Alim. Concoct. § 24, 5.

[2] Expériences, Dissert. 2. § 85 et seq. Dissert. 4. § 149, 0. 186. Dissert. 5. § 216 et alibi.

[3] Hunter's original observations are contained in his paper in the Phil. Trans. for 1772, p. 447 et seq.; they were afterwards given in an enlarged form in his Observ. on the Anim. Œcon. p. 226..1; see Baillie's Morb. Anat. Ch. 7. p. 148, 9; Works by Wardrop, v. ii. p. 136, 7. Eng. to Morb. Anat. fasc. 3. pl. 7. fig. 2. In the first vol. of the Trans. of the Edinburgh Med. and Chir. Soc. p. 311 et seq. we have a valuable paper by Dr. Gardner, on erosions and perforations of the alimentary canal, in which the author gives us an account of some cases that had fallen under his own observation, as well as a collection of the observations of others. We have also a case by Dr. Haviland, in the Cambridge Phil. Trans. v. i. p. 287 et seq. In Beck's Med. Juris. by Dunlop, p. 376..380, we have many references and good remarks. A very full account is given us by Dr. Carswell, in the 5th

with regard to certain species of vermes, that are occasionally found in the stomachs of animals, and which, as long as they remain alive, are not acted upon by the gastric juice, although after death it affects them as it does any other organized substance.

These facts are very curious and important, and they clearly prove that there is a difference in the mechanical and chemical relations of living and dead matter, a difference which, I fully admit, we are not able to explain or account for. It is the same kind of difficulty which occurs with regard to the contractile and sensitive functions of the muscles and the nerves, that they are both of them totally destroyed by the extinction of life, although for some time afterwards, neither of these organs seem to have undergone any alteration, either in their chemical or physical properties. In this, as in the other analogous cases, the doctrine of the animists proceeds upon the principle, that no modification of the laws of chemistry or mechanics can account for the phenomena, and that it is consequently necessary to assume the existence of some new agent to meet the emergency. But I may remark, as I have done on former occasions, that by this proceeding we throw no new light upon the difficulty, and that in reality we are only employing a different expression to announce the fact, and one which is less simple and intelligible. With respect to the particular case under consideration, our knowledge of the process is, in many respects, extremely incomplete; we have only an imperfect acquaintance with the successive steps of the operation, and we are, in a great measure, ignorant of the nature of the agents by which it is effected; nothing therefore can be more incorrect than thus prematurely to attempt to establish general principles before we are thoroughly acquainted with the facts on which they profess to be founded.

This is essentially the hypothesis which is maintained by Fordyce in his elaborate treatise. He remarks that aliment, when it is converted into chyle, undergoes a complete change, in its elementary constitution, and he admits that the gastric juice is a chemical agent, but he contends that the nature of the action is totally unlike what takes place in any other chemical process, and that it is therefore necessarily connected with the vitality of the stomach. The anomaly on which he particularly insists is, that by adding the same agent, the gastric juice, to various kinds of aliments, we have always the same product, as he maintains that the chyle of carnivorous is perfectly similar to that of herbivorous animals. But in order to prove the point which he wishes to establish he ought to show not only that the chyle, but that the fæces also, are similar. He further conceives that it is inconsistent with the principles

number of his Pathol. Anat., of the appearances which the parts assume in these cases, accompanied by beautifully executed and admirably expressive engravings; see also Archives Gén. de Méd. Fev. 1830, and Amer. Journ. Med. Sc. v. vii. p. 227..9.

of chemical action to suppose, that a single menstruum can form with a single principle, as, for example, farina, the three bodies which constitute chyle. He likewise asserts that chyle cannot be formed out of the body, and that if aliment be placed in a dead stomach it will not undergo the same changes as in a living stomach, although they are both kept at the same temperature[1].

Somewhat allied to the hypothesis of the animists, although much less vague and indeterminate, is the doctrine which has been lately advanced, that digestion is essentially a nervous function, or one that depends upon the immediate and direct agency of the nervous system. A number of well known occurrences, not only in pathology, but in the ordinary actions of the animal œconomy, prove to us that the powers of the stomach are intimately connected with the nervous system[2]. And indeed we might conclude that this would be the case, by an inspection of the anatomical structure of this organ, as it cannot be doubted that some useful purpose must be served by the variety and number of the nerves with which the stomach is provided. But there are many purposes to which the nerves may be subservient, besides the digestion of the food, while, on the other hand, it may be asserted, that we can form no idea of the mode in which the mere chemical action of two bodies can be affected by the nervous influence. The experiments, of which I have already given an account, in which the digestion was suspended by dividing the par vagum, although by some eminent physiologists they have been thought decisive in favour of the nervous hypothesis, yet even if we admit them in their fullest extent, they go no farther than to prove the agency of the nerves in the preparation of the gastric juice, they therefore refer entirely to the question of secretion, and as such have been already considered in the last chapter.

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[1] On Digestion, p. 139..146, 171.

[2] It is unnecessary to adduce particular examples of the connexion between the state of the nervous system generally, and the action of the digestive organs, as they must be sufficiently obvious to every one, from the result of his own experience. This, however, as I have had occasion to remark on various occasions, proves no more than that the nerves are the media by which the different parts of the system are connected together. Grief, and the other depressing passions, act upon the circulating system, and perhaps also from other causes, the secretions are diminished, and that of the gastric juice among the rest. At the same time the nervous system becomes less sensitive, therefore the feeling of hunger is less felt, and consequently we are less disposed to take the proper quantity of nutriment. Hartley supposes that "the stomach is particularly affected in grief;" On Man, v. i. p. 189; an opinion which receives some countenance from the peculiarity in the nerves of this organ. See also the remarks of Sœmmering, in § 179, "consensus ventriculi, cum aliis patribus in genere;" he extends these observations to the various organs; § 180..4. Vanhelmont's well known opinion that the stomach is the immediate seat of the soul, or the centre to which all the affections of the nervous system are referred, although in itself so palpably absurd, is enforced by a number of ingenious and just observations on the extensive influence which this organ exercises over the whole system; Ortus Med. p. 448..0.

There is indeed one point of view in which we may regard these experiments as bearing upon the subject of digestion. It may be conceived that the presence or absence of the nervous influence is the cause of the difference which we observe between living and dead matter; that perhaps the electricity, which has been supposed to be identical with the nervous influence, may operate so as to prevent the chemical change, which, under ordinary circumstances, leads to the decomposition of organized bodies, and may likewise prevent it from being acted upon by the gastric juice. It is impossible to say that some effect of this kind may not take place; but as we have no evidence of its existence, it would be premature to assume it as the basis of our hypothesis.

From this review of the various speculations that have been offered to account for digestion, there appear to be two, which are, to a certain extent, countenanced by the phenomena, and which show some analogy between the other operations of nature and the effects which are produced by the stomach. These are the hypotheses of fermentation and of chemical solution; it remains therefore for us to consider which of these presents us with the nearest resemblance and the closest analogy to the operations of the stomach. It is unnecessary to premise that both of these are to be referred equally to the laws of chemical affinity, and that the point to be determined therefore will be, whether the process of digestion is merely to be referred to chemical action generally, or whether it can be approximated to the particular case of fermentation.

The essential difference between the two cases may be conceived to be, that whereas in what may be strictly termed chemical solution, we have two bodies that act upon each other, and produce a third substance, exhibiting new properties; in fermentation this change is effected by the action of the elementary parts of a body upon each other, either without any addition ab extra, or by adding a very minute quantity of an agent, which serves merely to establish the commencement of the operation, and is afterwards no longer necessary to its continuance[1]. The species

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The term fermentation, or rather the corresponding Greek word, *Ζυμωσις*, seems to have been originally employed to express the spontaneous change which takes place in bodies, attended with an enlargement or tumefaction of their substance, in consequence of the extrication of some volatile matter or vapour. See Castelli, " Fermentatio." Vanhelmont is said to have been the first among the moderns who used the word fermentation, applying it to the change which dough experiences in forming wheaten bread. See Stahl, Fund. Chym. Dog. Rat. et Exper., Pars 2. Sect. 4. § 2. At one time it was employed in a vague and general way to designate almost every chemical change to which a compound body is liable; see page 513; but it has been of late restricted nearly in the manner stated in the text. Still, however, the most correct among the modern chemists are not entirely agreed as to the nature of the processes which ought to be classed among the fermentative, and they consequently differ in the number of the species of fermentations which are supposed to exist. Stahl appears to have contributed to produce a more correct idea of the operation, by confining it to a spontaneous change among the conof fermentation with which we are the most familiar is the vinous, by which a solution of mucilage and sugar is converted into carbonic acid and alcohol. These substances are produced by a mutual interchange which takes place between the elements of the substances contained in the solution, but the effect is considerably promoted by the addition of a portion of yeast, which is the result of a previous fermentation, and which establishes the commencement of the process, and causes it to proceed with more rapidity. To which then of these cases is the action of the stomach more analogous, to simple solution, or to fermentation? Does the gastric juice act more like a solvent or a ferment? I have already stated the objections which have been urged against the idea of its acting as a solvent; and it has been objected to the hypothesis of fermentation, that the products of the action of the stomach do not resemble those of the vinous fermentation, or of any other with which we are acquainted. But this is no objection to the hypothesis itself, as we have no reason to conclude that there may not be other operations, that are strictly entitled to the name of fermentation, besides those that are generally recognised by chemists. Besides the vinous and the acetous, we have the panary fermentation, that takes place in dough, which seems certainly entitled to the appellation, and there is no reason to conclude that others may not exist[1]. There are

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stituents of a body, or one which was brought about without the addition of any foreign ingredient. Ubi supra, § 3; also Fund. Chym. Dog. et Exp. Art. 3. Cap. 2. § 2. He supposes that it may exist not only in vegetable and animal, but also in mineral substances. The later chemists have, however, restricted it entirely to vegetable and animal bodies, and some of them, as Thomson, Chem. v. iv. p. 370; Murray, Chem. v. iv. p. 391; and Brande, Man. v. iii. p. 128. § 1855, confine it to the former. The number of fermentations generally supposed to exist are three, the vinous, the acetous, and the putrefactive; Mr. Brande, however, limits the number to two, the vinous and the acetous, correctly, as I think, rejecting the putrefactive, this being rather a complete decomposition of the body, than a conversion of it into new definite products. Thenard, Traité, t. iii. p. 471, enumerates four, adding the saccharine to the three former, and some chemists have also admitted the panary, or the fermentation of dough; see Aikin's Dict. "Bread." I conceive that there is a foundation for both the saccharine and the panary. Thenard supposes that the panary is merely a compound of the vinous and the acetous, depending upon the presence of farina and sugar in the flour. Vogel has, however, shown, that after all the sugar is carefully removed wheaten flour is still capable of fermenting; Journ. Pharm. t. iii. p. 214; and Vauquelin remarks that there is no sugar in the potatoe; ibid. p. 320. Dumas enumerates no less than six fermentations, the spirituous, the acid, the putrid, the muriatic or saline, the saccharine, the panary, and the "colorante," which developes the principles of colours; Physiol. t. i. p. 306, 7.

[1] Sir E. Home informs us, on the authority of Sir H. Davy and Mr. Brande, that an inflammable gas is extricated in the third stomach of ruminant animals; Phil. Trans. for 1807, p. 163; and there is reason to believe, that the gas which is evolved during digestion, generally contains more or less of hydrogen. Sir E. Home observes, that this circumstance establishes a difference between digestion and fermentation; it, however, only shows that the fermentation of the stomach differs from that of alcohol or acetic acid. See the analyses of the gases in the different parts of the human alimentary canal by Jurine, Chevreul, and Chevillot; p. 572.

several circumstances in which I conceive that digestion bears an analogy to fermentation, and in which it appears rather to resemble this operation than what may, with greater strictness, be styled chemical solution. 1. The substances possessed of various properties, and composed of elements combined in different proportions, are all reduced to an homogeneous mass, by the addition of a minute quantity of an extraneous body. 2. The quantity of the ferment, the substance which is the immediate agent in this process, is often extremely minute in proportion to the effect which it ultimately produces. 3. The subjects of fermentation are the products of organization alone. 4. The process is frequently deranged or suspended by apparently very slight causes, and such as would have previously appeared altogether inadequate to the effect. 5. When the process is completed, if the substances are kept in the same situation as at first, a new operation sometimes commences, inducing a new fermentation, which terminates in the production of a new substance, possessed of peculiar and specific properties different from the one originally produced. The analogy might perhaps be extended; but I conceive that enough has been stated to prove that it exists, and to shew that in some remarkable and even essential particulars, the process of digestion resembles that of fermentation [1].

I must, however, remark, that even if we were able fully to establish this point, it would only serve to explain one step in the operation, the conversion of aliment into chyme; for in the subsequent change of chyme into chyle, we appear to have neither the requisites for fermentation, nor have we any of those phenomena which characterize chemical action. It is, however, perhaps equally difficult to explain this change upon the principle of mere chemical solution, as we seem to be altogether at a loss to determine by what agency the chyle is formed, or separated from the mass with which it is combined. But this point appears to be altogether so completely involved in obscurity, that it will probably be desirable not to attempt any explanation of it, until we have acquired a more correct knowledge of the nature of the phenomena themselves.

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[1] The experiments of Pringle, Observations, Appendix, p. 346, 364 et alibi; and still more those of M'Bride, Essays, N°. 1, were supposed, at one time, to be almost decisive in favour of the doctrine of fermentation. But it may be remarked concerning them, that they are little applicable to what takes place in the stomach, and merely shew us in what degree different alimentary matters are liable to spontaneous decomposition, when placed under certain circumstances. The opinion which they both of them entertained on the subject of fermentation was not sufficiently correct; Pringle made no distinction between fermentation and the entire destruction of a body, and M'Bride's account of it, although more precise, is still defective. He defines it, "an intestine motion, which arising spontaneously among the insensible parts of a body, produceth a new disposition, and a different combination of those parts." p. 2. In this definition, as well as in Pringle's the results are scarcely taken into consideration.

In the mean time, I may remark, that it would contribute very much to our knowledge upon the subject, if we were able decidedly to ascertain whether chyle is contained in chyme, and, consequently, that the office of the duodenum is only to separate it from the mass; or whether some chemical change takes place, by which it is actually produced in this organ. Could we prove that the former is the case, it would, no doubt, render the subject less complicated; although still we might be unable to show by what agent, or by what kind of process, the separation is effected. Other topics for consideration might be suggested, but the above are of essential importance to our forming any correct notions upon the subject; until this point has been ascertained, we have no right to complain of the intricacy, or obscurity of the subject, and to speak of the process of chylification as of something mysterious, which cannot be accounted for without attributing to matter new properties, or calling in the aid of new powers to explain the phenomena.

SECT. 5. *Peculiar Affections of the Stomach.*

There are certain affections of the stomach, besides those which are immediately connected with digestion, which it will be necessary for us to examine; both with respect to the mode in which they are produced, and their ultimate effects upon the œconomy. Among the most important of these are the sensations of hunger and of nausea, and, as connected with the latter, the act of vomiting.

Hunger is a peculiar perception experienced in the stomach, depending upon a deficiency of food; which, although it has been vaguely classed among the impressions that belong to the sense of touch, is essentially different from it, and entirely of a specific nature. The efficient cause of hunger has been frequently discussed by physiologists, and it has been generally referred either to a mechanical, or to a chemical cause, according to the respective tenets of the authors. The mechanical physiologists ascribed it to the friction of the sides of the stomach or of the folds and projections of its inner coat against each other; an hypothesis which is disproved by the anatomy of the organ, from which we learn that this kind of friction cannot exist; as from the rounded form of the stomach, as well as from its structure and composition, it would seem impossible that its different internal parts can come into forcible contact with each other[1]. Besides, the feeling of hunger is obviously of a specific nature, and totally different from the mere sense of resistance, no more resembling that which arises from the pressure of a hard body upon the skin, than the sense of sight does that of pressure upon the eye-ball. The chemical physiologists, on the other hand, accounted for the sensation of hunger by the action of the gastric juice, which, in consequence of its powerful effect upon organized matter, was supposed to have a tendency to corrode the internal membranes of the stomach. But this opinion may be considered as entirely disproved by the fact, which was stated above, that the solvent power of the gastric juice is confined to dead animal matter, and is therefore incapable of acting upon the living stomach, while, at the same time, we have no reason to suppose that it possesses any corrosive properties, similar to those of a chemical acrid, or which could be supposed likely to produce any painful impression upon the nerves of the part.

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[1] Haller, however, adopts this hypothesis respecting the proximate cause of hunger, Prim. Lin. § 638; El. Phys. xix. 2. 12; he thinks the attrition takes place between the ridges of the nervous coat; and illustrates the supposed effect by the acute pain which is experienced when friction is exercised upon any exposed part of the skin. The effects produced by long continued *fasting* are described with his usual minuteness; Sect. 2. § 3..7. Soemmer-

I conceive that the only explanation we can offer of the phenomena, is to consider hunger as a specific sensation produced upon the nerves connected with the stomach, in the same manner as the organs of sense have their appropriate nerves, each of them adapted to the peculiar perceptions of the organ[1]. It is not improbable that the action upon the nerves may be, in some way, effected through the intervention of the gastric juice, perhaps analogous to the action of light upon the retina; but this subject will be considered more fully in the part of the work which treats expressly upon the nervous system.

The sensation of thirst, although not referred to the stomach, may be noticed in this place, in consequence of its connexion with the digestive organs generally, and more particularly with the state of the stomach. It is seated in the tongue and fauces, and, in the natural and healthy state of the functions, depends upon the deficiency of the mucous secretions of these parts[2]. Although it appears to possess less of a specific character than the sense of hunger, yet it probably ought to be regarded as

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ing ascribes the pain from long continued fasting to the action of the gastric juice; but it does not appear whether he attributes the ordinary sensations of hunger to this cause; Corp. Hum. Fab. t. vi. p. 237. § 152. See also Boerhaave, Prælect. § 88. cum not. t. i. p. 171 et seq. We have some good observations upon the phenomena of hunger, by Magendie; Physiol. t. ii. p. 24 et seq.; he refers the proximate cause of hunger to the action of the nervous system; p. 30; see also his Art. "Digestion," in Dict. de Scien. Méd. t. ix. p. 370..5; see the remarks of Adelon, Physiol. t. ii. p. 396; and Dict. des Sc. Méd. t. ix. p. 363 et seq.; also Elliotson's Physiol. p. 49..53. We have some interesting cases of long-protracted abstinence in Dr. Copeland's App. to his Trans. of Richerand, p. 565 et seq.

[1] See the remarks of Sœmmering, t. vi. p. 233. § 149, entitled "Propria ventriculo sentiendi facultas;" and § 156; "Fames et sitis proprii sensus non sunt."

[2] Boerhaave, Prælect. § 585, 804; Haller, Prim. lin. § 639; El. Phys. xix. 2. 9; Magendie, Physiol. t. ii. p. 31..3; Blumenbach's Physiol. § 330..2, cum nota B.; Elliotson's Physiol. p. 52; Adelon, Dict. Sc. Méd. t. ix. p. 375 et seq.

something more than the mere sensation which would be produced by the mechanical condition of the part, and as a peculiar action on a certain set of nerves, resulting from the effect of an appropriate stimulus. But, although there is much obscurity concerning the efficient cause of hunger and thirst, their final cause is sufficiently obvious; they are the means by which we are warned of the necessity for supplying the system with the materials requisite for its existence. They belong to that class of actions which are termed appetites; where an effect, which is a compound of a physical and a mental operation, is connected with an evident useful purpose in the animal œconomy, and which is brought about through the intervention of the nervous system.

A variety of circumstances, differing very much in their nature and operation, agree in producing a peculiar sensation, which we refer to the region of the stomach, termed nausea. It is accompanied by a general disturbance of the different functions of the body, as well as a diminution of the powers of the muscular and nervous systems; and if it be continued for any length of time, it produces an effort to vomit. The act of vomiting consists in an inversion of the peristaltic motion of the stomach, beginning at the pylorus, and proceeding to the cardia, by which the contents of the organ are carried back into the œsophagus, and finally rejected from the mouth. Although the action commences in the muscular fibres of the stomach itself, it is promoted by the co-operation of the muscles of the abdomen, and the diaphragm, which indeed contribute very considerably to the ultimate mechanical effect[1].

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[1] The question has been much discussed, how far the muscles of the abdomen and diaphragm co-operate with the stomach itself in the mechanical act of vomiting. The opinion generally adopted by the modern physiologists is, that it originates in the stomach itself; but that, perhaps in every instance, and certainly in all violent efforts, the neighbouring muscles assist the muscular fibres of the stomach. Haller, El. Phys. xix. 4. 12, 14. resting on the authority of Wepfer, De Cicut. Aquat. p. 112, Lieutaud, Mém. Acad. pour 1752, p. 223 et seq., Sauvages, Nos. Meth. t. ii. p. 337, and others, suppose that the stomach alone is competent to the operation; whereas it was maintained by Chirac and Duverney, Mis. Curios. Dec. ii. ant. 4. obs. 125. p. 247, 8, and Mém. Acad. pour 1700, Histoire, p. 27, that the stomach is entirely passive. Hunter also maintained the same opinion, at least that the contraction of the muscular fibres is not essential to the act of vomiting; Anim. Œcon. p. 199, 0; and a series of experiments has been lately brought forward by Magendie, in support of the same opinion. He even goes so far as to state, that when the stomach was removed, and a bladder substituted in its place, vomiting occurred, which must necessarily have been effected by the sole action of the diaphragm, and the abdominal muscles; Mém. sur le Vomissement, p. 19, 0; and Physiol. t. ii. p. 138..0. I apprehend, however, that the ordinary opinion is the correct one, that the action commences in the stomach, but is very materially promoted by the parts external to it. The functions of the uterus, bladder, and intestines, are all favourable to this opinion; in each of them contraction evidently begins in the organ itself. The effect of dividing the par vagum has been adduced by both parties, in support of their respective opinions; the fact appears to be, that when this nerve is divided, although

The immediate causes of vomiting may be reduced to three classes. 1. Any substance irritating the stomach itself, such as undigested food, certain stimulating medicaments, which, from their specific action, have obtained the name of emetics, and various chemical acrids, which appear to produce vomiting, rather in consequence of the violent irritation which they cause, to whatever part they are applied, than of any specific effect upon the organ itself. 2. Certain irritations applied to various parts of the body, more or less remote from the stomach, but connected with it, either by the intervention of nerves, or in some way which we cannot satisfactorily explain, although we constantly recognize their operation. Among these may be enumerated certain affections of the brain, the motion of a vessel at sea[1], certain visible impressions upon the retina,

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nausea ensues, actual vomiting does not take place. As it is to the stomach that the par vagum is especially destined, it affords a presumption that this is the part where the act commences. We have some judicious observations on the subject by Bell, Anat. v. iv. p. 54 et seq. See also Cuvier, Hist. des Sc. Nat. t. iv. p. 266, 7, for an account of the experiments of Portal, which lead to the conclusion maintained in the text. A series of experiments on vomiting were performed by Legallois and Béclard, which consisted in injecting into the veins a solution of emetic tartar. They particularly noticed the action of the œsophagus, the diaphragm, the parietes of the abdomen, and the stomach itself. With respect to this last organ, the experiments tend to the conclusion, that vomiting cannot take place without the compression of some of the contiguous parts upon the stomach. They performed the same experiment with Magendie, the substitution of a bladder for the stomach, with a similar result; Œuvres de Legallois, t. ii. p. 91 et seq. See further on this subject the art. "Vomissement," by Adelon, Dict. de Méd. t. xxi. p. 427 et seq.; also Blandin's notes on Bichat, t. iii. p. 460. A paper has been lately published by Dr. M. Hall, "On the Mechanism of the Act of Vomiting," the principal object of which is to show, that the opinion which has been generally entertained, respecting the connexion between the act of vomiting and the state of the organs of respiration, is incorrect. He argues, both from various physiological considerations, as well as from the result of direct experiment, that the diaphragm is passive in the operation, and that the larynx is closed; hence he concludes, that vomiting is a modification of expiration, or that the muscles of expiration, by a sudden and violent contraction, press upon the contents of the stomach, and project them through the œsophagus. Dr. Hall's view of the mechanism of vomiting appears to be correct, at least if we add to it a previous step, that the violent expiration which attends the act of vomiting is necessarily preceded by a sudden and violent inspiration; Quart. Journ. v. 25. p. 388 et seq. But I conceive, that this mechanical action would be incapable of producing vomiting, were not the state of nausea induced on the stomach itself, which primarily affects the muscular fibres of the organ, probably through the intervention of the nerves. The contradictory statements which have been brought forwards, as the direct result of experiment, seem to prove that the subject still requires further investigation.

[1] Darwin refers sea-sickness to an association with some affections of the organs of vision, which, in the first instance, produce vertigo; Zoonom. v. i. Sect. 20. But it may be objected that sea-sickness is not necessarily preceded by vertigo, and that blind persons are equally subject to it. Dr. Wollaston has explained the affection by a change in the distribution of the blood; the descending motion of the vessel tending to cause an accumulation of blood on the brain; Phil. Trans. for 1810.

peculiar flavours and odours, certain medical agents, when applied to other parts of the body, as to the fauces, the rectum, or even to the external surface, calculi in the kidneys, and hernia of any part of the intestinal canal. 3. Mental impressions of various kinds, depending altogether, or in a great degree, upon association[1].

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[1] Haller, Prim. Lin. § 653; El. Phys. xix. 4. 13; Sœmmering, Corp. Hum. Fab. t. vi. p. 269..273. § 178. We have a detail of the opinions of the earlier physiologists in Magendie's memoir; but it is less complete than that given by Haller, § 14.

CHAPTER XI.

OF ABSORPTION.

We now arrive at the last of the three functions, which I classed together as furnishing the materials for the direct support of the system, that of absorption; the process by which the substances that serve for the growth and support of the body are carried into the blood, and are assimilated to this fluid. Although this may appear to be the primary, and, as we presume, the most essential of the effects that are produced by the process, there is also a further object which is brought about by the absorbent system. I have had occasion to remark, in various parts of this treatise, that it appears to be a general principle in the animal œconomy, that all the particles of which the body is composed, after a certain period, lose the power of performing their appropriate functions, and that it consequently becomes necessary to have them replaced by new matter. It is by means of absorption that this exchange of particles takes place, the former constituents being taken up by the vessels, and returned into the general circulation, to be either discharged or employed under some new form, while a different set of absorbents receive the recent matter from the products of digestion, and likewise convey it to the blood, whence it is distributed to all parts of the body. The subjects which will more particularly occupy our attention in this chapter, are: 1. An account of the apparatus by which the process of absoption is effected; 2. The uses of the absorbent system; in the 3d place we must consider the mode in which the absorbents act, so as to receive and convey their contents; and, in the last place, we must inquire into the nature of the connexion which subsists between absorption and the other functions of the animal œconomy.

Sect. 1. *Description of the Absorbent System.*

The absorbent system, by which I mean to designate those organs which are exclusively employed in the performance of this function, may be regarded as consisting of four parts, the lacteals, the lymphatics, the conglobate glands, and the thoracic duct. Although they compose so essential a part of the animal frame, and are very generally distributed to every organ of the body, yet our acquaintance with them is comparatively of modern date. It appears indeed that some portions of them were known to Galen, but in a very imperfect manner only, while he was ignorant of their specific use, and of their destination, conceiving them to be only a branch of the sanguiferous system[1]. Their very existence seems, after this period, to have been overlooked or forgotten, until Eustachio discovered the thoracic duct; but, although he describes its form and structure with considerable accuracy, he, like the ancients, had no conception of its specific nature, as forming a portion of a great system of vessels, distinct from the arteries and veins, and only indirectly connected with them.

It is to Aselli that we are indebted for our acquaintance with the lacteals, as a specific and distinct system, possessing a peculiar structure, and an appropriate office; a discovery which he made in the year 1622[2]. In the course of his dissections, he observed a series of vessels, unconnected with the arteries and veins, dispersed over the mesentery of a dog; and in consequence of the appearance of the chyle with which they were filled, he gave them the name of lacteals. He appears to have formed a correct opinion, that their course is from the surface of the intestines towards the central parts of the body; but the discovery of their termination in the thoracic duct, and of the connexion of this duct with the great venous trunks, was made by Pecquet, in the year 1651[3].

When Aselli and Pecquet had directed the attention of anatomists to these organs, they became the subject of very general investigation, and every circumstance respecting their structure and organization was minutely examined. They are described as originating from the villi or small projections, that are attached to the inner membrane of the intestines, which from this circumstance has obtained the denomination of the villous coat. These villi are said to be composed of, or to contain, a number of extremely minute capillary tubes, which branch off or radiate from what may be regarded as the termination of the proper lacteal, a number of these tubes uniting to form the larger vessel. But although detailed descriptions, and even drawings of these parts have been made by Lieberkuhn[1] and others, which are supposed to represent their form and structure, there seems to be still some reason to doubt of the existence of these parts, or at least of the exact nature of their connexion with the trunks of the lacteals. The very uncertainty, however, which prevails upon the subject, is a sufficient proof of the extreme delicacy of the organs, and as far as any practical consequences may be derived from a knowledge of their anatomical structure, we may be entitled to consider them as possessed of the physical properties of capillary tubes, but connected probably with other powers, which belong to them as vital agents[2].

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[1] De Anat. Admin. lib. 7. sub fin.; De usu part. lib. 4. cap. 19; An sanguis in arteriis &c. cap. 5.

[2] See his treatise, De Lactibus, accompanied by the singular engravings; also Sheldon, on the Abs. Sys. p. 20, 1. For an account of what was known respecting the absorbents, before the time of Aselli, the student may consult Bartholin, de Lacteis Thorac. c. 2; he remarks that Galen, Fabricius, Piso, Gassendi, and Conring saw some parts of the absorbents, but had false notions respecting them. In addition to these, we may add the name of Fallopio and Eustachio, the former of whom appears certainly to have seen some of the absorbents connected with the liver; see his treatise entitled "Observationes de Venis," obs. 3. Op. p. 532; and the latter the thoracic duct, which he describes with considerable accuracy, as seen in a horse; Opusc. Anat. p. 279, 0. Vesling, in his Syntagma Anat., describes what appears to be the lymphatics of some of the abdominal viscera; and Vanhorne, Nov. Duct. Chyl., gives a plate of what is probably intended for the thoracic duct, although very incorrectly delineated. See also Haller, El. Phys. ii. 3. 1. and xxv. 1. 3; and Mascagni, Prolegomena, p. 1..5. Bartholin, in his 4th chap., gives an account of the progressive steps of Aselli's discovery. We have a most ample and valuable catalogue of the various publications on the absorbent system, by Sœmmering, appended to his treatise, De Morbis Vasorum Absorbentium, occupying no less than 34 pages.

[3] Experimenta Nova, passim, cum fig.; see also Bartholin's 5th chap. This anatomist seems to plume himself upon the circumstance of his having been the person who first saw the thoracic duct in the human subject; but this cannot be regarded as any great advance, after it had been clearly demonstrated in the mammiferous quadrupeds; see Pecquet, c. 6. There is some reason to suppose that Vesling had an imperfect view of it previous to Pecquet; he published his Syntagma Anat. in 1647. The first plate of the thoracic duct was published by Van Horne in 1652.

The lacteals, after they have acquired a sufficient magnitude to be easily recognized by the eye, are carried along the mesentery; like the venous part of the sanguiferous system, the small branches run together to form larger branches, while these again unite, until the whole compose a few great trunks, which terminate in the lower end of the thoracic duct. The small branches frequently anastomose with each other, and in some instances, the connexions are so numerous and intricate, as to form a complete plexus. They are furnished with numerous valves, which are of a semilunar form, disposed in pairs, and with the convex side turned towards the intestines, so that, except in cases of extreme distention, they must prevent the retrograde motion of the contents of the vessels[1].

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[1] Diss. de Fabrica Vill. Intest. passim., cum Tab. 1, 2.

[2] Haller's observations on the degree of credit which we ought to attach to the descriptions that have been published of the mouths of the lacteals, as is always the case with whatever proceeds from his pen, is deserving of great attention; see Boerhaave, Prælect. not. 9. ad § 91. t. i. p. 181; also not. 4. ad § 103. p. 235. Since the time of Haller our knowledge of the minute anatomy of the mouths of the absorbents is no doubt increased, but still, I apprehend, that much of what is stated as actually existing, rests very much upon conjecture. We have, however, the authority of Hewson in favour of Lieberkuhn's description, although he differs from him in some minute points; Enq. c. 12. pt. 2. p. 171 et seq. He adduces his own experiments and preparations in support of the doctrine. Cruikshank, on the absorbents, c. 11, and letter to Clare, p. 32..4, supports the same opinion; but he informs us, at the same time, that he has never been able to detect the orifices of the lymphatics; ibid. p. 60. See also Sheldon on the absorbent system. p. 32..8; and tab. 1, 2; Béclard, add. à Bichat, p. 128, and Hedwig, Disquisit. Ampullac. On the other hand we have the drawings of Mascagni, tab. 1. fig. 1. 3. and tab. 3. fig. 1, 2, 3, 5, which do not sanction the descriptions of Lieberkuhn. It must be acknowledged that Sheldon's testimony in favour of Lieberkuhn is very direct, and perhaps ought to outweigh the negative evidence even of Mascagni. Magendie altogether discredits the accounts that have been published; Journ. Physiol. t. i. p. 3 et alibi. The observations of Du Vernoi, Mém. Petrop. t. i. p. 262 et seq., do not seem to have been confirmed.

Besides the peculiarity in their course, in the nature of their contents, and their numerous valves, the lacteals are farther characterized by the thinness and transparency of their coats, by which they are rendered very difficult of detection, except when they are distended with the white and opake chyle. But notwithstanding the fineness of their texture, they would appear to possess considerable strength, so as to be capable of being distended by injections far beyond their natural dimensions, without being ruptured. When they are thus injected, the frequency of the valves occasions them to assume a jointed appearance, somewhat resembling a string of beads. They appear to be composed of two essentially distinct parts; an interior membrane, by the duplicature of which the valves are composed, and which probably constitutes a considerable part of their actual substance, and a membrane surrounding this, which may be considered as determining the bulk of the vessel, and giving its general form. To these two some anatomists have added an external peritoneal membrane, but this strictly speaking, is no more than the general envelope which the peritoneum affords to all the abdominal viscera[2].

We appear to have very unequivocal evidence of the contractile nature of the lacteals, and yet, owing, as we may presume, to the transparency of all their parts, it is doubtful whether the muscular fibres have ever been detected in them[3]. Like all the vessels of any considerable magnitude, they are provided with a set of arteries, by which they are nourished and immediately connected with the vital system; no nerves have been detected as specifically belonging to the lacteals, nor have we any direct evidence of their possessing any sensitive properties[1].

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[1] The original discoverers of these vessels were aware of the existence of the valves, but they were examined with so much accuracy by Ruysch, that the merit of the discovery is not unfrequently bestowed upon him. See his Dilucidatio Valvularum, Op. t. i. p. 1..13. They are very accurately described by Sheldon, on the Absorbent System, p. 28.

[2] For a general description of the lacteals, see Haller, El. Phys. xxv. 1. 4..8; Mascagni, Vas. Lymph. Corp. Historia, pt. 1. § 7. art. 8. p. 50, 1. tab. 1. fig. 7; Sheldon, on the Absorbent System, c. 2. pl. 3, 4, 5; Santorini, Tabulæ, N°. 13, fig. 3; Magendie, Phys. t. ii. p. 158..0. The translation of Mascagni's work, with copious notes, by Bellini, may be advantageously consulted: it is not accompanied by plates.

[3] Nuck has figured what he conceived to be fibres in the conglobate glands and thoracic duct; Adenologia; p. 35..8. fig. 13, 14, 19; but Mascagni supposes that what he saw referred to the adipose cells, and farther informs us that he never could detect fibres in any of the absorbents, pt. 1. sect. 4. p. 26. Cruikshank, however, has "repeatedly demonstrated fibres in the thoracic duct;" on the Absorbing Vessels, p. 61; and strongly advocates the irritability (contractility) of the absorbent system generally; ibid. p. 62. Meckel, Man. t. i. p. 185, does not admit the existence of the fibres, and this is the case with Chaussier and Adelon, art. "Lymphatique," Dict. des Sc. Méd. t. xxix. p. 256. Breschet considers it doubtful, Dict. de Méd. t. xiii. p. 389. Some curious observations were made by Desgenettes, on the action of the absorbents after the apparent death of the system, Journ. Méd. t. lxxxiv. p. 499 et seq. Similar observations were afterwards made by Valentin, t. lxxxvi. p. 231 et seq.; this action was not, however, supposed to depend upon contractility. Wrisberg informs us, that he has frequently seen

The discovery of the lymphatics was a few years posterior to that of the lacteals; for although their structure and composition are nearly similar, yet in consequence of their contents being transparent and colourless, they are less easily detected. On this account it was not until about thirty years after the discovery of Aselli that we have unequivocal evidence of their having been distinctly recognized, and it still remains somewhat doubtful to whom the honour of the discovery is to be ascribed. Perhaps the first anatomist who clearly announced them as a distinct system of vessels is Joliffe, but as he himself published no account of his own observations, his claim is not very satisfactorily substantiated[2]. We have, however, sufficient proof that these vessels were observed by Bartholin, and by Rudbeck, nearly about the same time, but that Bartholin was the first to publish the discovery. This publication gave rise to a claim on the part of Rudbeck and his friends, which led to a controversy that was carried on for some time with considerable acrimony. It is scarcely in our power, after this lapse of time, to form a decisive judgment on this question, but from the documents which we possess, I think we may conclude, that the lymphatics were first clearly discovered by Rudbeck, and that Bartholin had some intimation of the discovery; this, he

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spasmodic contractions in the large vessels, and in the thoracic duct; Observ. Anat. Med. de Vas. Abs. Morb., in Comm. Soc. Reg. Gott. v. ix. § 19. p. 149.

[1] Cruikshank remarks that nerves are apparently distributed to the absorbent vessels, but that we cannot perceive that they are much under the influence of these nerves; p. 64.

[2] The claim of Joliffe rests upon the testimony of Glisson, as given in his treatise on the Liver, published in 1654; Haller, Bibl. Anat. t. i. p. 452. He entitles one of his sections, "De Vasis Aquosis, sive Lymphæ Ductibus ad Hepar spectantibus," and adds the following narrative: "Incidi primum in eorum notitiam, indicio D. Jolivii, atque anno 1652, sub initium Junii, quo tempore ille doctoratus gradum adepturus, me Cantabrigiæ in eum finem convenerat. Asseruit nempe, dari vasorum quartum genus, à venis, arteriis, et nervis planè diversum; idemque ad omnes ut plurimas saltem corporis partes distribui, humorem aquosum in se complecti. Addebat porro, se in compluribus animalibus eorundum ductum investigasse, in artubus, scil. testiculis, utero, aliisque etiam partibus, certo sibi constare, liquorem in iis versum mesenterium tendere, et particulatim ad initium sive radicationem ejus;" c. 31. p. 319. Glisson, notwithstanding the designation which he gives to these vessels, afterwards expressly states: "quod ad hepatis negotium nihil spectare viderentur; neque enim dictus Jolivius, quenquam horum ductuum inde proficisci etiamnum monuerat." Haller does not appear to have thought Joliffe's claim to discovery to have possessed much weight; he remarks, concerning it, "Vasa lymphatica hic (Glisson) ut alii Angli, Jolivio tribuit inventa, ignoto inter incisores nomini;" Bibl. Anat. ut supra. See also the remarks of Mascagni in his prolegomena.

appears, rather disingenuously, to have concealed; yet we may allow that he had considerable merit in profiting by the hint, and in pursuing the investigation, with the skill and address which he displayed on all points connected with anatomy[1]. The peculiar nature of these vessels, and their supposed importance in the operations of the animal œconomy, soon attracted the attention of all the anatomists of the age, and from that period until our own time they were successively detected in the different parts of the body, and in the different classes of animals. The labours of Wm. Hunter and of Monro Sec. were particularly directed to the examination of the absorbent system, and some of the anatomists of the present day are still engaged in discussing the nature of their action, and the relation which they bear to the other parts of the system.

The lymphatics appear to be very similar to the lacteals in their structure, and in the nature of their constituent parts, being composed, like them, of a fine and transparent, but firm and elastic substance, provided with numerous valves, and forming frequent anastomoses. They seem likewise to possess a similar degree of contractility, although from the nature of their contents, it is not so easy to demonstrate it by actual experiment, and they are also analogous to them in their principal function, and in their ultimate destination. But they differ from the lacteals in their situation, and in their contents, for whereas the lacteals are confined to the mesentery and serve only to convey the chyle, the lymphatics are found in almost every part of the body, and are filled with a transparent and colourless fluid, which, as its name imports, was supposed to consist principally of water. The origin of the lymphatics appears to be from the various surfaces of the body, external as well as internal[1], and partly from the degree in which we are actually able to trace them by anatomical injections, and partly from observing changes to be produced in various organs, which we can only explain by the power of absorption, we are induced to suppose that they exist in every part of the body[2].

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[1] Those who are desirous of examining into the respective merits of Bartholin and Rudbeck may consult Haller, not. 4. ad Boer. Præl. § 121. t. i. p. 277..9. or Bibl. Anat. t. i. § 378. p. 400 et seq. and sect. 415. p. 447 et seq., where he will find a list of the various publications to which the controversy gave rise; and still more, El. Phys. ii. 3. 1, where the history of the discovery is detailed with the author's accustomed accuracy, and with that correct distribution of justice to the respective claimants, for which he is so highly and justly celebrated. The result of the inquiry has, I acknowledge, produced upon my mind the impression which is stated in the text. Bartholin was certainly a skilful and active anatomist, to whom the science lies under many obligations, but I think that his works betray the ambition of being regarded as a great discoverer, a spirit, which, when it once takes possession of the mind, is too apt to blunt the finer feelings of honour and integrity. Bartholin's own statement is briefly made in his Anat. Reform. p. 621, 2. There is reason to suppose that Bogdan's angry treatise was written under the immediate inspection of Bartholin. Haller obviously inclines to the part of Rudbeck; he says, " videtur ex his ipsis datis verus novorum vasorum inventor fuisse;" see also the remainder of the paragraph in Bibl. Anat. t. i. p. 447, 8; see also El. Phys. ii. 3. 1. p. 161, 2. Boerhaave, in his work, " Methodus Studii Medici," gives a history of the successive discoveries that were made respecting the lymphatics; C. 2. De Vasis Lymphaticis, t. i. p. 443 et seq. For a further account of the controversy see Mascagni, Prolegomena, and Meckel, Manuel par Jourdan et Breschet, t. i. ch. 2. p. 179..202. Bartholin's statement of his claim is contained in his Anat. Reform. vide supra, and in his treatise Vas. Lymph. Hist. Nov.; Rudbeck states his claim in his Nova Exer. Anat. and in his Epist. ad Bartholinum. See farther on this subject the 9th chapter of Elliotson's Physiol. *p.* 140..2.

Their great trunks are arranged into two principal series or systems, one near the surface, and the other more deeply seated, and we find that, for the most part, they follow the course of the great veins. Whether this depends upon any necessary connexion which takes place between these sets of vessels, during their course from the superficial to the central parts of the system, or whether they are lodged near each other for the mere purpose of mechanical accommodation, we are, perhaps, not able positively to determine; but the latter seems the more probable supposition. The main branches of the lymphatics are finally reduced to three or four great trunks, which, like the lacteals, terminate in the thoracic duct.

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[1] Magendie, Physiol. t. ii. p. 175, remarks, " On ignore la disposition que les lymphatiques ont à leur origine; on a fait à ce sujet beaucoup de conjectures, égalément dénuées de fondement." Injections demonstrate that they arise from minute branches which can be traced into the neighbourhood of the various surfaces, but beyond this we have no certain information.

[2] Haller entitles one of his sections, El. Phys. ii. 3. 4. " Ubi nondum visa sunt;" it would seem that, at the period when he wrote, there were very few parts of the human body in which they had not been detected; and since his time this number is still farther diminished. It appears, however, that there are some organs, more particularly the brain, the spinal cord, and the organs of sense, which are at least much less plentifully supplied with absorbents than the other soft parts; indeed it may be doubted whether we have any unexceptionable evidence of their having been seen in these organs. Magendie, writing in 1817, says, " C'est en vain qu'on a cherché jusqu' ici ces vaisseaux dans le cerveau, la moelle épineuse, leurs enveloppe, l'œil, l'oreille interne," &c.; Physiol. t. ii. p. 174; see also Journ. de Physiol. t. i. p. 3. 1821. Mascagni gives us a view of a few small lymphatics which he had discovered in the brain; tab. 27. fig. 1, 2, 3. Are we to consider the above fact as a proof that the brain and nerves are less disposed to undergo that gradual exchange of particles which has been so frequently referred to? For descriptions and views of the lymphatics, see Haller, El. Phys. ii. 3. 2 et seq.; Hewson, Enq. c. 3. and pl. 3. . 6; Mascagni, Vas. Lymph. Hist. part 1. sect. 7. p. 37 et seq.; and tab. 4 et seq.; Cruikshank, on the Absorbents, p. 148 et seq.; Sœmmering, Corp. Hum. Fab. t. v. p. 388 et seq.; Meckel, Diss. Epist. de Vasis Lymph.; Do. Manuel, sect. 6. ch. 2; many of Mascagni's plates are transferred into Cloquet's valuable " Manuel;" Rullier, Art. "Inhalation," in Dict. Sc. Méd. t. xxv.; Chaussier et Adelon, Art. " Lymphatique," ibid. t. xxix. p. 249. . 260; Quain's Elem. of Anat. p. 560. . 574. The origin of the lymphatics has generally been supposed to be still more obscure than that of the lacteals; we have, however, an account by Watson, which bears the marks of fidelity, of his being easily able to detect their open mouths on the surface of the bladder; Phil. Trans. for 1769; pl. 16. See also Monro on Fishes, p. 30.

This duct is the ultimate destination of all the lacteals and lymphatics; it is a vessel of considerable size, which lies in the neighbourhood of the spine, running in a somewhat tortuous course, from the third or fourth dorsal vertebra, to about half an inch above the trunk of the left subclavian vein. It is then bent down into the form of an irregular arch, and opens into this vessel, nearly at its union with the jugular of the same side. There is considerable irregularity in the form of the thoracic duct; in a majority of cases, it is composed of a single trunk; occasionally there are two trunks, which are not very dissimilar from each other in their dimensions, while not unfrequently we have one or more small trunks that pass in the same direction with the main duct, which are generally united to it in some part of its course, or, in some cases, are separately transmitted into the subclavian vein[1]. Besides what is properly considered as the thoracic duct, in which all the lacteals and the greatest part of the lymphatics terminate, a portion of these latter, especially those which proceed from the upper part of the body, and from the superior extremity of the right side, are generally collected into a separate trunk, named the great right lymphatic vessel, or right thoracic duct, which is connected with the right-subclavian vein[2]. Although this formation of the thoracic duct and its supplementary appendages, does not affect its physiological functions, and is no more than a mere anatomical variation, yet it is of importance to be aware of it in our experiments on the absorbent system; for it appears that the earlier anatomists were not unfrequently induced to form false conclusions on this subject, by supposing that they had intercepted the transmission of the chyle from the absorbent to the sanguiferous system, when they had merely prevented it from passing along the main trunk of the thoracic duct[3]. Excepting in its greater size, it is not essentially different from the other absorbent vessels; its coats are thin and transparent, yet possessed of considerable strength and elasticity; it is furnished with numerous valves, and appears to possess a remarkable degree of contractility[4].

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[1] In Mascagni, tab. 15, we have an example of this irregularity.

[2] This is said to have been discovered by Stenon in 1664; Meckel, Man., § 1703; Haller, Prim. Lin. § 766, and Hewson's Enq. pt. 2. p. 61..3. pl. 4. Cruikshank, p. 176, 7, conceives that Hewson was the first who described the lymphatics of the right side as being collected into one trunk. For the figure of the part, see Mascagni, tab. 19, Nos. 185, 187.

[3] See the observations and experiments of Sir A. Cooper in Medical Records and Researches, p. 86 et seq., where he shows that when the duct is obstructed either by mal-conformation, or by a ligature, the chyle still finds its way into the veins. See also the paper of Magendie, in his Journ. t. i. p. 21.

[4] For the description and views of the thoracic duct, see Haller, Prim. Lin. ch. 25. § 565; Op. Min. t. i. p. 586 et seq. tab. 11, 12; and El. Phys. xxv. 1. 10..3; Albinus, Tab. Vas. Chylif; Cheselden, Anat. pl. 26; Portal, in Mém. Acad. pour 1770, and Sabatier, pour 1786; Haase, de Vas. Cutis et Intest. Abs. tab. 2. and tab. 3. fig. 1.; there are two valuable treatises on

The conglobate or lymphatic glands compose a conspicuous portion of the absorbent system. They are met with in various parts of the body, always connected with the lacteals or the lymphatics. They are of various sizes, sometimes simple, sometimes in groups or clusters, and although their use is not understood, we may presume that they serve some important purpose, from the circumstance of every absorbent vessel, during its course, passing through one or more of these glands[1]. They are very numerous in the mesentery as connected with the lacteals, and as attached to the lymphatics; there are large clusters of them in the groin, the neck, and the axilla, as well as in the course of the greater lymphatic trunks, not far from their termination in the thoracic duct.

It is, however, only in the mammalia, or the animals which most nearly resemble them in their structure and functions, that these glands are found so abundantly; even in birds they are rare, and still more so in fishes[2]. Although we are not acquainted with the nature of the functions which is exercised by these glands, we may fairly presume that they serve, in some way or other, to the completion or the perfection of the absorbent system, as they are found principally in the higher

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this organ in Haller, Disp. Anat. t. i. by Bolius and by Saltzmann; Mascagni, ps. 1. sect. 7. art. 8. p. 52. and tab. 13, 15, 19; Sheldon, pl. 5; Cruikshank, p. 166..176; Magendie, Physiol. t. ii. p. 160; Meckel, Manuel, § 1698.

[1] It has been questioned how far this remark is literally true; Hewson affirms that he has injected lymphatics, which have been unconnected with any glands; Enquiries, pt. 2. p. 44, 5; and the same statement has been made by others. But Mascagni, in his numerous injections, has never met with this circumstance, and expresses himself as if he doubted the correctness of the observation; Vas. Lymph. Hist. pt. 1. sect. 4. p. 25; see also Gordon's Anat. p. 74.

[2] The researches of the modern anatomists have proved that the absorbent vessels exist in the great classes of the mammalia, birds, amphibia, fishes, and insects. Blumenbach observes, that the heart and the circulation of the blood are always co-existent with the absorbent system, and that although animals which are without red blood appear to absorb fluids, yet that it is not done by the same kind of vessels as in animals that possess red blood; Comp. Anat. by Lawrence, p. 253. I apprehend that the first part of this remark cannot be considered as perfectly correct, since they have been detected in the silk-worm, Sheldon on the Absorbent System, pt. 1. p. 28; and the Echinus Marinus, Monro on Fishes, p. 125..8. tab. 44. We may conceive that the whole process of growth and nutrition, in all its parts, depends upon a species of absorption even in the lowest orders of animals, although there are many considerations which would lead us to suppose, that it consists in little else than mere mechanical imbibition, quite distinct from proper vascular action. Dr. Fleming's account of the comparative anatomy of the absorbents may be perused with advantage, although it must be regarded rather as a popular, than as a technically correct view of the subject; Zoology, t. i. p. 338. Mascagni, speaking of the classes of animals that possess a lymphatic system, says, "hoc forsan donantur alia animalia corde et vasis sanguineis destituta," p. 2. See on this subject Chaussier and Adelon, in Dict. Sc. Méd. Art. "Lymphatique," p. 249; Breschet, Dict. de Méd. t. xiii. p. 397; Hewson, Phil. Trans. for 1768, p. 217 et seq., and Enq. pl. 2. ch. 4, 5, 6; and Owen, in Cyc. of Anat. v. i. p. 327, for an account of *the discovery* of the absorbents in birds.

orders of animals. In those of an inferior description, we have the vessels without the glands, while in those of a still lower order, neither the vessels nor the glands can be detected, so that the process of absorption must be carried on by some more simple apparatus. There has been the same kind of controversy respecting the structure of the conglobate, as of the conglomerate glands, whether they contain cells, or whether they consist of a mere congeries of vessels. Nuck[1] and the earlier anatomists generally maintained the former opinion, while the more recent authors, on the contrary, for the most part, incline to the latter[2]. Hewson informs us that "each gland is a congeries of tubes consisting of arteries, veins, lymphatic vessels, and nerves, connected by the cellular substance."[3] Mascagni gives an account of his own observations on the glands, when they are examined, after having been injected with wax or glue; "apparebit lymphatica . . . dividi, invicem coire, flecti, extenuari, dilatari, cellas efformare, rursus constringi, mutuo demum commixtione surculorum, præsertim vero ramis in cellas immissis, indeque inductis, amplo commercio donari."[4]

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[1] Nuck gives us the results of his own injections in a simple and candid manner, accompanied with rough engravings; C. 2. p. 30 et seq. fig. 9..12.

[2] Cruikshank, however, argues in favour of the cellular texture, c. 14, and Mr. Abernethy appears to have clearly proved that this is the case in the whale; Phil. Trans. for 1796, p. 27 et seq.

[3] Enquiries, v. iii. c. 2. pl. 2; see also Béclard, add. à Bichat, p. 231; Monro (Tert.) Elem. v. i. p. 558; Werner and Feller, Vas. Lact. and Lymph. Descrip. tab. 2; they delineate the gland as distinctly consisting of a network of vessels, I cannot, however, but suspect that the drawing is somewhat exaggerated; Haller, El. Phys. ii. 3. 16..27, gives a very full account of the structure, situation, and supposed uses of these glands; see also Boyer, Anat. t. iii. p. 243..257; Rullier, ubi supra, p. 120 et seq.; Breschet, ubi supra, p. 394. I have already had occasion to remark, p. 477, that Sylvius was the first who distinguished these glands from those that are more immediately concerned in secretion, and appropriated to them the name of conglobate, while he styled the latter conglomerate; these names have been generally employed, although not very correct or appropriate. For views of these glands, see Mascagni, tab. 1. fig. 8..12, tab. 2. fig. 4..8. tab. 4. fig. 2. tab. 8, 16, 26; Cruikshank, pl. 3; Sheldon, tab. 3, 5; Parr's Dict. "Absorbents," 1, 2, 3. Mr. Bell, in the art. "Amphibia," Cyclop. of Anat. v. i. p. 96, describes what he terms pulsating cavities, in the lymphatic system of certain animals of this class, which serve to propel the fluids towards the veins, into which they are received. They were discovered by Müller, and have been also seen by Panizza; something, which appears to be of an analogous nature, has been noticed by Dr. Hall in the eel. According to Dr. A. Thomson, the class of invertebrate animals have no absorbent system distinct from the circulatory system, Cyc. of Anat. v. i. p. 648.

[4] Vas. Lymph. Hist. pt. 1. sect. 5. p. 31. There has been considerable difference of opinion respecting the anatomical relation between the conglobate glands and the nerves; Malpighi and Nuck thought that these glands were plentifully supplied with nerves; Hewson that they have few nerves, and that they only become sensitive when affected by acute inflammation; Enquiries, pt. 3. p. 52; Mascagni informs us that he never detected nerves distributed to these glands, p. 30; he further states this to have been the case with Walter. Gordon remarks that no nerves have been discovered accompanying these vessels; Anat. p. 77.

SECT. 2. *Office of the Absorbent System.*

The office of the absorbents is literally expressed by their name; it consists in receiving or taking up certain substances and in transporting them from one part of the body to another. The substances which are thus taken up, may be referred to two only, the chyle and the lymph, the former being received by the lacteals, and the latter by the lymphatics. The immediate object of the action of the two sets of vessels is also essentially different, that of the first being to convey a fluid from the part where it is formed into the blood, in order that it may directly serve for the nutrition of the body, the latter serving, in the first instance, to remove what is useless or noxious, and to dispose of it in such a manner, that it may either be applied to some secondary purpose of utility, or be finally discharged from the system.

Although there is some uncertainty respecting the anatomical structure of the mouths of the lacteals, and there is considerable difficulty in explaining the mode in which the chyle enters them, we can have no doubt that they are so dispersed over the surface of the intestines, as to be able to receive the chyle when it is presented to them. By their contractile power, assisted by the mechanical action of the valves, and probably by other causes, which will be considered presently, the fluid, when it has once entered the vessels, is necessarily propelled from their extremities towards their trunks, until at length it arrives at the thoracic duct. The action and functions of the lymphatics do not appear to be essentially different from those of the lacteals; we have, however, a still less distinct conception of their extremities and of the mode in which they receive their contents; when the lymph has once entered them, we may presume that it is propelled forwards precisely in the same manner with the chyle. There is, however, one circumstance in which these two sets of vessels would appear to differ from each other, at least in degree; that whereas the lacteals seem to be capable of receiving nothing except chyle, which they, in some way or other, possess the power of selecting from the heterogeneous mass of matter through which it is diffused, and with very few exceptions, reject every thing else that is presented to them; the lymphatics, on the contrary, possess the distinguishing property of taking up, as occasion may require, every substance that enters into the composition of the body, as well as extraneous and heterogeneous matters of various kinds, that are accidentally, or intentionally placed in contact with their mouths. This is not only the case with our various fluids and solids, which are composed of similar elements, and might therefore be conceived to be readily convertible into each other, but they have the power of absorbing the earth of bones, and even of taking up various medicinal agents and carrying them into the system, so as to enable them to produce the same effect upon the functions, as if they had been received into the stomach.

With respect to the thoracic duct we have no reason to suppose that there is any thing specific in its action, or that, except in its size, it differs from the other absorbent vessels. Its particular office appears to be that of serving as a reservoir in which the chyle and lymph may be deposited, for the purpose of being gradually transmitted into the sanguiferous system, as there is some reason to suspect, that injury would ensue if too large a quantity of this fluid were poured into the veins at any one time. It is not improbable that a certain degree of retardation is necessary, in order that the contents of the absorbent system may be more completely assimilated, before they are mixed with the blood, which could not have been so conveniently effected, without the intervention of a receptacle similar to the thoracic duct.

From the above remarks it appears that we can have little doubt respecting the use of the vascular part of the absorbent system, but this is not the case with its glandular appendages. It can scarcely indeed appear surprising that we are unable to explain their use, while their structure is still involved in so much obscurity, and yet, on the other hand, it may be said that we know so little of glandular action, or of the change which it produces upon the fluids that are subjected to it, that we should rather attempt to elucidate the subject by physiological, than by anatomical investigations. The most probable opinions that have been entertained upon the point are, either that these glands are proper secreting organs, and are intended to prepare a peculiar substance, which is mixed with the chyle and lymph, or that they offer a mechanical obstruction to the progress of these bodies, by which means their elements are allowed to act upon each other, and thus to produce some necessary change in the nature of the fluids which pass through them[1]. The examination of the contents of these vessels does not enable us to decide this question, nor am I acquainted with any considerations, anatomical or physiological, which appear to have much weight in directing our determination.

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[1] Richerand supposes that the glands tend to assimilate and animalize the chyle, and to separate the heterogeneous matters from it, but this opinion is entirely conjectural, Elem. p. 153; this is nearly the opinion of Blumenbach, Inst. Phys. § 425, 442. Mascagni supposes that they serve to detain the fluid and to mix its parts together; this is proved by the difference in the nature of the fluid before and after it passes through the gland; pt. 1. sect. 5. p. 33; I do not, however, perceive that it is stated in what this difference consists. Magendie very candidly confesses his ignorance upon the subject: Phys. t. ii. p. 166, 201. Haller supposes that the functions of these glands are more important in the young than in the adult animal; principally, as it would appear, resting his opinion upon their greater size, and upon their containing a greater proportion of fluid in the former case; El. Phys. ii. 3. 25. It is natural to suppose that during the growth of the body, a greater quantity of nutritive matter will be conveyed to the blood which must necessarily pass through these glands, whatever use we may ascribe to them. Mascagni agrees with respect to the fact of their being larger and more turgid in youth; Vas. Lymph. Hist. ps. 1. sect. 5. p. 33. See also Chaussier and Adelon, ubi supra, p. 278; Rullier, in Dict. Sc. Méd. Art. "Inhalation"; Meckel, Man. sect. 6. ch. [illegible]; Adelon, Dict. de Méd. Art. "Chylifères."

We must, however, suppose that some important change is effected by their means, from the fact mentioned above, that every absorbent, during some part of its course to the thoracic duct, passes through one or more of these glands. But the same mode of reasoning might lead us to conclude, that although the absorbent glands are necessary to the existence of the higher orders of animals, they are not so for the purposes of nutrition and growth generally, as it appears that there are large classes of animals, which resemble the mammalia in many of their nutritive functions, and in the vascular part of the absorbents, which are without any lymphatic glands, or are very sparingly furnished with them [1]. It is not easy to point out any circumstances that belong exclusively to the mammalia, which can assist us in explaining the necessity for these appendages to their lymphatic system.

Ever since the complete discovery of the lacteals and the lymphatics, it has been a general opinion, both with anatomists and physiologists, that their appropriate office was absorption; but it has been a very warmly contested point, whether this operation was exclusively performed by these vessels. The ancients, who were ignorant of their existence, supposed that the process of absorption and transmission, so far at least as they had any definite ideas upon the subject, was performed by the veins; and in modern times, after the full discovery of the extent and properties of the lymphatic system, it was still supposed, that the veins assisted in the process, and even, in some cases, were the principal agents. This was almost the universal opinion until the middle of the last century [2], and is the doctrine which was strenuously maintained both by Boerhaave [3] and by Haller [4].

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and "Lymphatique (Physiologie)"; Desgenettes, Journ. Méd. t. xc. p. 322 et seq.

[1] Blumenbach's Comp. Anat. by Lawrence, ch. xiii. p. 256.

[2] It would appear that Malpighi conjectured that the lymphatics originated from the glands; De Struct. Gland. p. 3. Nuck, in consequence of the results of some of his injections, was led to think that they were immediately connected with the blood-vessels, Adenographia, ch. 4; and this opinion was after that time very generally embraced. Monro (Sec.) in his treatise, de Venis Lymph. Valv. p. 14..21, gives a most ample list of references to the authors who adopted this opinion, including, indeed, almost all the anatomists of eminence previous to the period when he wrote.

[3] Prælect. § 103. t. i. p. 234; also § 247 et seq. t. ii. p. 303.

[4] In note 1 to § 106 of Boer. Prælect. t. i. p. 241, he gives a statement of the question, and a list of the authors who have defended the doctrine of venous absorption; see also note 1 to § 245. t. ii. p. 197, and El. Phys. ii. 1. 28. Magendie enumerates Ruysch, Boerhaave, Meckel, Swammerdam, and Haller, as the most powerful supporters of this doctrine; Physiol. t. ii. p. 238. See also Walter, sur la Resorption, Nouv. Mém. Berl. pour 1786, 7.

The arguments which were employed by these distinguished physiologists, as well as by the other anatomists who had preceded them, may be all reduced to two classes: the first, and indeed those on which they were disposed to place most confidence, were the results of experiments, which seemed to demonstrate, that injections of various kinds were capable of passing from one set of vessels to the other, thus indicating that there existed a natural and direct communication between them. The most skilful anatomists of that period generally admitted this to be the case; and I do not find that the correctness, either of the experiments or of the deduction from them was ever called in question[1]. The other series of arguments, which, although indirect, were supposed to be of great force, was derived from the anatomical fact, that in many parts of the human body, where the effects of absorption are sufficiently obvious, no lymphatics had been detected, and still farther, that there are large classes of animals, and those possessed of an organization in many respects similar to the mammalia, where the absorbent system appears to be wanting. In these cases, therefore, it seemed that we were reduced to the necessity of supposing that absorption was effected by the veins, and if it were so in one case, there was no difficulty in extending it to the rest, especially so far as related to other parts of the same animal.

The doctrine of venous absorption was first formally attacked by William Hunter and Monro (Sec.), who seem, nearly about the same period, to have entered upon the regular investigation of the subject[2]. With respect to the mode of reasoning that

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§ 15 et seq. Hoffman appears to have been one of the earliest writers who decidedly maintains the opinion that absorption is exclusively carried on by the lymphatics; Med. Rat. Lib. 1. sect. 2. ch. 3. Some of the most direct experiments in favour of venous absorption are those of Kaaw Boerhaave, who informs us that fluids injected into the intestines, under certain circumstances, were afterwards detected in the mesenteric veins; De Perspir. § 469. p. 202, 3; but the experiments are related very briefly, and in so general a way as not to admit of our placing much confidence in them.

[1] Another class of experiments consisted in passing ligatures round the thoracic duct, so as to render it impervious to the passage of the chyle, yet still the nutrition of the animal did not appear to be interrupted; and the same conclusion seemed to be warranted by various cases of natural obstruction of the duct, or by certain malformations of the part, where it was either defective or did not convey its contents into the veins.

[2] It is a painful, yet necessary task, which is imposed upon the historian of science, to notice those personal controversies which occasionally take place, and which frequently originate in the right to certain discoveries. Few have been more acrimonious than the one alluded to in the text; the respective claims of the two parties may be found in Wm. Hunter's Med. Com. and in Monro's Observations Anat. and Phys., and the sixth chapter of his Treatise on the Brain. I believe I may assert, that the sentiments of the great majority of the men of science were in favour of the former. Monro, in his Inaugural Dissertation, published at Edinburgh in 1755, p. 25, and still more explicitly in his Treatise on the Lymphatics, published at Berlin in 1757, c. 12. p. 556, clearly states his doctrine respecting the non-absorption of the veins; but

had been employed by their predecessors, they endeavoured, by carefully repeating the experiments, and by observing attentively every circumstance connected with them, to show that where injections had passed between the veins and the absorbents, some rupture or extravasation had taken place, and that when the process was performed with proper care, and the necessary allowance made for unavoidable accidents, the connexion between the sanguiferous and absorbent systems could not be substantiated[1]. They afterwards examined, with much assiduity, the various parts of the body, where absorbents had not been previously detected, and they were successful in discovering them in so many new situations, that it appeared to be a fair inference, that every part is provided with a proper absorbing apparatus, although the peculiar texture and appearance of the vessels rendered them difficult to be demonstrated[2]. They were

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there appears ample testimony to prove, that Wm. Hunter had, for some years previously, publicly taught the same doctrine in his lectures, in the most decisive and unequivocal manner. It was a very natural, and even a very laudable feeling in the present Professor Monro, to decline entering into the merits of a discussion in which his father's character was involved, but certainly, as he thought proper to reprint Black's letter to his father, Elem. v. ii. p. 459, justice demanded that he should likewise have inserted the two which the same eminent philosopher subsequently wrote to Wm. Hunter; Med. Com. p. 22..5. Although the connexion of Dr. Baillie with the Hunters might render him a suspicious evidence, yet on the other hand, his thorough knowledge of the point in discussion, as well as his well-known candour and impartiality, must render his testimony of considerable value. After speaking of Wm. Hunter's investigations, Dr. Baillie remarks; "This discovery has been claimed by a celebrated professor of anatomy in Edinburgh; but I shall avoid entering into the dispute. It is enough to say, that Dr. Hunter taught this doctrine in the year 1747, six years before the professor declares himself to have made the discovery. Dr. Hunter has, therefore, an undoubted claim to priority, whatever praise may belong to any other person for having made the same discovery without assistance."

[1] Although the correctness of their experiments is generally admitted, yet the researches of some of the most accurate of the modern anatomists seem to indicate that occasional communications exist between some of the lymphatics and the contiguous veins; but this is a different kind of relation from that which was contemplated by the older anatomists, and would appear to be much less uniform and extensive. This point is fully discussed by Fohmann, in his late work, "Sur le commun. des vaiss. Lymph. avec les veins." Lippi, in his "Illustr. fisiol," had given an account of a series of observations, tending to prove the direct connexion between the sanguiferous and the absorbent systems, but these are supposed by Fohmann to be incorrect. The observations of Fohmann have been confirmed by Louth, in his "Essai sur les vaisseaux Lymph." See also the remarks of Antomarchi, Ann. Sc. Nat. t. xviii. p. 108, 9. Panizza of Pavia also opposes the doctrine of Lippi; Osserv. c. 3, 5. Mr. Kiernan, in his elaborate researches into the anatomy of the liver, remarks that the doctrine of Lippi has been "satisfactorily confuted" by Panizza; Phil. Trans. for 1833, p. 729. See also Dr. Graves's Lecture on the lymphatic System; and Elliotson's Physiol. p. 128, 9.

[2] See Cruikshank on the absorbent system, Introd. for an account of Wm. Hunter's researches on the subject; Monro states the result of his investigation in his Dissert. de Sem. et Test., inserted in Smellie's Thes. t. ii., and in his treatise, de Ven. Lymph. Valv. p. 103.

ably seconded in their labours by various anatomists, both in this country and on the continent, who extended their observations to other classes of animals, and discovered an absorbent system in many of them, where it had not been previously suspected. Among those who were the most successful in this department we may class Hewson, who shared with Monro the merit of having first observed the absorbents in fishes, and Mascagni, Sheldon, and Cruikshank [1].

In addition to these investigations, which must be considered as principally intended to counteract the arguments employed by preceding anatomists, the main scope of which was to show, that the structure of the body did not render it necessary for us to have recourse to venous absorption, experiments were performed for the direct purpose of proving that the veins are incapable of performing this function. Some of the first and most decisive of these were executed by J. Hunter. They consisted in filling portions of the small intestines with milk, or some similar kind of fluid, and retaining it there so as to produce a degree of distention of the part, and afterwards examining whether any of the fluid that was employed had entered the veins of the intestines. This was said in no instance to have occurred, and hence it was argued, that as venous absorption did not take place, under circumstances which seemed favourable to its action, the veins were not possessed of this power [2].

In consequence of the number of observations and experiments of this description, which were, from time to time, laid before the public, and from various physiological and pathological considerations, all of which appeared to concur in confirming the doctrine of absorption being exclusively performed by the lacteals and lymphatics, the old opinion was daily losing ground, until at length the modern doctrine seemed to be fully established, in so much that there was perhaps no hypothesis in the whole range of physiological science, which appeared to rest on a firmer foundation, than that of the non-absorption of the veins [3].

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[1] The controversy which took place between Monro (Sec.) and Hewson, respecting the priority of discovery on this point, was no less acrimonious than the one mentioned above between Monro and Hunter; see Hewson's Enquiries, App. to the 1st. vol. We may presume that the general sentiment in this case was in favour of Hewson, as the Royal Society presented him with the Copley medal in 1769, for his experiments on this subject, which they would scarcely have done, had they not supposed them to be original. Hewson's communications to the Royal Society are in their volumes for 1768, p. 217 et seq., containing a paper on the lymphatics of birds, in which he mentions that he had also discovered these vessels in a turtle and in fishes; and for 1769, in which we have two papers, the first on the lymphatics in a turtle, p. 198 et seq., and the second in fishes, p. 204 et seq.

[2] The experiments are related in Med. Comment. c. 5. p. 42..8. Cruikshank gives an abstract of them in c. 5. p. 21 et seq.; he remarks, "these experiments appear to me perfectly conclusive."

[3] We have a very judicious summary of the opinions that had been successively adopted on the subject of venous absorption, given us by Mascagni,

It affords us, however, a striking illustration of the uncertainty of all human knowledge, and the mutability of all opinions, even those that seem to be founded upon the most direct and unequivocal evidence, that shortly after this unity of sentiment had taken place among physiologists, and when the controversy had ceased, or when the only subject of discussion was to ascertain in what degree the different anatomists had contributed to the establishment of the doctrine, it was again called in question by one of the first authorities of the age; direct experiments were adduced, that bore the marks of great ingenuity in their contrivance, and accuracy in their execution, the results of which were perhaps at least as decisive in favour of venous absorption, as the former had been in support of the opposite doctrine. The labours of Magendie on this subject, to which this observation refers, come to us in such a form, as to entitle them to the highest attention, and although on all topics connected with the animal œconomy, where we are principally to depend upon experiments performed on living animals, it is necessary to be extremely cautious in forming our judgment, yet it is in a great measure, upon such facts, when fully established, and clearly developed, that our ultimate conclusions must be founded.

The arguments by which the Hunters endeavoured to establish their position, that the process of absorption was exclusively carried on by the lymphatic vessels, were derived partly from general considerations, connected with the analogies of the other parts of the animal œconomy, and partly from experiments performed for the express purpose of proving their hypothesis. Of these latter an account has been given above, and I shall only further refer to them for the purpose of remarking, that they were conceived to be so convincing and so perfectly satisfactory, that they were generally acquiesced in by anatomists and physiologists, almost without a single exception.

The considerations of a more general nature, which were adduced to prove the exclusive function of the lymphatics, were principally two; in the first place, the analogy which they bore to the lacteals, in their physical properties, their anatomical structure, and their destination; and as it was admitted that absorption was the appropriate office of the lacteals, so it was concluded that the same office must be performed by the lymphatics. In the second place, a variety of facts were adduced, in order to show that when the system became affected by the introduction of any noxious substance into the circulation, a mor-

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in Part 1. Sect. 2 and 3, of his great work. He considered the doctrine of absorption being exclusively performed by the lacteals and lymphatics as firmly established. See also Rullier, ubi supra, p. 136 et seq. The doctrine of venous absorbents was, however, still maintained by many intelligent anatomists, especially by the high authority of Meckel, in his treatise, "De Fin. Ven. et Vas. Lymph." and of Walter, "Sur la Resorption;" see particularly the general conclusion, § 92.

bid state of the lymphatics might be traced from the part where the injury was inflicted, along their trunks, towards the thoracic duct; thus proving both that some injurious substance had been received, and that it had been conveyed by these vessels. It was then argued, that if these vessels possess the power of absorption in certain cases, and if we know of no other function which they perform, it may be inferred that the whole business of absorption is carried on by them.

The force of the analogical argument appears to be still admitted; it is therefore upon the experiments that have been lately performed, that the opposite opinion is principally founded; it will consequently be necessary for us to examine how far they are so direct, as decisively to prove the point in discussion; or whether they are more to be regarded, as tending to weaken the force of the experiments that were formerly adduced in support of the contrary doctrine. We shall find upon inquiry that they bear upon both these positions; and among those which must be classed under the first denomination, there is one which is detailed by Magendie, as performed by himself, in conjunction with Delille, which seems to be so well contrived, and the results of which, as related by the author, are so unequivocal, that it would almost appear sufficient singly to substantiate the hypothesis [1]. The experiment consisted in dividing all the parts of one of the posterior extremities of a dog, except the artery and the vein, the former being left entire for the purpose of preserving the life of the limb. A quantity of a poisonous substance, the Upas tiuté, was then applied to the foot, when, in the short space of four minutes, its effects were rendered visible upon the functions of the animal, and in ten minutes it proved fatal. In this case it was supposed that there could be no conceivable communication, by which the substance could be conveyed from the extremity to the central parts of the system, except the vein; and hence the conclusion seemed to follow irresistibly, that the vein was, in this case, the absorbing vessel. In order to render the result still more unexceptionable, a second experiment was tried, in which small leaden tubes were introduced into the artery, and the vein; and, after being secured in their places by ligatures, the vessels themselves were completely divided, so that the two streams of the arterial and venous blood respectively, were now the only channel of communication between the extremity of the limb, and the body of the animal; yet, under these circumstances, the poison produced the same effect as in the former case. It will be unnecessary to adduce any more experiments of this description; for the object was so clearly defined, and the result so unequivocal, that it has been generally supposed that there could be no room for objection, except such as might depend upon the want of skill or accuracy in the operator, or upon certain causes which always interfere with experiments upon the living body, but to which the one in question does not appear to be particularly obnoxious [1].

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[1] Journ. Physiol. t. i. p. 23 et seq.; Elém. t. ii. p. 183..5.

And with respect to the other description of experiments, those which were performed for the purpose of contradicting, or invalidating, the statements which had been brought forwards by J. Hunter, we have equally direct evidence in favour of the modern doctrine. We are told that experiments, similar to those of Hunter, have been made by Flandrin, but with contrary results; and that Flandrin's experiments were repeated by Magendie, and found to be correct [2]. We have here, therefore, the opposing testimony of men, both of them eminent for their general science, and especially for their address in experimental researches. If they both stood upon equal ground, we might fairly estimate the authority of Hunter as equal to that of his opponent: but when we reflect upon the advantage which, from various causes, accrues to every succeeding experimentalist, over those who have gone before him, the balance of opinion must necessarily incline to the latter. In this case, moreover, we are informed that the experiments of Magendie, and his friends, were considerably more numerous than those of Hunter; and I may farther observe, that receiving each of them as they are given to us by their respective authors, and supposing that we may repose with equal confidence upon their correctness, the latter would appear to have the advantage, not merely in their greater number, but likewise in the mode in which they were performed. I do not, however, feel disposed to assent to the modern doctrine of absorption, merely upon the faith of experiments, until they have been further repeated, and diversified; but, in the mean time, we may go so far as to assert, that venous absorption is neither impossible, nor, perhaps, antecedently improbable; and that with the evidence which we now have in its favour, we can admit of no physiological hypothesis, or train of reasoning, which necessarily involves its non-existence[1].

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[1] There is, indeed, a circumstance connected with the experiment, which seems to require further explanation, or in which the expression employed is somewhat ambiguous. In speaking of the mode in which the poison was applied, M. Magendie says, that it was "enfoncé dans-la patte" of the dogs; now if this implies that a wound was made in the part, into which the substance was inserted, it may be conceived that a portion of it would be, in the first instance, introduced into the veins, and carried directly to the heart. This, however, it is evident, would not be a case of absorption, but simply of the power which certain bodies possess of uniting with the blood, and still retaining their specific properties. Various facts, both medical and physiological, prove the extent and frequency of this occurrence; among others I may refer to the recent experiments of Mr. Key, in Med. Chir. Tr. v. xviii. p. 212, 3.

[2] Physiol. t. ii. p. 181 et seq.; Journ. Méd. t. lxxxv. p. 372, et seq.; t. lxxxvii. p. 221 et seq.; and t. cx. p. 73 et seq. In taking a view of the controversy, respecting the absorbing power of the veins, I have endeavoured to regard it, as little as possible, as a mere question of authority. I would not, therefore, assert that I conceive the experiments of Magendie to be, in themselves, superior to those of Hunter; but as they appear to have been much more numerous, and are of later date, we may suppose them to be entitled to more confidence.

But, whatever conclusion we may be induced to form respecting the office of the veins, or the share which they possess in absorption, it appears a well-established principle, that the only use of the lacteals and the lymphatics is to absorb certain substances that are presented to their orifices; it will now therefore, remain for us to inquire, what is the distinctive function of each of these systems of vessels, or what is the nature of the substance which they each of them respectively absorb? With regard to the lacteals, the question is easily answered; the only substance which they are destined to receive, is the chyle; and they appear to be the appropriate vessels for conveying this substance from the intestines, where it is produced, to the thoracic duct[1]. We may, therefore, consider the lac-

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[1] A summary of the experiments and arguments of Magendie, in favour of the doctrine of venous absorption, are contained in his Memoir "Sur les Organes de l'Absorption;" Journ. Physiol. t. i. p. 18 et seq., an abstract of which is given in his Elém. Phys. t. ii. p. 238..243. He concludes his observations with the three following positions: "1. It is certain that the chyliferous vessels (lacteals) absorb chyle. 2. It is doubtful whether they absorb any thing else. 3. It is not proved that the lymphatic vessels possess the power of absorption, and it is proved that the veins have this power." M. Ségalas performed a series of experiments, which may be considered as the reverse of those of M. Magendie; he divided the blood-vessels of a portion of the intestine and left the lacteals, when he found that no absorption took place; Magendie's Jour. t. ii. p. 117 et seq.; hence he concluded that the lacteals do not possess the power of absorption. The same conclusion, so far as respects the mesenteric absorbents, is the direct inference from the late experiments of Tiedemann and Gmelin, of which an account will be given below. Bichat states the arguments that have been urged against venous absorption, and admits their force; yet he is not disposed to decide against the doctrine; Anat. Gén. "Syst. Absorb." t. ii. p. 104, 5. His commentator, Béclard, embraces the opinion more decidedly, p. 130. One of the most intelligent defenders of the doctrine, among the contemporaries of Hunter, was Meckel; see his treatise, De Fin. Ven. ac Vas. Lymph., written in 1772. We have a fair view of the state of the question, as it stood between thirty and forty years ago, in the Edinburgh System of Anatomy, v. iii. pt. 6. sect. 8. § 2. p. 236..245. See also some sensible observations in Hewson's Enq. v. i. c. 9. It appears, both from his work and from Monro's, that the result of injections was the principal argument employed at that time to prove absorption by the veins, an argument which would be most satisfactory, could we prove that no rupture or extravasation had taken place: but in consequence of the perpetual liability to such accidents, it must be regarded as of a very equivocal nature. The analogy of the lymphatics with the lacteals, and the effect of the absorption of deleterious substances, are the proofs on which Hewson principally rests his opinion. This circumstance is also insisted upon by Cruikshank, who remarks, that in the absorption of poisons, it is the lymphatics, and not the veins, that are inflamed; On the absorbent system, p. 28. A remark of the same kind is made by Mr. Bell; Anat. v. iv. p. 303; he says, indeed, that the veins do occasionally become inflamed, but that they are much less liable to inflammation than the lymphatics. It has been urged, as a proof of absorption being carried on by the lymphatics, that this process continues for a considerable time after the circulation has ceased. Bichat limits this period to two hours; ubi supra, t. ii. p. 118; but it is supposed by many anatomists to remain for a considerably greater length of time. It is necessary to observe, that although Magendie conceives that the lacteals have the power of absorbing chyle, and probably are the principal agents in this operation, yet he performed a series of experiments, in conjunction with Delille, the results of which convinced him that the mesenteric veins also possess this power; see Journ. Physiol. t. i. p. 23 et seq.; also Elem. Physiol. t. ii. p. 183..5. The experiment consisted in detaching a portion of the small intestines from the remaining part of the canal, in dividing all its lacteals and its blood-vessels, except one artery and one vein; a deleterious fluid was then injected into the divided intestine, and after a certain

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interval, the effects of the poison were manifested in the system. Without intending to throw the least reflection upon the fidelity of the narrator or the skill of the experimentalist, I cannot but remark, that I conceive, in so complicated an operation, it would be impossible to guard against various sources of inaccuracy, that would essentially interfere with the inference that we must draw from the experiment.

[1] It is not intended, by this observation, to deny absolutely that extraneous substances are never, under extraordinary circumstances, admitted into the lacteals. The earlier experimental physiologists generally agreed, that colouring substances might be detected in the chyle; this was especially the case with Lister and Musgrave's experiments on indigo; Phil. Trans. for 1683, No. 143. p. 6; and for 1701, No. 270. p. 819. No. 275. p. 996; also Lowthorp's Abrid. v. iii. p. 101..5, and La Motte's Abrid. 2. par. ch. 4. p. 75, 6; and, what is more important, their results were confirmed by Haller; who informs us, that he repeated the experiments with success: El. Phys. xxiv. 2. 3. This is also stated as the result of J. Hunter's experiments, Med. Comment. p. 44 et seq.; and Cruikshank assents to the opinion; on the Absorbents, c. 8. But it is generally agreed that the power of these vessels in admitting the introduction of extraneous substances is very limited; and the late experiments of Magendie, Flandrin, and Dupuytren, tend to show, that even this very limited power does not exist; see Physiol. t. ii. p. 168, 9; where it is stated, as the result of direct experiment, that when alcohol, camphor, &c. are mixed with the food, the sensible properties of these substances are detected in the blood, but never in the chyle. These experiments, I may observe, are directly opposed to those of Hunter; while, on the other hand, we are informed, that they agree with the results which had been obtained by Hallé; see Fourcroy, Syst. v. x. p. 91. We have also a similar kind of experiment stated in a general way, in the Edinburgh Med. Journ. v. xix. p. 154, 5; where a quantity of starch and indigo was confined in a portion of the intestine, when it was found upon examination, that none of it had entered the lacteals. We have, also, a very elaborate train of experiments by the active and intelligent physiologists, Tiedemann and Gmelin, which appear to have been conducted with great attention to every circumstance that might affect their accuracy, the results of which confirm the conclusions of Magendie. Their object was to ascertain whether any direct communication exists between the digestive organs and the blood-vessels, except through the route of the lacteals, and the thoracic duct. The experiments consisted in mixing with the food of certain animals, various odorous, colouring, and saline substances, which might be easily detected by their sensible or chemical properties, and in comparing, after a proper interval of time, the state of the chyle with that of the blood in the various mesenteric veins. The odorous substances employed were camphor, musk, alcohol, oil of turpentine, and assafœtida; these were generally found to be retained in the system, so as to be detected in venous blood, and in the urine, but not in the chyle. The colouring matters were sap-green, gamboge, madder, rhubarb, alkanet, and litmus; these appeared, for the most part, to be carried off without being absorbed; while the salts, viz. potash, sulphuro-prussiate of potash, muriate of barytes, muriate and sulphate of soda, acetate of lead and of mercury, and prussiate of mercury, were less uniform in their

teals as the immediate agents in nutrition, by which the matter, after being duly elaborated in the digestive organs, is transmitted to the blood, for the purpose of being assimilated to this fluid, and finally employed in repairing the waste that is necessarily occasioned by the separation of the various secretions.

With respect to the lymphatics, although it would appear that they, at all times, contain a greater or less quantity of the transparent fluid, from which their name is derived, yet we have reason to suppose that their contents are of a more miscellaneous nature than those of the lacteals. If we adopt the Hunterian hypothesis, we must suppose that all the constituents of the body, as well as a variety of other substances, which are either intentionally or accidentally placed in contact with the extremities of the lymphatics, are capable of entering into them, and of being conveyed along them to the thoracic duct. And if we embrace the opinion of Magendie, that the function of absorption is divided between the lymphatics and the veins, or even principally carried on by the latter, there are many morbid phenomena, which seem to prove that extraneous bodies of various kinds are capable of passing along them[1]. How

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course. A considerable portion of them seemed to be rejected, while many of them were found in the urine, several in the venous blood, and a very few only in the chyle. Hence the authors conclude, that the odorous and colouring substances never pass into the lacteals, and that saline bodies do so occasionally only, or perhaps incidentally; the whole of them are, however, found in the secretions, and they must, therefore, have entered into the circulation by some other channel than the lacteals; Edin. Med. Journ. v. xvii. p. 455 et seq.

[1] It is obvious that, upon the hypothesis of Magendie, this variation will not take place in the contents of the lymphatics, or at least in a much less degree. And it must be admitted that the properties of the lymph seem to be more uniform than might have been expected, had it been composed of, or formed from, all the different constituents of the body. According to Magendie, it bears a strong analogy to chyle, especially in the characteristic property of separating by rest into two parts, one more solid and fibrous, and another which remains fluid, and more resembles albumen; Physiol. t. ii. p. 171, 2. Chevreul has given the following analysis of the lymph of a dog; ibid. p. 173:

Water	926·4
Fibrin	4·2
Albumen	61·
Muriate of soda	6·1
Carbonate of do.	1·8
Phosphate of lime	·5
Do. of magnesia	
Carbonate of lime	
	1000·0

Mascagni, however, says, that the lymph is not uniform in all parts, but that it partakes of the properties of the contiguous substances, bile, fat, &c. Vas. Lymph. Hist. p. 1. § 4. p. 28, 9. A similar opinion is maintained by Blumenbach, Inst. Physiol. § 438. p. 237. Hoffmann's Observations on the Lymph, considering the period when they were written, are not without their value; Med. Rat. lib. i. § 2. ch. iii.

far the substance which is conveyed by the lymphatics may occasionally serve for the purposes of nutrition, it is not, perhaps, very easy to ascertain ; but we may venture to assert that nutrition is not their sole, or even their primary function. This, we can scarcely doubt, is the appropriate office of the chyle ; and although it may be admitted, that under peculiar circumstances, or for a limited period, the lymph may contribute to the support of the system ; yet every analogy would induce us to suppose nutrition to be no more than a temporary, or secondary office of the lymphatics.

We are indebted to J. Hunter for the first consistent hypothesis upon this subject ; and I conceive that it must be regarded as one of the most important physiological doctrines which we owe to his genius. He conceived that the primary use of the lymphatics is to mould and fashion the body, so as to give it its proper form, and to enable it to increase in bulk, while its individual parts retain their appropriate figure, and proportionate size. When we reflect upon the mode in which an organized part is enlarged, we perceive that it does not grow by accretion, like a crystal, nor by simple distention, a process which is inconsistent with its texture, and the nature of its composition. We shall find, on the contrary, that it grows by an increase of each individual part of which the whole structure is composed. With respect to the muscles, for example, the number of fibres are augmented, at the same time that each individual fibre is increased in size, that the same thing takes place with respect to its tendinous extremities, and the other membranous parts that are dispersed through its body ; so that if we compare the structure and composition of the corresponding muscles in their different periods of growth, we shall find the same general relation between its parts, with regard to their size and situation, at the same time that the bulk of each is increased.

The same position is, perhaps, still better illustrated by observing what occurs with respect to the bones. We observe the bone of a young animal to possess a characteristic shape, to have a certain number of projections and depressions in its different parts. If we examine the same bone in the adult animal, we shall find a general correspondence between the parts of both ; we have, for the most part, the same number of projections, and bearing the same relation to each other. But it is obvious that this change of shape would not have been effected by the accretion of new matter to the original bone, nor by the distention of the parts already existing ; in short, the only way in which it could have been brought about, is by the removal of the particles of which the young bone was formed, and the gradual deposition of others in their proper situations, so as to produce the adult bone. Now, an operation of this kind can have been effected by no means with which we are acquainted, except by absorption ; and hence we conclude that the lymphatics alone, or at least in conjunction with the veins, are the agents employed for this purpose[1].

The action of the absorbents is still more strikingly displayed in many morbid states of the system, where the effects are more visible to the eye than in the ordinary operations of the body in its natural and healthy state. Whenever, from any cause, an effusion of a fluid, or a deposition of a solid, occurs in an unnatural situation, we find that these extraneous substances are gradually removed. We also observe that parts, even while in their natural situation, are capable of being removed by the action of pressure; where the source of supply is, by any means, cut off from a part, even although the part itself is not otherwise affected, its particles are gradually abstracted until it finally becomes, in a great measure, obliterated. The examples that we have of this kind are often very extraordinary, as, for example, where we find the pressure of a soft part, such as the pulsation of an artery, or of an aneurysmal tumour, sufficient to wear down the texture of the hardest bone.

The lymphatics, with or without the aid of the veins, are the only agents which can be supposed to produce these effects, yet the substances which compose them, or the fluids which they contain, cannot act as direct solvents of the bones, because they are not brought into contact, nor, if they were so, are they adapted for this kind of action. The removal of the component parts of the body, by means of the absorbents, we find to bear no relation either to the mechanical texture of the parts; nor, so far as we can judge, to their chemical composition. Thus, if a pulsating tumour be so situated as to press at the same time upon a bone and a muscle, we shall find that the earthy part of the one, and the fibrin of the other, will be removed; while the membranous part remains in either case little affected. If, however, the pressure be still continued, the membranous basis begins to be absorbed; thus exhibiting the singular fact of a body, while in a soft and flexible state, wearing down a similar substance in a more dense and compact form. This property in the absorbents seems to be obviously connected with that principle in the animal œconomy, to which I have so frequently had occasion to refer; that the matter of which the organs of the body are composed, during the performance of their various functions undergoes some change, which renders it no longer fit for its original purpose; and that it therefore becomes necessary for it to be removed, and for new materials to be deposited in its place. We seem to have no means of forming even a plausible conjecture concerning the nature of this change, whether it be chemical or mechanical, but whatever it be, we may conceive that an important secondary purpose is served by it, viz. the one which has been described above, that of moulding the form of the body, and regulating its increase. Hence we arrive at the conclusion, that the lacteals and the lymphatics are both of them essential to the growth of the body, although in a different way; the lacteals procure the materials, and convey them into the blood, whence they are abstracted by the secretory arteries, while the lymphatics regulate the mode of their deposition, and contribute to reduce the parts into their proper form and dimensions. Upon this principle we can comprehend the mode in which an organized body may increase in size and receive the addition of new matter, without affecting the relation between its individual parts, an object which could not possibly be accomplished by the mere addition of new particles, in whatever way they were added to the former, without these being, at the same time, removed[1].

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[1] A good view of this hypothesis, as well as of the opinions which were at that period sanctioned by the most eminent anatomists of the age, may be found in Winterbottom's Inaug. Diss., published in 1781; Thes. Med. t. iv. p. 263 et seq.; the only reference which he gives is to Hunter's lectures. See also Cruikshank on the Absorbing Vessels, p. 108, 9; and Adelon, art. "Absorption," Dict. des Sc. Méd. t. i. The doctrine was very admirably laid down by Mr. Allen, in his Lectures on the Animal Œconomy.

SECT. 3. *Mode in which the Absorbents act.*

In considering the mode in which the absorbents act, there are two distinct subjects that present themselves for our inquiry; how do the substances enter the mouths of the absorbents, and how, after they have entered, are they conveyed along the trunks? With respect to the lacteals, I have described the peculiar apparatus, which is said to be attached to their mouths, called villi, consisting of a number of small vessels, that are so disposed, as to be brought into direct contact with the substances which are intended to enter them. It has been generally supposed that the fluids enter the villi upon the principle of capillary attraction, and that the object of this peculiar structure is to obtain a number of these capillary vessels, which, in consequence of their minuteness, may be more active in the process of absorption[2]. This supposition is not, however, without its difficulties. The structure and physical properties of these villi have been thought not to be peculiarly well adapted for the purpose of this species of attraction, if we are to suppose that it operates in the same manner in them as it does in rigid or inorganic tubes.

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[1] Prof. Monro, (Tert.) gives a summary view of the functions of the lymphatics in his Elements, v. ii. p. 598, 9, which, upon the hypothesis of the non-absorbing power of the veins, is just and comprehensive, except in regard to the share which they have in regulating the growth of the body; the same omission occurs in Blumenbach's account of Absorption; Inst. Phys. sect. 29. Cruikshank enters fully into the proofs of the absorption of solids, and the inquiry into the mode in which it is accomplished; On the Absorbing System, p. 108 et seq.

[2] Boerhaave supposed that the peristaltic motion of the intestines had a considerable influence in propelling the chyle into the mouths of the lacteals; Prælect. § 103. t. i. p. 233, 4; but I conceive that this would be as likely to produce an opposite effect.

But before we can form a decided judgment upon this point, we must determine exactly in what sense we are to employ the term capillary attraction; whether we are to refer it merely to a mechanical action, or whether we are to suppose that there is an attraction between the tube and the fluid, of a kind which may be termed elective, whether it has the power of taking up some substances in preference to others, although possessed, as far as we can perceive, of the same physical properties.

There are many circumstances connected with the lacteals, which lead us to conclude that they exercise, to a certain extent, this power of selection, and that there is a specific attraction between the vessel and the fluid, which causes the latter to enter into the former[1]. As far as we are able to judge, when particles possessed of the same physical properties are presented to their mouths, some are taken up while others are rejected; and if this be the case we must conceive, in the first place, that a specific attraction exists between the vessel and the particles, and that a certain vital action must, at the same time, be exercised by the vessel, connected with, or depending upon its contractile power, which may enable the particles to be received within the vessel, after they have been directed towards it. This contractile power may be presumed to consist in an alternation of contraction and relaxation, such as is supposed to belong to all vessels that are intended for the propulsion of fluids, and which the absorbents would seem to possess in an eminent degree[1].

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[1] Most of the eminent modern physiologists admit of a certain species of elective attraction, although they differ somewhat respecting its extent and its relation to the other vital powers. Bichat conceives that absorption is a vital action, in which there is a relation between the vessel and the fluid, which is of an elective nature; Anat. Gén. t. ii. p. 125. Dumas supposes that the lacteals do not act upon the principle of capillary attraction, because their absorbent power ceases with their vitality; but it may be remarked upon this opinion, that the capillary action is conceived to exist only at their extremities, the propulsion of the chyle along the vessels themselves being a contractile operation, and therefore connected with life. He supposes that the lacteals possess an elective sensibility, which he even goes so far as to assimilate with the power by which the impressions of external objects are conveyed to the sensorium; Physiol. t. ii. p. 397, 8. Dr. Young admits of the existence of this elective power, but ascribes it to the agency of electricity; Med. Lit. p. 112. Richerand carries the doctrine still farther; he does not admit that capillary attraction has any share in the process of absorption, but refers it all to sensibility and contractility, the sensibility of the nerves of the part disposing it to select and receive certain substances, which are afterwards propelled by the contractility of the vessels themselves. Parr maintains the opinion that the extremities of the lacteals possess the power of elective attraction; Dict. Art. "Absorb. Vasa," "Lactea Vasa," "Lymphæ Ductus." Sir C. Bell speaks of the appetency of the capillaries, and the discriminative property of the secretory and other small vessels; Anat. v. iv. p. 290; this mode of expression can be metaphorical only; but it is objectionable, as conveying no correct conception of the nature of the effects so designated. Magendie, referring to the hypothesis of the action of the absorbents depending upon the specific sensibility of their extremities, and the inorganic contractility of the vessels themselves, observes, "on a peine à concevoir comment des hommes d'un merite éminent aient pu proposer ou admettre de pareilles explications;" Physiol. t. ii. p. 162, 3; also Journ. de Physiol. t. i. p. 3 et alibi.

The conclusion which would follow from this view of the subject is, that an attraction exists between the mouths of the lacteals and the chyle, which seems to be analogous to, or identical with, the elective attraction which unites different chemical substances; that the lacteals, as well at their extremities as through their whole extent, are possessed of contractility, by which the fluids, when they have once entered, are propelled along them, an effect which is probably promoted by the pressure of the neighbouring parts, while the numerous valves, with which they are furnished, prevent the retrograde motion of their contents[2].

Our inquiries have been hitherto more immediately directed to the action of the lacteals; we must now examine how far we are to suppose that a similar kind of action takes place with respect to the lymphatics. There is, perhaps, in this case, an additional difficulty concerning the extremities of the vessels, and the mode in which their contents are, in the first instance, received by them; but there is reason to suppose that the transmission of the fluids themselves is conducted upon the same plan with that of the lacteals. With respect to their extremities, as we are not able to trace them to their commencement by our anatomical examinations, we can form no judgment except from analogy, and when we consider how very little is actually known respecting the mouths of the lacteals, it will appear that analogy, in this instance, can scarcely afford us any assistance. We are in fact in almost total ignorance about the whole subject; we do not know where they are situated, with what parts they are connected, how they are brought into contact with the substances which they receive, nor by what power they are enabled to take them up.

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[1] Sheldon remarks, "that they are the most irritable of any system of vessels in the human body;" On the Absorbents, p. 28. Dr. Young, on the contrary, seems to doubt whether the absorbent vessels possess any peristaltic motion, and is inclined to ascribe the whole effect to capillary attraction; he cites the analogy of the lachrymal duct, the contents of which he conceives are propelled entirely by capillary attraction, the duct itself being altogether passive; Med. Lit. p. 112. Mascagni supposes that they do not possess contractility, because he could not detect the fibres; Vas. Lymph. Hist. pl. 1. sect. 4. p. 27, 8. Cruikshank supports the doctrine of the irritability (contractility) of the absorbents; On the Absorbent System, c. 12.

[2] This was in substance the doctrine of Haller; Prim. Lin. c. 25. § 568. He supposes that the fluids enter by capillary attraction, aided by the peristaltic motion of the intestines, and that the contents are afterwards carried forwards by the contractile force of the vessels. Tiedemann and Gmelin, in the valuable dissertation to which I have referred above, maintain that the absorbents, as well as the blood-vessels, possess a vital power, by which they propel their contents, but which is different from irritability (contractility). They promise to enter into the explanation of this supposed contractile power at some future period; as I am not aware that this has yet been done, I forbear to make any *observations* upon their hypothesis.

There appears likewise to be much more difficulty in ascertaining the nature of the contents of the lymphatics than of the lacteals. The lymph, as far as it has been made the subject of actual examination, is said to be nearly uniform in its nature, but the experiments that have been made upon it are not numerous, and probably did not admit of any great degree of accuracy[1]. And we seem to have very decisive proofs, from the inflammation or other visible effects produced, that the lymphatics are capable of absorbing a great variety of substances, differing from each other the most widely in their nature, so that it would almost appear, as if, by a certain mode of application, any substance might be forced into them. Nor is this conclusion affected by the hypothesis of Magendie; for although we might agree with him in supposing, that, in the ordinary operations of the system, the veins are the principal, or even the sole instruments in removing the materials of which the body is composed, yet we have unequivocal evidence, that when certain poisonous or medicinal agents are applied to their extremities, they may be received or forced into them, and conveyed into the circulation. The case of the metallic or other medicinal substances, that are taken up by the lymphatics, may appear to be less difficult to explain, because the absorption is generally produced by friction, or some mechanical process, which may be supposed to force the substance into the mouths of the vessels, or to produce an erosion of the epidermis, which may enable the substances to come into more immediate contact with the mouths of the vessels. We may also imagine, that when the component parts of the body are brought into close approximation with their capillary extremities, they are then taken up in the same way that the chyle is absorbed from the intestines.

There is moreover a peculiar difficulty which attends this part of our subject, arising from the circumstance, that the densest solids are absorbed as well as the more fluid components of the body. What are we to conceive of the intimate nature of this operation? If solution of the substance be necessary, we are at a loss to find a proper solvent; many of the substances are insoluble in water, or in the serous fluid which is found in the vessels, while, on the other hand, it is, perhaps, not easy to conceive, how the substances can be absorbed, without being previously dissolved, and still more so, how the solids can have their texture broken down, and enter the vessels, particle by particle, as it were, and be suspended in the lymph in a state of extreme comminution[1].

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[1] Magendie supposes that the lymph does not originate from the decomposition of the component parts of the body that had been previously organized, but that it consists of a certain part of the blood, which, instead of returning by the veins to the heart, is carried to this organ through the lymphatics and the thoracic duct. The great argument for this opinion is the uniformity in the properties of the lymph, and their analogy to those of the blood; Physiol. t. ii. p. 196, 7. Berzelius entertains a peculiar opinion respecting the origin of the lymph; he supposes that there is, in many parts of the body, a substance, which is composed of decayed animal matter, united to the lactic acid, and the lactate of soda, &c.: this is absorbed, carried to the blood, and discharged by the kidney: View of Animal Chemistry, p. 82. Chevreul examined the fluid taken from the lymphatics of a dog; he found it to contain nearly the same ingredients with the blood, but united to a much greater proportion of water; Magendie, Phys. t. ii. p. 171, 2.

Physiological writers, when treating upon this subject, have been frequently in the habit of employing expressions, which, we may presume, they must have intended to be taken in a metaphorical sense only, as when they speak of the solids being corroded by the lymphatics or broken down by them. But such a phraseology, it is evident, can amount to nothing more than an expression of the fact in different terms, and certainly affords us no insight into the mode in which it is effected.

This is one of those questions in which the metaphysical physiologists have brought forwards the operation of the vital principle, for the purpose of explaining the difficulty[2]. It is said that as long as a part possesses the vital principle, this agent enables it to resist the action of the absorbents, but that it immediately becomes subject to their influence when it loses this principle. We have, indeed, abundant evidence to show, that dead matter is more easily acted upon by the absorbents than the same matter while it retained its vitality; indeed we may go farther, and safely affirm, that no part can be absorbed until its texture is destroyed, and consequently until it is deprived of life. No substance can possibly enter the absorbents while it retains its aggregation, so that it necessarily follows, that the preliminary step to the absorption of a body is its decomposition. We are then to inquire how this is effected, and what connexion the means so employed have with the subsequent absorption of the decomposed matter.

We may conclude that the first step in this series of operations is the death of the part, by which expression is meant, in the present instance, that it is no longer under the influence of arterial action. It therefore ceases to receive the supply of matter which is essential to the support of all vital parts, and the process of decomposition necessarily commences. It would appear, however, to be a principle in the animal œconomy, that an organ remains under the influence of the absorbent system, after it ceases to be connected with the arterial. It therefore becomes subject to those causes which tend to its dissolution; but, as we may presume, before the operation is fully established, or while it occurs in each successive portion of which the whole is composed, the portion so changed is taken up by the lymphatics. There are various circumstances which may be conceived to be efficient in cutting off the ordinary supply by the arteries: a deficiency of nutritive matter in the system at large, a local disease or derangement of the arteries that belong to the organs, the obliteration of the arteries by pressure or any kind of mechanical obstruction, may be enumerated among the probable or possible causes which might produce the death of the part, at the same time that the absorbents connected with it may retain their activity[1].

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[1] Monro, (Sec.) in his Treatise on the Brain, c. 5, enumerates the arguments which have been generally adduced to prove the absorption of the solids. The most direct and unexceptionable of them are various morbid occurrences, in which tumours, or certain portions of the components of the body, are removed; the facts that are brought forwards are by no means of equal value, but, upon the whole, they may be regarded as amounting to a proof of the position. See also Blumenbach, § 436, and Bell's Anat. v. iv. p. 311, 2. Ribes remarks, that the absorption of the bones must be effected by the veins, because they are not furnished with lymphatics; Mém. Soc. d'Emul. t. viii. p. 621.

[2] Blumenbach, on this occasion, resolves the difficulty by the mysterious operation of his vita propria; Physiol. § 436. I have not been able to peruse the treatises of Albrecht, and of Ontyd, to which he refers.

And in many instances, without the intervention of any thing that can be properly styled morbid, the same kind of effect will be produced, although in a less degree. According to the principle of perpetual change, which pervades the whole organized system, we have the two operations, of accumulation, and of expenditure, always going forwards. In the state of the most perfect health and vigour, and when the body has attained its complete size and form, these operations should exactly balance each other; but any deviation from this exact balance, by which the relative action of the capillary arteries and the absorbents is affected, will destroy this equilibrium. The part of this process which is the most difficult to comprehend is the nature of the change, which the materials of the body undergo, when they cease to be under the influence of the arteries, and are converted into that state which adapts them for being taken up by the absorbents, whether the change be mechanical, consisting merely in the comminution of the substance, or chemical; and if the latter, what is the exact nature of the change, and how it is effected. These are points upon which I conceive that we are totally ignorant, so that I shall offer no conjecture upon the subject.

I have given an account above of the recent experiments of Magendie on the organs by which absorption is performed, and we are also indebted to the same physiologist for a new hypothesis respecting the mode in which the organs act. Having ascertained, by a previous train of experiments, the degree of effect which certain narcotic substances produce upon the system, so as to be able to refer to these as a standard of comparison, he was induced to examine how far the absorbing power of the vessels was promoted or retarded by the states of plethora or of depletion. The result seemed to be, that plethora uniformly retarded, and depletion as constantly promoted absorption, and hence he concludes, that it must consist in a mere mechanical action, independent of any principle connected with vitality. Proceeding upon this position, which, however, does not appear to be, in any respect, a necessary consequence of the premises, he conceives that the only physical action which can satisfactorily explain the phenomena, is that of capillary action exercised by the sides of the vessels upon the substances to which they are exposed[1].

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[1] See the observations of Sir C. Bell; Anat. v. iv. p. 311, 2.

So far Magendie's hypothesis may seem to agree with the one which is generally adopted, but upon farther investigation, it will be found to do so rather verbally than really, for we find that he does not suppose that the absorbed fluid enters by the open mouths of the vessels, but that it is imbibed by the substance of the vessel itself, or rather filters through its parietes, and that, when it has entered by this means, it is then carried forwards by the current of the fluid previously contained in the vessel. To prove the possibility of the hypothesis, he performed experiments on the veins shortly after death, and found that they were capable of imbibing and transmitting a sensible quantity of a fluid that was presented to them, and that the same also took place, although less readily, with respect to the arteries. To complete the proof of the hypothesis, Magendie endeavoured to execute a series of analogous experiments upon the vessels of a living animal. They consisted in taking a portion of a vein, the jugular, for example, in completely detaching it from its connexions of every kind, and dropping upon its external surface a solution of a deleterious substance, the effects of which were quickly manifested in all parts of the system[2]. But I may remark on this, as I have done on a former occasion, that without impeaching either the veracity or the address of the experimentalist, the operation appears to be of that complicated nature, which it would be impossible to perform so as to preclude all sources of inaccuracy, and those such as would materially interfere with the result.

With respect to the absorption of solids, it would follow from this view of the subject, and, indeed, it is admitted by Magendie to be the case, that nothing can enter the vessels by this kind of filtration, except what is perfectly dissolved in the fluids; he does not, however, inform us, in what manner the fluids which are contained in the vessels, or rather those that are contiguous to them, can effect this solution. Upon considering the doctrine of Magendie in all its parts, I cannot but regard it as both antecedently improbable and unsupported by facts or analogies, that it is very difficult to conceive how the various substances which are absorbed, whether by the lymphatics, or by the veins, can enter by this kind of filtration or transudation, while it affords no satisfactory explanation of the cause why some substances are taken up in preference to others, or of the mode in which the absorption of the solids can be accomplished[1].

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[1] "Parmi les conjectures que l'on pouvait se permettre à cet regard, celle qui ferait dépendre l'absorption de l'attraction capillaire des parois vasculaires, pour les matières absorbées, etait sans doute la plus probable." Journ. Physiol. t. i. p. 6.

[2] Journ. Physiol. t. i. p. 9, 0; Dict. Méd. et Chir. Prat. Art. "Absorption" t. i. p. 91 et seq.

The conclusions which were formed by Magendie upon this subject, have been more lately enforced by a series of elaborate and ingenious experiments, that have been executed by Fodéra. He commences by stating a number of facts, which tend to prove that the membranes generally, and the coats of vessels in particular, permit fluids to filter through them, and that this may take place before their texture or organization is visibly changed by the process of decomposition. But a more important and interesting part of his investigations refers to the power which the vessels of the living body possess, of allowing fluids to enter them by filtration; or, as he terms it, by imbibition.

In order to prove this point, he injected into two separate cavities of the body two fluids, which by their union produce a compound, the presence of which may be obviously and unequivocally indicated; and which could only be formed by the two ingredients coming into contact with each other. The cavities of the pleura and the peritoneum were selected for this purpose; into one the solution of the ferro-prussiate of potash, and into the other that of the sulphate of iron were injected, and the result was, that upon examining the body after a certain interval, many of the membranous and glandular parts, connected with the abdomen and thorax, were tinged with a blue colour. If the animal was left untouched, these phenomena took place slowly, but the operation was very considerably promoted by the application of galvanism. This effect was exhibited in an experiment analogous to one of Magendie's, in which a solution of ferro-prussiate of potash was enclosed in a portion of intestine, while a cloth, soaked in sulphate of iron, was placed on its external surface, when upon the transmission of the galvanic influence, the cloth was instantly stained of a blue colour. It was farther observed, that according to the direction of the current, the blue colour might be produced either on the outside or the inside of the intestine.

From a number of experiments of this nature, Fodéra comes to the following conclusions; 1. That exhalation and absorption may be referred to transudation and imbibition, through the pores of the membranous textures, "capillarité des tissus," which enter into the composition of the organs. 2. That this double effect may be produced in all parts of the body, and that the fluids which are imbibed may be conveyed equally by the lymphatics, or by the arteries and veins [1].

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[1] The doctrine of the transudation of fluids through the vessels during life, was maintained by many of the mechanical physiologists of the last century; it entered largely into the speculations of Kaau Boerhaave; see his treatise, de Perspiratione, c. 27. passim; it was a fundamental doctrine of Bordeu; Sur le Tissue muqueux, § 72; and is admitted by Haller; El. Phys. ii. 2. 23. Wm. Hunter very decidedly supported the same opinion; Medical Comment. c. 5; and Walter, ubi supra, § 28..35; an opinion very similar to it is adopted by Mascagni, ps. 1. sect. 1. p. 6, 7; and is maintained by his commentator Bellini, t. i. not. 4, p. 33..0. Cruikshank, on the contrary, endeavours to refute the hypothesis, and observes, that many of the appearances which have been observed upon dissection, and which have been thought to prove transudation during life, were in reality the effect of what occurred after death; On the Absorbent System, p. 11, 14.

"It must, I conceive, be admitted that these experiments go very far to prove that membranes, perhaps, even during life, and certainly after death, before their texture is visibly altered, have the power of permitting the transudation of certain fluids; but before we can assent to the doctrine of Fodéra, that absorption generally is effected in this manner, and that the chyle enters the lacteals simply by filtration, or imbibition through the coats of the vessels, there are many preliminary difficulties to be cleared away, and many additional circumstances to be ascertained, in order to complete the analogy. Although there may be a strong resemblance, or even a perfect identity between the chemical composition of a membrane and a vessel, their physiological texture, which, in the present case, is more particularly concerned, is probably very different. And, if the fluids can enter the vessels by this species of filtration, what reason can be given why they should not have an equal tendency to transude again out of the vessel? What cause can be assigned why they should enter the vessels at all, when there would appear so much easier a passage from one part of the cellular substance to some contiguous portion of the same texture?

Besides, when we reflect upon the nature of the experiment, in connexion with the inferences that are deduced from it, we find that it implies a degree of correctness in the execution, and of attention to a variety of concurrent circumstances, greater, perhaps, than ought to be ascribed to any physiologist, of whatever skill or dexterity he may be possessed. It requires that there should be not a single divided extremity of an artery, or of a vein, nor a single open cell of the membranous texture exposed to the fluid employed; for if any portion of it should enter into either of these organs, the essence of the experiment is destroyed. And, even granting that all these difficulties could be counteracted, still it would by no means follow, that because an active chemical solution can pass through a membrane, or the coat of a vessel, that the same could be done by the chyle. I feel, therefore, no hesitation in asserting that, curious and interesting as are the experiments of Fodéra, they do not prove the position which they profess to establish; and that they ought not to affect our ideas respecting the doctrine of absorption.

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[1] Recherches Expér. sur l'Absorption et l'Exhalation; see also Magendie, Journ. t. iii. p. 35 et seq.; this paper consists of an abstract of Fodéra's Mem. by Andral, jun. On this subject see the remarks of Prof. Tiedemann; Physiol. par Jourdan, § 168; Mr. Mayo, Physiol. (3d. ed., p. 97 et seq.; Sir D. Barry, Researches, p. 80..2; Dr. Elliotson, Physiol. p. 133; and the appendix to Dr. Hodgkin's Trans. of Edwards, p. 402 et seq.; this work contains much valuable information, especially on the opinions of the continental physiologists, on the various subjects connected with the function of absorption.

We are indebted to Sir D. Barry, whose observations on respiration I have already had occasion to refer to [1], for a series of experiments which he performed in order to prove, that absorption depends altogether upon atmospherical pressure. The experiments, which appear to have been sufficiently numerous, and to have been attended with very decisive results, consisted in introducing into a wound a portion of some poison, the effects of which had been previously ascertained, and to compare these with what took place when the pressure of the atmosphere was removed, by the application of an exhausted cupping-glass over the wound. The results appear to have been very remarkable; and, in a practical point of view, cannot fail to prove of great and obvious utility. The same dose of poison, which, under ordinary circumstances, destroyed an animal in a few seconds, was rendered completely harmless by the operation of the vacuum; and when the symptoms had commenced, and even when they had proceeded so far as to impress the spectators with the idea that the life of the animal was destroyed, still the vacuum had the effect of speedily and entirely removing them [2].

Important, however, as these experiments are in a practical point of view, there appear to me to be certain circumstances, which must be taken into account before we can admit of the hypothesis that is deduced from them. In the first place, it would seem that the poison was not simply laid upon the surface, but was inserted into a wound; hence it would be immediately mixed with the blood, and be carried by the veins to the central parts of the system. Now, it is obvious, that when a vacuum is formed over the divided end of a vessel, and especially of a vessel which is supposed to be passive, or to be influenced only by physical causes, the motion of the fluid through this vessel must be retarded. And this would be equally the case, upon whatever principle we supposed the fluid to be propelled, or carried through the vessel in question.

But it does not necessarily follow that this would be the case in the natural state of the parts, when the vessels remain entire, and the atmospheric pressure is exercised equally upon all the contiguous organs. Nor does it appear to be an obvious consequence of Sir D. Barry's experiments, that the same effect would ensue, if we had it in our power to apply a poisonous substance to the extremity of a lacteal or a lymphatic, provided as these vessels are with valves, and exercising a contractile power over their contents. The immediate effect would be the distention of the contiguous parts, and the dilatation of the vessel itself; but, provided the extremity of the vessel remained closed, this operation might promote, rather than retard, the progress of its contents.

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[1] P. 330.

[2] Exper. Researches, p. 2. "On Absorption;" the following are among the positions which he lays down; "That the whole function of external absorption is a physical effect of atmospheric pressure." "That the circulation in the absorbing vessels, and in the great veins, depends upon this same cause, in all animals possessing the power of contracting and dilating a cavity around that point, to which the centripetal current of their circulation is directed." He explicitly states his opinion, that vital action is not concerned in absorption:

In the last place, I conceive that it is altogether impossible to apply Sir D. Barry's principle to the action of the lacteals; they appear to be so far removed from the influence of atmospheric pressure, that we must suppose their contents to be propelled by some inherent power in the vessels themselves, or by some mechanism immediately connected with them; and presuming this to be the case, we have a very strong analogical argument for supposing, that the function of absorption, in the other parts of the system is conducted upon the same principle.

When we examine the extent of the lymphatic system, and endeavour to trace out its connexion with the various parts of the body, we observe that a great number of these vessels have their origin from the neighbourhood of the cutis[1], and it has therefore been supposed that absorption is carried on to a considerable extent by all parts of the surface. The doctrine of cutaneous absorption seemed to explain a great variety of phenomena in the animal œconomy, both physical and pathological, and was generally had recourse to as one of those operations, which had a powerful influence upon the functions of the body, both in their natural and their morbid condition. That under certain circumstances the absorbents are able to take up substances applied to the skin, especially when aided by friction, is sufficiently proved by the effect of various medical agents which are enabled by this means to enter into the circulation, and to act upon the system in the same manner as if they had been received into the stomach. Thus mercury applied to the surface produces its specific effect upon the salivary glands, and lead upon the muscular fibre, while opium, tobacco, and other narcotics, manifest their peculiar action upon the nervous system.

But besides this absorption of substances applied to the skin, and forced into the mouths of the vessels by friction or other mechanical means, it was an opinion very generally embraced by physiologists, that when the body is simply immersed in water, the cuticle still remaining entire, the same kind of absorption takes place, and even that the skin has the power of imbibing water from the atmosphere, when it exists there in any unusual quantity. This was the opinion of Sanctorius, and of all those who, since his time, performed experiments of a similar kind; the weight which the body was found to have gained, under certain circumstances, when this could not be explained by the excess of the sensible ingesta above the egesta, was attributed to cutaneous absorption. We are now, indeed, aware, that a part at least of what Sanctorius, and the statical experimentalists ascribed to cutaneous absorption, depends upon the action of the lungs, but still after this deduction, it was conceived that a certain proportion of this excess of weight could only be accounted for upon the supposition of the absorption by the skin [1].

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[1] We have a view of the cutaneous lymphatics, as far as they can be rendered visible by injections, in Haase, de Vas. Cut. et Intest. Absorb. tab. fig. 2; also in Mascagni, tab. 2. fig. 9..28. and tab. 3.

This doctrine was, however, some years ago, called in question by Seguin, who attempted to disprove it by a series of direct experiments, which were certainly ingenious, and would be almost decisive against it, were they not subject to certain causes of inaccuracy which it is not easy to obviate. The experiments consisted in immersing a part of the body in the solution of a saline substance that acts in a specific manner upon the system, which it is easy to recognize, as for example, corrosive sublimate. Now, we are informed, that provided the cuticle be entire, we have no evidence of any of the mercury being received into the system, or of any part of the salt being abstracted from the water [2]. Experiments tending to the same conclusion with those of Seguin's, although not so decisive in their nature and results, were performed by other physiologists. Currie carefully examined the weight of the body before and after immersion in the warm bath, but could not find that it gained any addition, and was hence led to conclude, that absorption never takes place by the skin, except when the substances applied to it are forced into the mouths of the absorbents by mechanical violence, or when an abrasion of the cuticle, or a destruction of some of the subjacent parts has taken place [3]. This opinion seems to have been generally acquiesced in by the modern physiologists, so that it has been assumed as a general fact, that except under the particular circumstances referred to above, the cutaneous absorbents were not capable of taking up substances that are merely applied to the external surface [4].

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[1] Mascagni, in p. 22, 3 of his great work, brings forward the various facts and arguments that have been adduced in favour of the doctrine of cutaneous absorption. See a popular view of the subject by Dr. Wilkinson; Med. Museum, v. ii. p. 117; also the article "Integuments," in Rees's Cyclopædia, and an essay by Dr. Kellie, in Ed. Med. Journ. v. i. p. 170 et seq.

[2] Fourcroy, Med. Eclair. t. iii. p. 232..241. and Ann. Chim. t. xx. p. 185 et seq.

[3] Med. Reports, ch. xix.

[4] Magendie, in conformity with the doctrine which he maintains respecting absorption generally, supposes that where we have evidence of this operation having been carried on at the surface of the body, the veins and not the lymphatics are the principal agents; Physiol. t. ii. p. 189..6. His observations on this point are ingenious and deserving of attention, but it appears to me to be the least tenable part of his hypothesis. A series of experiments were performed some years ago by Dr. Rousseau, of Pennsylvania, the object of which is to prove that cutaneous absorption has no existence, even under any circumstances, and that in all those cases where substances

This subject has been lately investigated by Dr. Edwards, with that skill and address which he has manifested on so many other points connected with the animal œconomy. The conclusion to which he leads us is, that the function of absorption is actually carried on by the skin to a considerable extent, and probably without interruption, although in different degrees, according to the condition of the animal, and the circumstances to which the body is exposed. He considers, under separate heads, the absorption which is effected by the skin, when the body is immersed in water, and when it is immersed in air. With respect to the case of absorption, while the body is immersed in water, the first experiments were performed on cold-blooded animals, and he appears to have proved unequivocally, that in them absorption takes place from the skin with great facility, and in considerable quantity. If a lizard be exposed for some time to a dry atmosphere, a great proportion of its fluids are removed by transudation, and if we then immerse a part only of its body in water, we may observe a visible and copious augmentation in the quantity of its fluids, both from the appearance which it presents to the eye, and, more decidedly, from the increase of its weight. He therefore infers that a similar operation must be carried on by the human skin, that both transudation and absorption are always going forwards, and that the body gains or loses in weight, according to the excess of the one above the other. Dr. Edwards remarks at some length upon the experiments of Seguin; he does not question their accuracy, but he conceives that we are not warranted in concluding from them that no absorption takes place because none was observed in these particular cases. He points out a variety of circumstances which may tend to diminish the absorption or to increase the transudation, so that the balance of effect shall be in favour of the latter operation, and thus, so far as the weight of the body is concerned, might seem to exclude the idea of absorption. He supposes that Seguin's experiments were performed under such circumstances, and that therefore the weight of the body was not increased, or was even diminished after immersion in the bath, an effect which would depend partly upon the absorption being retarded by a turgid state of the vessels, while the warmth of the medium would tend to promote the transudation[1].

With respect to the absorption by the surface, when the body is immersed in air, it is admitted to be less easily detected under these circumstances than in the former case; but Dr. Edwards

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applied to the surface are supposed to be absorbed by the vessels of the skin, the effect is in reality to be referred to the substance being converted into vapour and inhaled by the lungs; he even pushes his hypothesis so far as to suppose, that when mercurial frictions are applied to the surface, the heat of the body volatilizes the metal, and in this way enables it to enter into the system by pulmonary inhalation; see account of the experiments by Dr. Stock, in Ed. Med. Journ. v. ii. p. 10 et seq.

[1] De l'Influence, &c. ch. xii. p. 345 et seq.

conceives that his experiments, especially a series which he performed on guinea pigs, warrant the opinion, that absorption does take place, although in considerably less quantity than during immersion in water; and he finally concludes, that when the body loses weight during immersion, in damp air, the amount is to be regarded as the difference between the loss by transudation, and the increase in consequence of the absorption of aqueous vapour[1].

SECT. 4. *Connexion between Absorption and the other Functions.*

Having now considered in succession the functions of digestion and of absorption, by which the aliment is first reduced to the state which is adapted to nutrition, and is afterwards carried into the circulating system, it remains for me to offer a few observations upon the changes which it undergoes from the time that it enters the absorbents until it is finally deposited in the different organs of which it is destined to form a constituent part. We have seen that the chyle, when it is received into the lacteals, both in its chemical and its physical properties, exhibits a considerable resemblance to the blood: the question then for us to determine is, whether, after it is poured into the veins in the imperfect state, in which the process of sanguification appears to be only in part accomplished, it is, in the first instance, mixed with, or diffused through the whole mass of blood, and that all the remaining changes are effected by the ordinary operations of secretion and excretion, or whether there be any specific process for the more complete assimilation of the chyle, by which it may be, in the first instance, converted into one or more of the constituents of the blood.

That the function of respiration, in some measure, contributes to assimilation is at least a probable opinion, and we may also suppose, that the consequence of the transudation which is carried on from all the surfaces of the body may have its effect in discharging any redundant quantity of water; but, except these, we are not acquainted with any processes which can affect the constitution of the blood, while it remains in the larger vessels. We know, however, that it is principally in these vessels that it acquires its characteristic properties, more especially that the fibrin obtains its peculiar texture and its power of spontaneous coagulation, which existed in a less perfect degree at least in the chyle, and this is still more remarkably the case with the colouring matter. It appears to be pretty well established, that the substance which gives the red colour to the blood is the vesicle that surrounds the globule; it may also be laid down as a point which is generally admitted by physiologists, that although this substance contains iron, yet that the iron is not the immediate cause of the red colour. We have likewise reason to conjecture that it is through the medium of this colouring matter that the oxygen of the atmosphere more particularly manifests its action, when the blood is transmitted through the lungs, and is converted from the venous to the arterial state; yet this seems to afford us no insight into the mode in which the red colour is, in the first instance, produced, why it exists in a slight degree in the chyle, and why it is found so much more copiously in the blood. We may conclude, that it is not the consequence of mere concentration, nor are we able to refer it to any of those operations which are known to be going forwards in the animal œconomy, with the nature of which we are so well acquainted, as to allow us to speculate upon their ultimate effects.

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[1] De l'Influence &c., c. xiii. p. 556 et seq. See on this subject Magendie, Physiol. t. ii. p. 189..196, and Dict. Méd. et Chir. prat. art. "Absorption," where he endeavours to prove, that it is the veins and not the lymphatics which are the agents in cutaneous absorption; also the remarks of Adelon, Physiol. t. iii. p. 10 et seq., and art. "Absorption," Dict. de Méd. t. i. p. 124 *et seq.*; and of Walter, "Sur le Resorption," p. 25.

When we consider the nature of the relation which exists between the sanguiferous and the absorbent systems, we appear to be warranted in supposing that the fluid which is contained in the arteries should be regarded as constituting the blood in its most perfect state; that it is then carried by the capillary vessels into all parts of the body, where it loses a certain quantity of its constituents, and is thus reduced to the state of venous blood, that the thoracic duct pours into the great veins the materials necessary to supply the loss which it has experienced, and that some change, either chemical or mechanical, is effected in the lungs, which again converts it into the state of arterial blood. According to this view of the subject, the process of the conversion of venous into arterial blood, will consist in the addition of a quantity of carbon, hydrogen, oxygen, and nitrogen; in passing through the lungs a portion of carbon, oxygen, and hydrogen, are separated, under the form of carbonic acid and water[1], while in the course of the circulation, we may presume, that the excess of nitrogen, which must be thus produced, will be separated from the blood, in the form of muscular fibre or of membrane, or still more, of urea, of which nitrogen composes so large a proportion.

It only remains to inquire into the relation which subsists between the absorbent and the nervous systems. And this, we have every reason to suppose, is of that indirect nature only, which I have had occasion to describe when treating of the action of the heart, where the function of the part is not necessarily dependent upon the power of the nerves, although there may be many cases in which it is materially influenced by this power. It appears from the examinations of the anatomists that there are but few nerves sent to the absorbent system, and that even these few seem rather to pass by them, in order to be transmitted to more distant organs, than to be ultimately destined for the lymphatic vessels or glands themselves. The mode of action of the absorbents (at least if we except that of their mouths, which is altogether involved in obscurity) is of that kind which may be explained without the aid of nervous sensibility, and, indeed, every circumstance connected with them seems to show, that, like the sanguiferous system, their ordinary operations are to be referred to contractility alone.

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[1] It is scarcely necessary to remark, that the water which is exhaled from the lungs is not supposed to be generated in this organ, but merely abstracted from the pulmonary blood by a species of secretion.

CHAPTER XII.

OF GENERATION.

THE functions that have hitherto passed under our review are supposed to depend essentially upon the contractility of the muscular fibre, by which either a mechanical or a chemical change is produced, the first constituting the direct, and the second the indirect effect of this property[1]. These functions would appear to embrace all that is absolutely necessary for the support and continuance of animal life, and probably to be each of them, to a certain extent, essential to its existence. There are, indeed, certain species of animals which may seem to afford an exception to this remark. In some cases the minuteness of the object scarcely permits us to acquire any satisfactory information concerning the œconomy of the individual; and there are others, where the organization is so imperfect, as to prevent us from forming any direct comparison with the more complicated animals.

And yet even here we have a certain analogy, which may guide us in our investigations. Although many of the inferior orders have no circulating system[2], by which the fluids are progressively carried to the different parts of the body, yet we may conclude that they are deposited in appropriate cavities or reservoirs, from which they are taken up, as occasion may require, so as to afford each organ its due supply of nutrition. Although not possessed of lungs, by which to inhale the air, and thus bring it into proximity with the blood or other analogous substance, yet we find that the same chemical change is produced by the external surface of these animals, as by the lungs of the vertebrata. It appears probable that all animals possess some power of regulating their temperature, although this power exists in a very small degree only in many of the lower orders. It is obvious that every organized being must possess a power equivalent to digestion, that the formation of their different parts and textures can only be performed by the power of secretion, and that a species of absorption must be the mode in which they appropriate to themselves the materials of which they are composed.

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[1] Certain objections, which I admit are not without considerable force, independent of the respectability of the sources whence they proceed, have been urged against the designation of *contractile*, which I have assigned to the first class of functions. Although I think it would not be difficult to prove that, at least in the higher orders of animals, muscular contraction is the first step in the process, yet, to avoid all discussions of a merely verbal nature, we may substitute the term *physical*, which is indicative of their obvious and essential effects.

[2] Blumenbach, Comp. Anat. by Lawrence, c. xii. § 166, 7, note F; Cuvier, Leç. d'Anat. Comp. No. 24. t. iv. p. 188; Lamarck, Anim. sans Vert. t. i. p. 149, 360; t. iii. p. 248.

The next order of functions, those which depend upon the operation of the nervous system, are much more confined in their existence and more limited in their operation. None of the functions which have been enumerated above are necessarily connected with a nervous system, and accordingly there are numerous tribes of animals in which no nerves have been detected. The presence of this system invests the animal frame with a set of powers and actions that are essentially different from those that depend upon contractility. They do not necessarily produce either a mechanical or a chemical change. The mode in which they act is indeed altogether beyond our comprehension, and the effects which ensue from this action are not referable to any other of the known operations of physical agents. Still, however, we are enabled to observe the operation of cause and effect with as much minuteness in the action of the nervous as of the muscular system; and by observation and experiment we can, in most cases, predict with sufficient accuracy the results of the impressions of various kinds which are made upon the brain and nerves.

But before we enter upon the consideration of this class of vital operations, it will be proper to inquire into the mysterious phenomena of generation, a function which may be regarded as, in some measure, intermediate between the other classes; for although it can scarcely be referred to any modification of contractility, yet it is not essentially connected with nervous action, as it exists in animals that are without a nervous system, as well as in those that are the most amply provided with it.

In treating upon this subject, I shall divide my remarks into three heads. After a few observations upon the anatomical structure and physical properties of the generative organs, I shall, in the second place, inquire into the part which the sexes respectively perform; and lastly, I shall examine the nature of the process, and give a brief sketch of some of the most celebrated hypotheses that have been adopted to explain this function.

SECT. 1. *Remarks on the Structure of the Generative Organs.*

There is no function which exhibits a greater variety in the comparative anatomy of the parts that are subservient to it than the one now under consideration. But all the warm-blooded vertebrated animals agree in the essential particular of there being two sexes, the congress of which is necessary for the completion of the process; the one furnishing a peculiar secretion, and the other an appropriate receptacle in which the secretion may be deposited[1]. The male consequently possesses an apparatus necessary for the formation of this secretion, and a duct by which it may be afterwards conveyed into the body of the female, while the latter is provided with a cavity into which the secretion is received, and in addition to this, with an organ of a more complicated structure, the functions of which are perhaps not in all respects thoroughly understood, where the rudiments of the young animal are originally formed, and upon which the male secretion produces its specific action.

For one of the earliest descriptions of the male generative organs, which contains a correct account of the minute anatomy of the parts, and of their relation to each other, we are indebted to De Graaf. A point upon which he insisted, and which he elucidated with more accuracy than had been done before his time, is the complete vascularity of the testis. The most distinguished authors who had preceded him conceived that a large part of this organ consisted of what they termed a glandular or medullary substance, while De Graaf, on the contrary, states his opinion, that the testis is merely an assemblage of very minute vessels which elaborate the peculiar secretion[2]. These are described as being folded up, as it were, into regular bundles or lobules, which do not anastomose with each other, and which may be drawn out into single vessels of a very extraordinary length. Their diameter has been stated to be no more than $\frac{1}{200}$ of an inch, while it has been estimated that the total length of the vessels which compose one of the testes amounts to more than 500 feet[1].

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[1] In all the vertebrated animals, with a few exceptions which appear to be incidental, and, as it were, anomalous varieties (see Phil. Trans. for 1823, pl. 15, 20) the sexes are distinct. Some of the mollusca possess both sets of organs, and require no co-operation; while in others, where both sets of organs are present, mutual congress is necessary, each animal impregnating the other; Haller, El. Phys. xxix. 1. 1..6; Lawrence in Blumenbach's Comp. Anat. note K. in ch. xxiv. p. 460, 1; Cuvier, Leçons d'Anat. Comp. No. 29, sect. 4. t. iv. p. 164 et seq.; Milne Edwards, art. "Annelida," Cyc. Anat. v. i. p. 171. We are informed that there are certain of the microscopical animals which require the co-operation of three individuals; Senebier, Introd. to Spallanzani, Opusc. de Phys. p. lxxvi. There are large classes of the inferior animals, which are propagated by simple division, or by the separation of a smaller body from the larger one of the parent, without the intervention of any thing analogous to sexual congress; but the mode of their propagation bears no analogy to the function of generation as exercised by the more complicated animals; Blumenbach's Comp. Anat. by Lawrence, ch. xxiv. note K. p. 461. The mode of generation in the lowest classes of animals may be traced in the various parts of Lamarck's Work, Anim. sans Vertèb. t. i. p. 404, 433; t. ii. p. 8, 21, 31, 46, 69, 213, 407; t. iii. p. 61, 67, 73, 141, 159, 197, 207, 237, 245, 274..7; t. iv. p. 85, 94 et alibi. See also Dr. Roget's Bridgewater Treatise, pt. 4. ch. 1. We may remark, that the perfection of the function of generation, or the analogy which it bears to the same function in the higher orders, does not proceed in a regular progression with the other functions of the invertebrated animals.

[2] De Vir. Org. Gen. inserv. p. 55. tab. 1..4; this treatise, which was published in 1668, and which seems to have been composed from actual observation, contains an ample account of what had been done by his predecessors. His plates, although not executed with the elegance of modern engravings, and partaking, perhaps, in some cases, rather of the nature of plans, than of actual views of the parts, are characteristic and expressive. The anatomists, whose descriptions of the testis he particularly contrasts with his own, are Galen, Riolan, Fallopius, Spigelius, and Vesling, with others of less note. For figures of the testis, see also Bidloo, Anat. Hum. Corp. tab. 45..7; and Albinus, Acad. Annot. lib. 2. tab. 7. fig. 1, 2, 3; and tab. 3, fig. 1. On the subject of De Graaf's discoveries the remarks of Bell may be consulted;

Besides the testis, the vesiculæ seminales, both from their size and their situation, have been supposed to perform some important part in the function of generation, although it has been difficult to ascertain the exact nature of the purpose which they serve. The opinion formerly entertained was, that they are merely reservoirs in which the semen is deposited as it is secreted.

In consequence, however, of the observations of Hunter, who remarked that the fluid contained in these cavities appeared to be different from that found in the testis, many of the later anatomists have supposed that the vesiculæ seminales produced a secretion of a peculiar nature, the use of which may probably be to dilute the semen or to add to its bulk[2].

The principal physiological point connected with the male organs, that requires our attention, respects the mode in which the secretion is produced. It might be supposed that a gland capable of secreting a fluid, endowed with such extraordinary

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Anat. v. iv. p. 191 et seq.; his 2d, 3d, 4th, and 5th plates, also the plan in p. 195, exhibit good views of the testis and the parts connected with it.

[1] Haller, El. Phys. xxvii. 1. 16, adopts the opinion of De Graaf, and remarks, that except these lobules and the cellular texture which separates them from each other, the testis contains "nihil ultra et neque aliud parenchyma, neque glandulosi quid." See also Boerhaave, Prælect. t. v. part 1. p. 137. § 644, cum notis; Haller, "De Vas Sem. Obs." cum Tab. Op. Min. t. ii. p. 1 et seq.; also a paper of Haller's in Phil. Trans. for 1750, No. 494, p. 340 et seq. For the diameter and length of these vessels, see Monro de Test. et Sem.; Blumenbach, Inst. Phys. § 523, p. 278. Monro, (tert.) in his Outlines, v. iii. p. 46. fig. 39. and also in his Elements, v. ii. p. 179 et seq., gives an account of the testis, principally from his father's observations. See also Bichat, Anat. Des. t. v. p. 188, 9; for figures of the part, see Cowper, Anat. Corp. Hum. tab. 45, 6, and Caldani, Icones Anat. pl. 131.

[2] See Hunter on the Animal Œconomy, p. 31 et seq. Haller; El. Phys. xxvii. 1. 23..6; gives a list of animals who are provided with this organ, and of those who are without it; he also enumerates many in whom there is no direct communication between its duct and the vas deferens. This circumstance appears to afford a strong anatomical argument against the opinion of the vesicle being merely a reservoir for the semen; the case of the gall bladder, which Haller supposes to be analogous, scarcely applies to this part. M. Magendie, however, still adheres to the old opinion; t. ii. p. 406; and the same is the case with Blumenbach; Inst. Phys. § 529 cum notis; and with Sœmmering; Bibl. Med. t. iii. p. 87; as referred to by Blumenbach; see also Dr. Elliotson's judicious observations in note H. But whatever opinion we may form on this point, it is obvious that the testis is the only essential organ, the seminal vesicle being frequently absent. With respect to the animals in which this organ is wanting, besides the above reference to Haller, see Blumenbach's Comp. Anat. by Lawrence, § 315, and note D; Cuvier, Leç. d'Anat. Comp. No. 29. t. v. p. 29..41. For figures of this organ, see Albinus, Acad. Annot. lib. 4. tab. 3. fig. 1, 2; Cowper, Anat. Corp. Hum. tab. 47. fig. 2; Caldani, Icones Anat. pl. 131. fig. 13, 14, 15.

property as that derived from the testis, would exhibit something peculiar in its structure, or what would lead to some indication of the means by which it acquired its specific properties. But we are scarcely entitled to say that this is the case. We find, indeed, that the arteries which are sent to the testis exhibit the peculiarity of being of an unusual length in proportion to their diameter, and of pursuing a singularly tortuous course[1] (a circumstance which we have seen above is much more remarkably the case with the seminal ducts themselves) so that the blood must necessarily pass very slowly along them, but we do not perceive any other circumstance in which they differ from the ordinary state of the vessels which supply the secreting organs. We are then led to inquire, what is the precise change which would be induced upon the blood, or any of its components, by being slowly propelled through a long narrow tube. We may conceive that it would be separated into its proximate principles, that only the more fluid or soluble ingredients would be carried forwards, while there would be sufficient opportunity for all the changes to be effected which arise from the action of these principles upon each other. Such considerations do not, however, tend to throw any light upon the action of the spermatic vessels, nor is the result of their action what might perhaps have been previously expected from the operation of the cause.

The semen, as I have already had occasion to remark, belongs to the class of mucous secretions; it has, however, some properties of a specific nature, which are unlike what we meet with in any other of the animal fluids. Its odour is specific, the fibrous substance which it contains, and the changes which it undergoes by its spontaneous decomposition are peculiar to itself, while we cannot perceive any relation between these circumstances and the mode in which the fluid is secreted, or the purposes which it afterwards serves in the animal œconomy[2].

An observation was made about the middle of the 17th century, which is in itself a very curious matter of fact, and which gave rise to an almost infinite number of experiments and speculations; I allude to Leeuwenhoek's discovery of the existence of animalcules in the semen[3]. So extraordinary a fact, and more especially as taken in connexion with the peculiar properties of the fluid, could not fail to excite the greatest interest, and accordingly the observations were repeated by many physiologists both at that period, and among our contemporaries. As is frequently the case on those points which depend upon the evidence of the microscope, various opinions were advanced respecting the appearance and properties of these animalcules. Their existence, however, was generally admitted by those physiologists whose authority is, on all accounts, the most to be relied on, and even those who opposed Leeuwenhoek differed from him rather as to the nature of the

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[1] Haller, El. Phys. xxvii. 1. 10, 1; Blumenbach, Inst. Phys. § 509. From the mechanical structure of these vessels, Keill estimates, that the blood must move in them 150 times slower than if they had been disposed in the ordinary manner; Essays, p. 153; we may conclude that his calculation is not without some foundation, although probably the effect is considerably overrated.

[2] Haller, El. Phys. xxvii. 2. 11..16; Fourcroy and Vauquelin, Ann. Chim. t. ix. p. 64 et seq.

[3] I have spoken of this as the discovery of Leeuwenhoek, in conformity with the common opinion; although there is sufficient reason to believe that the observation was first made by Hamme. Leeuwenhoek informs us, that this was actually the case, but we may conceive that the fact would have

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passed almost unnoticed, had it not been brought so fully before the public by his zeal and activity. See Haller in not. 1. ad Boer. Prael. § 651; and El. Phys. xxvii. 2. 3. The discovery was also claimed by Hartsoeker, Essay de Diop. Art. 88. p. 227, but apparently with little justice, for it appears pretty evident, that, even if he saw these bodies, he was not aware of their peculiar nature, until it had been ascertained by Leeuwenhoek. In a letter to Van Zoelen, dated Dec. 17, 1698, Op. t. iii. p. 57 et seq., Leeuwenhoek refers to the claim that had been just made by Hartsoeker, that he had discovered the animals twenty years before, and had published an account about the same time in the Journ. des Sçavans, No. 30, for 1678. This claim led Leeuwenhoek to give a full account of the transaction with Hamme, from which it appears that Hamme showed the bodies to Leeuwenhoek the year previous to their alleged discovery by Hartsoeker, and that Leeuwenhoek immediately transmitted an account of them in a letter to Brouncker, President of the Royal Society, dated Nov. 1677; see Phil. Trans. v. xii. No. 142. p. 1040 et seq. Leeuwenhoek refers to the same subject, Op. t. iii. p. 285, and t. iv. p. 169, in which passages he speaks of Hartsoeker with a degree of severity which is not usual with him; his general habit was to state his opinions with great moderation. See also Valisneri, Op. t. ii. p. 102; Senebier, Introd. to Spallanzani, Opusc. de Phys. p. xli. In Haller's Bibl. Anat. t. i. p. 663, 4, we have a list of Hartsoeker's works, with their respective dates; from this it appears that the animalcules were first mentioned by this writer in the Journ. des Sçav. for 1678; in p. 371 et seq. we have an article by the editor, containing an account that had been transmitted to him from Hartsoeker, of his having observed these animalcules; but the fact is simply stated without any comment. There is, however, a reference to a previous account that had been given in a former part of the same volume. This is contained in a letter from Huygens to the editor, in which, speaking of a new microscope that had been lately brought from Holland, he alludes to the discovery, but without naming the discoverer, p. 345..7. The passage in the "Essay de Dioptrique," in which Hartsoeker speaks of the discovery is as follows: "Il y a plus de vingt ans que j'examinai le premier, à ce que je crois, la semence des animaux avec des microscopes, et que je découvris qu'elle est remplie d'une infinité d'animaux semblables à des grenouilles naissantes, comme je le fit mettre dans le 30me Journal des Sçavans de l'Année 1678—," p. 227. This work was published in 1694. Haller says, that Hartsoeker gave a more full account of the animals in Journ. des Sçav. for 1695, but this paper I could not find. In his "Suite des Conjectures Physiques," published in 1708, he speaks of the animals, but without claiming the discovery or alluding to the controversy to which it had given rise; Dis. 7. p. 105..7; this was ten years after Leeuwenhoek's letter to Van Zoelen. In the "Cours de Physique," published in 1730, Hartsoeker enters fully into the subject, claims the discovery, as having been made by himself in 1674, and bitterly complains of Leeuwenhoek's injustice towards him.

bodies, and their supposed operation in the animal œconomy, than with respect to their actual existence [1].

But notwithstanding the weight of evidence which was brought forwards, a degree of doubt still remained in the minds of many physiologists as to the specific nature of these animalcules [2]. It seemed impossible not to admit, that living bodies were present, but it was contended that they were merely a species of the animalcula infusoria, those minute beings, which, by the aid of the microscope, may be detected in all fluids that are impregnated with any animal or vegetable substance, and which, it may be presumed, are produced from ova, that are constantly floating in the atmosphere, and which make their appearance in all situations that are favourable to their evolution and subsistence [3].

It was to remove these doubts that the researches of Spallanzani, and still more lately those of Prevost and Dumas were particularly directed. The first of these naturalists applied himself, with his accustomed zeal and industry, to the refutation of the hypothesis of generation which had been proposed by Buffon and Needham. In the prosecution of this object he was led to inquire into the nature and appearance of the infusory animalcules, and to compare them with those bodies which are found in the seminal fluid. He has detailed his observations on both these points, with what may probably be thought an unnecessary degree of prolixity, but in such a manner as to impress the reader with a full conviction of his accuracy. The general result is, that the observations of Leeuwenhoek are, on every essential point, confirmed by those of Spallanzani, that the seminal animalcules have a definite figure, which it would seem not difficult to recognize, and that they are obviously different from the animalcules that are found in infusions[1].

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[1] This was particularly the case with Buffon, whose remarks and observations will be noticed below, as involving a peculiar hypothesis of generation.

[2] We find that Linnæus was one of those who discredited the observations of Leeuwenhoek. A thesis was published under his presidency in the year 1746, in which the author supports the opinion, that the bodies observed in the seminal fluid are not independent animals, but merely inert particles, which are set in motion by some physical cause. Several eminent names are brought forwards, who are said to countenance this doctrine, and Lieberkuhn is supposed to have proved it by his microscopical observations; see Wahlbom's Diss. "Sponsalia Plantarum," in Amœnitates Acad. t. i. p. 79; also Spallanzani, Opusc. de Phys. t. ii. p. 131, 2.

[3] This was the case with Tuberville Needham, a naturalist, whose accuracy and candour give his opinions every claim to our respectful attention. In several parts of his "new microscopical discoveries," published in 1745, he refers to the observations of Leeuwenhoek, and endeavours to show, that what he saw were not proper animalcules, but certain inanimate bodies, which had a mere mechanical motion; p. 56..9; 60..2; 82..4. See also the corresponding passages in the translation of the above work, "Nouvelles Observations Microscopiques;" p. 65..0; 71..4; 97..2; with the translator's remarks and Needham's reply. In his letter to Folkes, written in 1748, which is subjoined to the latter work, he admits the bodies in question to be animals, but does not suppose them to have any specific relation to the semen; he merely regards them as belonging to the class of infusory animalcules; Nouv. Obs. Micr. p. 183, 212..4, 242. It will be found that in his replies to the translator's notes, Needham considerably modifies the opinions maintained in the text: the translation was published in 1750.

The observations of Prevost and Dumas appear to have been made with the greatest care, and are related with minuteness. They examined the spermatic fluid of various animals from the different orders of the mammalia, birds, and those with cold blood, and they found the animalcules as described by Leeuwenhoek and by Spallanzani, so as to leave no doubt of their existence, as specifically belonging to the secretion of the testis[2].

The semen, whether prepared in the testis alone, or by the conjoined operation of the testis and the vesiculæ seminales, is so disposed, that by the excitement of the termination of the excretory duct, it is projected with considerable force into the body of the female[3]. The excretion of this fluid may be supposed to obey the same laws which influence the other analogous organs of the animal body, but it is so far peculiar, as being connected with a specific and very powerful nervous sensation. This constitutes the most urgent of what are termed the appetites or passions; in the lower tribes of animals, it would appear to be altogether instinctive, and in the human race, if not instinctive, is so violent in its operation, as frequently to counteract every other feeling both of a physical and a moral nature[1].

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[1] Spallanzani's observations are detailed at full length in his volumes entitled "Opuscules de Physique," as translated and edited by Sennebier; the first volume is principally occupied with an account of the different infusory animalcules, as they are found in various animal and vegetable fluids. The account of the seminal animalcules is contained in the second volume, ch. i. p. 90 et seq. tab. 3. fig. 1, 5; in the following chapter, p. 122 et seq., he compares his observations with those of Leeuwenhoek, and notices their complete coincidence. We have various observations in the remaining chapters, which prove the independent existence of the seminal animalcules, and at the same time their distinction from the infusory animals.

[2] Mémoires de la Societé de Physique de Geneve, t. i. p. 180 et seq. pl. 1, 2. The authors afterwards published an account of their observations in the Ann. des Sciences Naturelles, in a series of papers contained in the first and second volume of that work. See the remarks of Adelon, art. "Génération," Dict. Méd. t. x. p. 215..9; of Dalyell, art. "Animalcules," Brewster's Encyc.; of Bory St. Vincent, in Magendie's Journ. t. iv. p. 160 et seq., and in Dict. Class. d'Hist. Nat. t. iii. p. 356, who names them Zoospermes; and of Raspail, § 960 et seq. Mr. Owen, in his new arrangement of the Entozoa, designates them "Cercaria Seminis, cui locus semen virile"; Proc. of Zool. Soc. No. 29, p. 75.

[3] The congress of the sexes, attended with the entrance of the male, takes place only in the two first classes of the vertebrated animals; in the amphibia there is the congress without the entrance, while in fishes there is neither congress nor entrance. Among the invertebrated animals the action is accomplished in various ways, which bear no exact relation to the other functions, or to the circumstances upon which their classification is founded; Blumenbach's Comp. Anat. c. 23, 24, with Mr. Lawrence's notes; Cuvier, Leç. d'Anat. Comp. No. 29, t. v. p. 164, 5; see also Adelon, Physiol. t. iv. sub init., for the gradation of the generative organs. In these cases the excitement of the termination of the excretory duct must be effected in a different mode from what it is in the former. In the change of form in the male organs, we have one of the most remarkable examples of that structure which has been termed by anatomists erectile. The final cause of the change in the figure of the corpora cavernosa is sufficiently obvious, and it has been generally supposed that the immediate physical cause consists in the injection of blood into a series of membranous cells of which the body is composed, produced by preventing the return of the blood through the veins. It is not, however, very easy to explain the way in which the various mental and

The female organs are necessarily much more complicated and elaborate than the male. The part which the male bears in generation is quickly accomplished, whereas the female has to undergo a series of operations, which occupy a great length of time, and are connected with the most important changes in the animal œconomy. There are two orders of parts, upon each of which it will be necessary to offer a few observations; the

physical causes operate so as to produce the injection, nor how, upon the cessation of these causes, the removal of the blood is so quickly effected; see Haller, El. Phys. xxvii. 3. 7. 10, 11. Anatomists have differed in opinion respecting the muscularity of the urethra; Haller thought that it was not a muscular organ, El. Phys. xxvii. 3. 31, and Sir C. Bell coincides with him, Anat. t. iv. p. 17. Prof. Monro informs us that his father conceived it to be muscular, Elem. v. ii. p. 171. See farther remarks on this subject by Prof. Monro, in the Edin. Journ. Med. Scien. v. ii. p. 8, 9. Sir E. Home is of the same opinion, and Mr. Bauer supposes that he has been able to demonstrate the muscular fibres; Phil. Trans. for 1820, p. 183 et seq. pl. 22, 3. Hunter supposes that the cells of the corpora cavernosa are muscular, Anim. Œcon. note in p. 43. Moreschi has attempted to prove that the part is entirely vascular, not, as had been generally supposed, cavernous or cellular; whatever opinion we may be induced to form upon this point his elaborate plates may be studied with advantage; Comment. de Ureth. Corp. Struct. See the art. "Erectile," by Béclard, Dict. Méd. t. viii. p. 257. I have been favoured by Mr. Kiernan with the following communication respecting the anatomical structure of the blood vessels of these parts, which he has kindly allowed me to insert in my work. "The erectile tissue of the penis is still described in some modern works on anatomy as being composed of cells, placed between the arteries and veins. It appears, however, from the researches of Hunter and Moreschi on the corpus spongiosum, and from those of Mascagni, Cuvier, Tiedemann, and Müller, on both the corpora cavernosa and spongiosum, that these supposed cells are veins, resembling in structure the other veins of the body, but which, from their free anastomoses, have the appearance of cells; the two bodies differing from each other only in the dimensions of the veins, which are larger in the cavernous than in the spongy body. The arteries ramify in the interstices of the veins, but the precise mode of their termination has not yet been satisfactorily ascertained. Müller, who has given a very minute description of these arteries, says that some of them (those to which, from their resemblance to the tendrils of the vine, he has given the name of Arteriæ Helicinæ) project into the interior of the veins, and pour their blood directly into them. Müller considers the Arteriæ Helicinæ as the principal agents in the erection of the penis."

[1] So far as we can form an opinion from a single case, it would appear, that in the human subject this passion is not, strictly speaking, instinctive; Edin. Trans. v. vii. p. 67.

organ on which the seminal fluid acts, so as to produce or evolve the fœtus, and the organ which is destined for the reception and nutrition of the fœtus after it has been so produced or evolved. The former of these constitutes what is termed the ovarium, and the latter the uterus.

In all animals that possess different sexes, the female is provided with a glandular body, containing a number of cells or vesicles, from which the substance proceeds, that is either converted into a fœtus, or serves to compose its basis or substratum. The way in which this is accomplished will more particularly fall under our consideration in the next section; at present it will be sufficient to remark, that the ovary is usually situated at some distance from the part into which the semen is introduced, and, as might be supposed from this circumstance, out of its immediate or direct influence. In the mammalia, the fœtus, when thus elaborated, is deposited in the uterus, to a portion of the interior surface of which it attaches itself, by a very curious and almost inexplicable process, where it remains fixed for a certain period, until its organs and functions shall have been sufficiently matured to enable it to resist external violence and to provide for its own support. From the mode in which animals of this description bring forth their young, they are named viviparous. Birds, on the contrary, and a large part of the cold-blooded quadrupeds[1], compose the class of oviparous animals; here the fœtus is not transmitted into any cavity analogous to the uterus, but after being provided with the necessary protection from external injury, and a due supply of matter for its growth and nourishment, is immediately discharged from the body of the mother.

The nature of the female ovarium, and its connexion with the other parts of the uterine system, have been the subject of very minute anatomical examination. The name of ovarium is said to have been first applied to it by Steno, in consequence of the analogy which it was conceived to bear to the organ, from which the eggs of the oviparous animals are produced[1]. De Graaf appears, however, to have been the first writer who ascertained the actual structure of this organ, and who pointed out, with at least much more accuracy than those who had preceded him, the changes which the several parts undergo in the different periods of impregnation. He describes the ovarium as composed of a particular kind of cellular texture, in which are contained a number of vesicles, filled with an albuminous fluid, each of which is capable of producing a fœtus by the application of the male secretion, through the intervention of a process, which will be more particularly described hereafter[2].

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[1] Spallanzani indeed informs us, that his observations led him to conclude, that what had been termed the ova of the cold-blooded quadrupeds are, many of them, not properly entitled to this appellation. The substance that is discharged from the female is merely the fœtus moulded into a round form, and is not enclosed in any shell or other extraneous body from which it is afterwards evolved. The objection appears, to a certain extent, to be well founded; but it may be remarked, that it applies rather to the anatomical structure of the part than to its physiological relations; See his Experiences sur la Génér. by Senebier, c. 1 et alibi. Harvey, on the contrary, contends that all animals proceed from ova; Exer. de Gen. 1; the difference of opinion evidently depends upon the different sense in which that term is used, the first being derived from its anatomical structure, the second from its physiological relations. Many eminent authors, as Fabricius and Harvey, speak of the uterus of oviparous animals, but this term can scarcely be admitted even as analogically correct. I may remark, that the sense in which Harvey employs the word ovum is so general as to reduce it to little more than a verbal analogy; see Exer. 62. p. 287; Exer. 63. p. 292 et alibi.

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[1] Myolog. Spec. p. 145. It would appear, indeed, that anatomists before the time of Steno had conceived an analogy between the organs of viviparous and oviparous animals, but that he was the first who clearly expressed the opinion and applied the term to the former class; see Boer. Præl. § 699, 0. cum notis; Haller, El. Phys. xxviii. 2. 33.

[2] See his work, De Mulier. Organ. Gener. inserv., particularly Ch. 12, and tab. 1, 5, 6, 7, 8, 11, 13, 14. These vesicles have accordingly been termed Ova Graafiana, or Vesiculæ Graafianæ, by Haller, ubi supra, Blumenbach, Instit. Physiol. § 550. and other writers. Haighton enumerates the circumstances which De Graaf was supposed to have ascertained respecting the ovaria; but some of the positions are not correct, and others are doubtful; Phil. Trans. for 1797, p. 160, 1. Bartholine "De Ovar. Mulieb." and Drelincour "De Fœm. Ovis." may be consulted in connexion with De Graaf; although the latter is to be noticed principally as a specimen of laborious research. Among the modern works on this subject I may select Sabatier, Anat. t. ii. p. 455..462; Boyer, Anat. t. iv. p. 584..7; Bichat. Anat. Descript. t. v. p. 294..6, and the long article "Ovarie," by Murat, in Dict. Scien. Méd. We have good views of the ovaria in Smellie's Anat. Tables, No. 5; in Monro's Elem. v. ii. pl. 8. fig. 2, 3, and pl. 9. fig. 2, 3, 4; and in Caldani, pl. 134, 149, and 152. The changes which are produced in the female ovarium by the influence of the male semen, the nature of the rudiments of the fœtus, the state of its functions, and mode of their action, with its gradual evolution and subsequent growth, until it acquires that condition, which can enable it to support an independent existence, have been lately made the subject of minute investigation. The limits to which I am restricted will allow me to do no more than to refer my readers to some of those sources, whence they may obtain more full information. Among the most important of these are Baer, De ovi genesi, and Sur la formation de l'Oeuf, par Breschet, accompanied by figures; his opinions generally coincide with those of De Graaf; Breschet, Etudes de l'Oeuf humain, with plates; Velpeau, Embryologie humaine; Cloquet, Anat. p. 735 et seq., with the accompanying plates. See also the elaborate art. "Oeuf," by Desormaux, Dict. Méd. t. xv. The art. "Fœtus," by Dugès, Dict. Méd. et Chir. prat. t. viii., although principally devoted to pathology, may be perused with advantage in this connexion. Raspail's observations appear to be correct and appropriate, "New System," § 613 et seq. The 2d section of Mr. Mayo's 15th chapter contains a valuable summary of the recent observations, with references to the various authors. The 2d chapter of the 3d part of Dr. Roget's Bridgewater Treatise contains an interesting summary of opinions. I have already, on more than one occasion, referred to Dr. A. Thomson's learned dissertation on the vascular system of the fœtus. The report made by Dutrochet, in conjunction with Serres and Is. St. Hilaire, on a memoir which was presented by Coste to the Acad. Scien., may be perused with advantage, as containing an abstract of the information that has been lately obtained on this subject, together with many original remarks

These glands are connected with the uterus on each of its sides by two ducts, which, from the name of the anatomist who first correctly described them, are termed the Fallopian tubes. These, in the unimpregnated or quiescent state of the uterine system, are narrow, tortuous, and almost impervious, particularly at their lower extremity. The other end is more capacious, and is so situated as to possess the power, on certain occasions, of embracing the ovarium, being furnished with a loose fringed membrane, which seems to be the part in which its specific action more immediately resides[1].

The uterus, in its unimpregnated state, is a compact, dense, membranous body, provided with a copious supply of blood-vessels, which run through its substance in all directions, and is also furnished with a considerable number of lymphatics and nerves[2]. It is likewise possessed of great muscular power, as is proved by the mechanical force which it exercises during parturition. Yet, notwithstanding this circumstance, it would appear that it is somewhat difficult to demonstrate the fibres, for we learn that some anatomists of eminence have even doubted of their existence[3].

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by t heauthor; Ann. Sc. Nat. t. iii. 2d ser. p. 78 et seq. In the British and Foreign Medical Rev. p. 238 et seq. we have a notice of an essay by Baer, on the early development of the human embryo, accompanied by figures. The Ed. Med. Journ. v. xli. p. 153 et seq. contains a judicious review of the works and opinions of Breschet and Velpeau, with various critical remarks, and in v. xlv. of the same journal we have an abridged translation, by Dr. Barry, of the valuable work of Valentin of Breslau, on the development of the human ovum, and of an essay by Prof. Wagner, on the Germinal vesicle. The accurate and elaborate researches of Purkinje have contributed very considerably to advance our knowledge of the ovum in its earliest stages and of its progressive changes.

[1] Fallopius's account of his observations is contained in his Instit. Anat. Opera, p. 438. See Haller, El. Phys. xxviii. 2. 29..32.

[2] For a view of the lymphatics of the uterus I may refer to Mascagni, "Vas. Lymph. Hist." p. 94. tab. 14, and to Cruikshank on the Absorbents, p. 156..8; for the nerves to Walter, "Tab. Nerv. Thor. et Abd." No. 1. fig. 1. 472, 3; and to Tiedemann, "Tab. Nerv. Uteri." Haller has given two good plates of the uterus and its appendages, Opera Min. t. ii. p. 32. and Icon. Anat. Fas. 1. tab. 3. fig. 1. We have good plans of the uterus in Bell's Anat. v. iv. p. 229 et seq., which very aptly illustrate its form in all its different states and its connexion with the contiguous parts. By way of comparison the plates of Swammerdam may be consulted in his "Miraculum Naturæ."

[3] Haller, El. Phys. xxviii. 2. 9, admits that the uterus is obviously muscular in quadrupeds, but says that the human uterus is of a peculiar structure. He describes it as being a cellular spongy mass, in which it is not easy to detect any muscular fibres; yet he acknowledges their existence, § 11, and brings forward many proofs of their contractility, § 10. Blumenbach, however, informs us that he could never detect any muscular fibres in the uterus, and in conformity with his general practice, attributes its action to the vita propria, § 547. He assigns other offices for the vita propria of this organ, § 561, 598. This may also be regarded as the opinion of Bichat, Anat. Des. t. v. p. 287. Dr. Ramsbotham appears to be the latest writer who does not admit the muscularity of the uterus; see Ed. Med. Journ. v. xxxix. p. 456.

Besides the immediate and specific use of the generative organs, in the continuance and reproduction of the species, they appear to exercise a peculiar and specific influence over the system at large, affecting its general form and its powers, both mental and corporeal, causing the growth and development of particular parts, and giving to the individual, in a more remarkable degree, those characters which constitute the peculiarity of sex. The constitutional difference of the two sexes during infancy is not very considerable, but at the period of puberty, when the generative organs are developed and their functions established, the difference is very much increased, and continues during the remainder of life. It will not be necessary to enter into a minute detail of these differences; it will be sufficient to remark, that they exist in the anatomical structure of the body, in its chemical constitution, and in its powers, and that besides those organs which are immediately subservient to generation, certain parts, which are more peculiarly characteristic of the two sexes, then become prominent, as the beard of the male and the mamma of the female[1].

We have an opportunity of observing this influence of the generative organs over the constitution by noticing the effect which takes place, when, from any cause, their proper development is not permitted to take place. If, in consequence of any original mal-conformation, the testes remain defective, or when emasculation has been performed, the male never assumes the characters of the sex, and we have reason to conclude that the same is the result of the extirpation of the ovaria, although the

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But it would appear that, with some exceptions, the most distinguished modern anatomists are advocates for its muscularity; Dr. Elliotson enumerates the following names among the supporters of this doctrine: Malpighi, Morgagni, Mery, Littre, Astruc, Ruysch, Monro, Vicussens, and Haller. To these may be added, among the earlier anatomists, Vesling, Syntagma Anat. p. 53, 4, and among the moderns, the great name of Wm. Hunter, see pl. 14 of his work on the gravid uterus. If any doubt remained upon the subject, we might consider it as removed by the accurate investigations of Sir C. Bell; Med. Chir. Tr. v. iv. p. 335 et seq.; also Anat. v. iv. p. 279. Dr. Blundell observes, that the uterus of the rabbit exhibits a distinct muscular structure; Researches, p. 35. Dr. Lee speaks of the muscular tissue of the uterus as of a well recognized body; Med. Chir. Tr. v. xvi. p. 377 et seq. and v. xix. p. 94 et seq. See also the remarks of Dr. Hodgkin, in his Trans. of Edwards, p. 444, and of Dr. Quain, Anat. p. 850.

[1] See Haller, El. Phys. xxvii. 3. 15. and Blumenbach, Inst. Phys. sect. 35, for an accurate account of these differences. We have a volume by Roussel expressly on this subject, "Système de la Femme et de l'Homme," in which he has described with considerable minuteness the characteristic peculiarities of the two sexes, both physical and moral, with their effect upon the various functions and organs; it contains many good remarks, although somewhat sentimental and diffuse in its style. In Vesalius, Opera, t. ii. p. 682. tab. 75, 6; in Cowper, Anat. Corp. Hum. tab. 1, 2, 3, and in Bidloo's Anatomy, fig. 1, 2, 3, we have beautiful and characteristic engravings of the male and female form; Sœmmering has pourtrayed the peculiarities of the bones of the female in his "Tabula Sceleti Feminini."

changes in the female are less obvious, and we have not such frequent opportunities of observing the effect of the operation[1].

The influence of the generative organs upon the constitution is sometimes singularly illustrated in those individuals who have been styled hermaphrodites, and who have been popularly supposed to unite in themselves the functions of both sexes. We are now convinced of the impossibility of this occurrence, but in these cases, we find that, owing to the imperfection of the parts and the deficiency of their functions, the constitution is so much modified, that it is sometimes difficult to determine which sex predominates[2].

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[1] Haller, El. Phys. xxvii. 3. 3. Haighton found that by dividing the Fallopian tubes sexual feelings were destroyed, and the ovarium gradually wasted; Phil. Trans. 1797, p. 173 et seq. Pott gives us a short account of the case of a female, where both the ovaries were extirpated; he remarks that the person "has become thinner and more apparently muscular; her breasts, which were large, are gone; nor has she ever menstruated since the operation, which is now some years." Works, v. iii. p. 330. We have a case related in the Phil. Trans. for 1805, p. 225 et seq. of an adult female, in whom the ovaria were defective, and where there was a corresponding defect in the state of the constitution. To the same general principle we ought probably to refer the partial growth of a beard on females in the decline of life; Blumenbach, Instit. Physiol. § 660. It has been likewise observed that female birds, when they have ceased to lay eggs, have occasionally assumed the plumage, and to a certain extent, the other characters of the male; Hunter on the Anim. Œcon. p. 75 et seq; Blumenbach, Elem. Phys. § 660, note; Home in Phil. Trans. for 1799, p. 174. We have a paper on this subject by Dr. Butter, in the Wernerian Memoirs, v. iii. p. 183 et seq., from which it appears that this change is by no means unfrequent in old female birds; he conceives it not to be, in any respect, a morbid change, "but the natural result of age and time." We have a still later account by M. St. Hilaire, which is translated with observations by the editor, in Jameson's Journ. for Oct. 1826, p. 302 et seq. Mr. Yarrell has, however, found, that the change does not depend upon age, but that it is connected with some disease or imperfection of the sexual organs. He further observes, that in both sexes, their characteristic peculiarities disappear or are diminished when these organs are imperfect, and that in this case, the sexes approximate so much, that it is difficult to distinguish between them; Phil. Trans. for 1827, p. 268 et seq. It may be remarked that emasculation has a less marked effect on the form of the inferior animals than of the human species. On this subject see the art. "Eunuche," by Adelon, Dict. de Med. t. viii. p. 360 et seq.

[2] It is not easy to determine the exact degree in which the mixture of the two sexes has occurred in the human species, in consequence of the tendency to exaggeration which is so frequently met with in the older writers, but we may venture to assert, that a perfect uterus, with its appendages, and perfect testes, have never been detected in the same individual. We have an elaborate memoir by Haller, entitled, "Num dentur Hermaphroditi Commentarius," Op. Min. t. ii. p. 9 et seq. in which he relates some observations of his own, and takes a view of the best authenticated cases that are on record, of the irregularities of the sexual organs. In most instances it is easy to point out to which sex the individual ought to be referred, but the author concludes that the evidence is irresistible in favour of the existence of certain cases, in which there was an actual mixture of the two sexes, but with an imperfect development of the organs. Sir Ev. Home comes to a similar conclusion in a paper which contains a number of facts and observations on the same sub-

SECTION 2. *Remarks on the Functions of the two Sexes in the Process of Generation.*

The office of the male is simply to introduce into an appropriate part of the body of the female a portion of the peculiar

ject; Phil. Trans. for 1799, p. 157 et seq. He remarks, "there is much reason to believe, that no instance of an hermaphrodite, in the strict sense of the word, has ever occurred in the more perfect quadrupeds, or in the human species;" p. 157. He arranges the supposed or reputed cases into four classes: "1st. Such malformations of the male, as led to the belief of the persons being hermaphrodites; 2d. Such malformations in the females as have led to the same conclusion; 3d. Such males as, from a deficiency in their organs, have not the character and general properties of the male, and may be called neuters. 4th. Those in which there is a real mixture of the organs of both sexes, although not sufficiently complete to constitute double organs; which, I believe, is the nearest approach towards an hermaphrodite that has been met with in the more perfect animals;" p. 159, 0. A remarkable case of this last description is related by Dr. Baillie; the external parts resembled those of a female, while there appears likewise to have been the rudiments of the male organs; the internal organs were not examined; Morb. Anat. p. 410, 1, also works by Wardrop, v. ii. p. 371. We have a well detailed case in Mém. Soc. Méd. d'Emul. t. iii. p. 293..5, of a male where the parts were imperfect, and the form of the body resembled that of the female. Mr. Mayo mentions two cases, one of an imperfect male, the other of an imperfect female, where the general form of the body resembled that of the other sex; Physiol. p. 368.

An individual was exhibited in this metropolis, in the year 1818, who possessed a remarkable mixture of the characteristics of the two sexes. Without a more minute examination of the generative organs than it was possible to make during life, it was not easy to determine to which sex they should be referred. The countenance resembled that of the male, and there was a beard, although scanty. The form of the body and of the limbs was, however, decidedly female, and the mammæ were considerably developed. Upon the whole I am disposed to conjecture, that the individual in question was a female, where the uterus and the ovaria were defective, and where there was an irregularity in the form of the external organs. In the Ed. Med. Journ. v. xliii. p. 313 .. 8, a case is related, where the individual is stated to hav econsidered himselfto be a female until the age of thirty, when the testes were developed, and the constitutional characteristic of the female, which had hitherto predominated, was converted, although imperfectly, into that of the male. There are some valuable observations on this subject in Blumenbach's Essay on Gener. sect. 3. p. 81; in Beck's Med. Jurisp. ch. 4. p. 42..52, also in the art. "Generation," in Rees's Cyclopædia; and "Hermaphrodite," in Dict. Scien. Méd. by Marc, t. xxi. p. 86 et seq.; and in Dict. de Méd. t. xi. p. 71 et seq.; also the art. "Hermaphrodite," by Dalyell, in Brewster's Encyc. The belief in the existence of human hermaphrodites was formerly entertained by the most eminent anatomists; see Cheselden's Anat. p. 314. tab. 33; and an essay by K. Boerhaave on Hermaphrodism, in Mem. Petrop. t. i. p. 315 et seq., published in 1750. We have two very valuable papers by Dr. Duncan, jun. in Edin. Med. Journ. v. i. p. 43 et seq. and p. 132 et seq., which, although not directly connected with this subject, contain some observations which tend to illustrate it. Dr. Knox admits the reality of the existence of hermaphrodism, and attempts to explain it upon the principle, that every individual possesses the rudiments of both organs; that, in ordinary cases, one or the other is developed according to circumstances, but that, in secretion which is furnished by the testis. That of the female is considerably more complicated; it consists, in the first instance, in providing a substance which, in connexion with the male secretion, is to constitute the fœtus, in furnishing a suitable situation in which the fœtus may be deposited, in affording it the due nourishment, until it is able to support itself by its own powers, and, lastly, after the fœtus is detached from the mother, she continues to supply it with a peculiar kind of food, especially adapted to the digestive organs of the young animal[1]. This long series of operations is, however, exercised only by the females of the class of the mammalia. In proportion as we recede from these, the office of the mother is less complicated and continuous. In birds, the process of utro-gestation is dispensed with, nor is there any organ in the body of the female adapted to the production of a peculiar kind of food for the nourishment of the newly born animal; in place of the first, incubation is substituted, and the latter is compensated by the instinctive care of the mother, in selecting for her offspring the kind of food which is adapted to the state of its digestive organs. As we descend into the lower classes of oviparous quadrupeds and fishes, we find the functions of the female still farther diminished. In many tribes it consists merely in furnishing a substance which is to receive the male secretion, in some cases within, and in others out of the body, while the young animal is spontaneously evolved from its ovum, and is supported without any assistance from the mother.

Considerable difficulties, however, attend our explanation of every part of this process, and a great variety of hypotheses have been proposed, some of them supported by numerous and direct experiments, in order to remove these difficulties. That I may be able to throw some light upon the subject, I shall consider each part of the operation separately. And first, with respect to the immediate effect of the male secretion. It has

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certain instances, there is an imperfect attempt at the development of both. Brewster's Journ. v. iii. p. 322 et seq.

In some of the mammalia we have a decided mixture of the two sexes, although the appropriate parts, and consequently their functions are imperfect. This is particularly exemplified in the free-martin, of which we have an account by Hunter; Phil. Trans. for 1779, p. 179 et seq., and Observ. on the Animal Œcon. p. 55 et seq. Sir Ev. Home, in the paper referred to above, relates an instance of a similar conformation having occurred in a dog; p. 168, 9. Morand informs us that he examined an hermaphrodite carp, which had ova on one side and a melt on the other, and he states this to be not an uncommon occurrence in fish; Mém. Acad. pour 1737, p. 51, 2. We may presume, that as we descend to the lower orders, even of the vertebrated animals, we shall meet with a nearer approach to perfect hermaphrodism. We have an account of a preparation, taken from an ourang-outang, in the museum of Guy's Hospital, where all the parts belonging to both sexes are said to be preserved; Ed. Med. Journ. v. xxxvii. p. 288, note.

[1] Technically speaking, the function of the male is confined to the process of impregnation, that of the female comprehends the various operations of conception, gestation, parturition, and lactation.

been stated above[1], that Fourcroy and Vauquelin detected a fibrous substance in the seminal fluid, with which, as being of a peculiar nature, it may be reasonably supposed, that its specific effects are in some way connected. Except, however, the mere existence of this fibrous substance, we are totally unacquainted with it; we know nothing of its properties, either chemical or physiological, and are, of course, unable to explain how it operates in the formation of the fœtus.

The presence of the spermatic animalcules is, however, a still more remarkable circumstance in the constitution of the seminal fluid. If we are to rely upon the experiments and observations of Spallanzani and of the Genevese physiologists, we are unavoidably led to the conclusion, that the secretion of the testis contains animalcules of a specific kind, that they are not derived from any extraneous source, but form one of its essential components; that they are present in the various species of males, but that they cannot be detected when, either from age or from disease, the animals are rendered sterile. Hence we can scarcely refuse our assent to the position, that these animalcules are, in some way or other, instrumental to the production of the fœtus, although we are altogether unable to assign the particular mode in which they produce their effect[2].

When we consider the anatomical structure of the organs of the two sexes in the mammalia, where there is the greatest number of parts, and where the functions appear to be the most elaborate in their nature, a difficulty has arisen in conceiving how the secreted fluid can be brought into contact with the parts of the female where the rudiments of the fœtus appear to be lodged; yet the analogy of some of the lower animals seems to show that this contact is necessary. Many experiments have been instituted to discover the exact part of the female organs into which the semen is projected, or to which it may be afterwards conveyed by absorption, or by any other vital or physical process. It has been doubted whether the fluid is capable of entering the uterus, and various experiments and observations have been adduced, even by Harvey and some of the most eminent physiologists, to prove that this is never the case[3]. But, upon considering all the facts that have been brought forwards on both sides of the question, the opposite opinion appears to be the most probable, and to this Haller inclines, although fully aware of the objections that have been urged against it[4]. There is much more difficulty in supposing that it can pass beyond the organ; so that it will remain for us to inquire, how the presence of the semen in the uterus can act in the production of the fœtus[1]. This will lead us to examine what part of the female acts in the process, and in what way the uterus, or the different organs connected with it, are affected, or what change they each of them experience.

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[1] P. 493.

[2] See the general conclusions of Prevost and Dumas in Ann. des Sciences Naturelles, t. i. p. 289, and in t. ii. p. 147..9.

[3] De Gener. Exer. 39. p. 145; 67. p. 308; 68. p. 312; et de Concept. p. 405. The difficulty of accounting for the passage of the semen to the ovarium, which was supposed to be necessary for conception, gave rise to the hypothesis of the aura seminalis. Harvey's chapter "De Conceptione," appended to his work on Generation, affords a singular contrast to the correct and cautious spirit which so generally pervades his writings.

[4] See Boerhaave, Prælect. not. 6. ad § 673, t. vi. p. 74, 5. and El. Phys. xxix. 1. 11, where the subject is very minutely examined. The testimony of

After the period of puberty, when the uterine system has acquired its full size, and the power of exercising its appropriate functions, certain causes, and especially the excitement of the seminal fluid, produce an unusual flow of blood to the ovaria, and the parts connected with them. The fimbriæ of the Fallopian tubes become turgid, and embrace the ovaria, where one of the vesicles, which appears to be more immediately affected, is protruded from its former position, and bursts, discharging a drop of an albuminous fluid, which is received by the tube and conveyed to the uterus; this constitutes the ovum, and is to be regarded as the first rudiment of the future fœtus. The vesicle from which the ovum has escaped, experiences a peculiar change in its texture and appearance, and is converted into what is named the corpus luteum.

The operation by which the uterus receives and supports the ovum, which is transmitted to it, is no less wonderful than that by which it is conveyed there. It is immediately attached to some part of the internal surface of the uterus, a communication is established between them, the exact nature of which is still,

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Leeuwenhoek is very direct in favour of the reception of the semen into the cavity of the uterus, a fact which he ascertained by actually detecting the spermatic animalcules in this organ; Arc. Nat. in Op. t. i. p. 155, 166, 169. The confirmation which we have lately had of his observations must lead us to conclude that on this point, in which he could not easily be mistaken, we may rely with confidence upon his statements. We learn also from the same authority that the animalcules have been detected in the cornua of the uterus and the Fallopian tubes of the rabbit and the dog; t. i. p. 170, 1; and t. iv. p. 208, 9. Respectable authorities are not wanting who assert that the seminal fluid has been found in the Fallopian tubes of the human subject, but this scarcely seems to have been the result of direct observation. Should it however appear that this was actually the case, we are unable to say by what power it could have been conveyed there. See the remarks of Dr. Elliotson, with the authorities to which he refers; Blum. Inst. sect. 39, note A, p. 325, 6. De Graaf, de Org. Mulieb. p. 243; Ruysch, Advers. Anat. Dec. 1. tab. 2. fig. 3; and Sauvages, Physiol. p. 222; all maintain the opinion that the seminal fluid is deposited in the uterus, as the result of their own observation.

[1] The ciliary motions which have been observed by Parkinge and Valentin; Ann. Sc. Nat. t. iii. 2d ser.; and by Dr. Sharpey, in certain parts of the organs of respiration, have been also detected in the generative organs; it is possible that these motions may, in some way, contribute to the passage of the semen to its ultimate destination, and thus enable it to act, either directly or indirectly, upon the ovarium. Dr. Sharpey, however, observes that the direction of the motions in these organs is from within outwards, so that it is difficult to assign any other office for them "than that of conveying outwards the secretion of the membrane, unless we suppose that it also brings down the ovum;" Cyc. of Anat. v. i. p. 633.

in some measure, unknown; while we have an elaborate organization of membranes of various kinds, for the purpose of supporting the fœtus, protecting it from injury, and conveying to it all the substances that are necessary for its nutrition and existence. The uterus, in the mean time, increases in size so as to contain the fœtus, and its membranes; its vessels have their diameters proportionably enlarged, and it acquires a great addition of fluids, by which it is enabled to perform all its extraordinary functions[1].

In considering the physiological relations of the different parts of this process, the following points will particularly require our attention. Upon what organ does the seminal fluid first produce its appropriate effect? If upon the ovarium, in what does that action consist, and what is the nature of the changes which the ovarium experiences? In what stages of the process of conception is the ovum conveyed into the uterus, and by what power is the conveyance effected? By what means does the fœtus attach itself to the uterus, and how is it afterwards nourished and supported there? What change takes place in the action of the uterus, by which its new membranes are formed and its bulk increased? And, lastly, in what manner does it immediately contribute to the support of the fœtus? When we consider how many interesting physiological discussions are involved in these queries, and to what an almost infinite number of speculations and controversies they have given rise, it will be sufficiently evident, that I can only attempt to take a brief survey of some of the most interesting topics, and those which may more directly contribute to throw light upon the other operations of the animal œconomy[2].

[1] For a view of the changes which the uterus and its appendages undergo at this period, the student may examine Albinus, Tab. Uteri Mulier. grav.; Bidloo, Anatomia Hum. Corp. pars 4. tab. 53..63. "De Ingravidato Utero, Fœtu, ejusque Annexis;" and especially the splendid work of Wm. Hunter.

[2] Attempts have been made by various physiologists to elucidate the hypothesis of generation by watching the growth and evolution of the chick in ovo; of these observations the most important and interesting are those of Fabricius, Harvey, Malpighi, and Haller. Fabricius's treatise "De Formatione Ovi" contains much valuable information, and is illustrated by expressive engravings; the same commendation may be justly ascribed to his treatise, "De Formato Fœtu," in which the growth and successive formation of the parts of the fœtus are described and figured. Harvey's "Exercitationes de Generatione," although inferior to his immortal work on the circulation, is not unworthy of its author; if his reasoning appears less conclusive than on the former occasion, we must, in part at least, ascribe it to the intricacy and mysterious nature of the subject. Of Haller's essays, "De Formatione Cordis" and "De Formatione Pulli," it is sufficient to remark, that they have always been regarded as among the most valuable of his productions. I have already had occasion to refer to the admirable figures of Mr. Bauer; Phil. Trans. for 1822. We have many useful observations on this subject by Dutrochet in an essay "Sur les Envelopes du Fœtus," published in the "Mém. Soc. Méd. d'Emul." t. viii. p. 1..64, particularly the first section, containing an account of the eggs of birds, in which the author animadverts upon the anatomical arguments which were brought forwards by Haller in favour of the doctrine of pre-existing germs. We have an interesting account

The first of the above inquiries, the part of the organ upon which the seminal fluid produces its first effect, involves the point that has been already referred to, the situation in which it is originally deposited. We have seen that there is sufficient ground for believing that it enters the uterus, and that it is perhaps conveyed even into the Fallopian tubes. But upon either supposition we can offer no conjecture as to the mode in which it operates, farther than to say, that it produces a specific excitement, the nature of which we are unable to explain; this is directly applied either to the uterus or to the Fallopian tubes, and is thence propagated to the ovarium. An increased flow of blood seems to be the immediate result of the excitement, to which succeeds the enlargement of one of the vesicles, the escape of the ovum, the reception of it by the fimbriated extremity of the tube, and its transmission to the uterus[1]. It appears to be analogous to the general operations of the animal œconomy, that a specific stimulus, acting upon any organ, should call forth the specific functions of that organ, and that there should be a regular succession of its actions, depending upon the constitution of the part, until the ultimate effect is accomplished; but beyond this we can offer no explanation of the process.

But a question has here been started as to the fact; is the seminal fluid the sole and specific cause of the excitement? or can the turgescence of the ovarium, the escape of the ovum, and the consequent formation of the corpus luteum, take place without the co-operation of the male? If so, what is the state of the ovum which is thus evolved, and where is it deposited? While it continues in its unimpregnated state does it remain lodged in the Fallopian tube, or is it conveyed to the uterus?

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of the observations on the chick in ovo, by Adelon, Physiol. t. iv. p. 317 et seq. I may, in this place, refer again to Dr. A. Thomson's elaborate essay on the fœtus, which I have already had occasion to recommend to the careful perusal of the student. Connected with this topic, there is an essay by Cuvier, "Sur les Oeufs des Quad." in Mem. du Mus. t. iii. p. 98 et seq. and pl. 2. See also the various works referred to in note 2, p. 647; more especially the treatise of Velpeau, with its accompanying plates.

[1] Many of the older physiologists supposed that during sexual excitement, the female organs furnished a peculiar secretion, which unites with that of the male; this is well known to have been the doctrine of Hippocrates, "De Genitura," Op. t. i. p. 231..5. Fallopius, in his Observ. Anat. says, "Omnes anatomici uno ore asserunt in testibus fœminarum semen fieri, et quod semine referti reperiuntur," but he adds, "quod ego nunquam videre potui ....;" Opera, p. 421. De Graaf's 13th chapter is entitled, "De Semine Muliebri;" p. 194. See Haller, El. Phys. xxix. 1. 13; but this opinion is now generally discarded. It is, however, adopted by Blumenbach, § 552; and from his expression it would seem that he supposes it to be produced by the uterus, § 561. Blumenbach refers to Harvey, as maintaining the opinion of the female semen, but from the following passage in Exer. 35. I should conceive erroneously; ".... nec fœmina semen profundit, unde ovum oriatur." ".... neque in coitu semen ab utroque proveniat...." The existence of a peculiar female secretion forms the basis of Buffon's hypothesis of generation, but this is regarded as altogether without foundation; Nat. Hist. v. ii. p. 396 et seq. ch. 4.

We cannot doubt that the introduction of the seminal fluid, in the ordinary method of congress, is the sole cause of what is termed conception, the production of an impregnated ovum[1], and it was formerly supposed that the same stimulus was equally the cause of the evolution of the vesicle, and the consequent formation of the corpus luteum[2]. This opinion, has, however, been lately called in question, and as it is one which involves some very important considerations, both of a legal and of a moral nature, it becomes a point of the highest interest to examine the grounds on which it rests. Blumenbach appears to have been the first who decidedly maintained that, under certain circumstances, a corpus luteum may be produced without the co-operation of the male[3], and some facts in confirmation of the same opinion have been more lately adduced by Sir E. Home[4].

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[1] Haighton performed an elaborate set of experiments in order to ascertain how far the division of the Fallopian tubes prevented impregnation, and the result was that, after this operation, a fœtus was never produced. Corpora lutea were, however, formed in this case, and in consequence of his considering the production of these bodies to be a test of impregnation, he drew a conclusion which is directly the reverse of what the experiments might seem to warrant. He conceives that the semen penetrates no farther than the uterus, and acts upon the ovaria by sympathy. One important point he has established, that the effect which is propagated to the ovaria, whatever be its nature, is not accomplished until nearly fifty hours after coition. His experiments were performed on rabbits, and bear every mark of accuracy and fidelity; Phil. Trans. for 1797, p. 159 et seq. The experiments of Cruikshank, which were very numerous, and appear to have been made with the requisite degree of skill and correctness, lead to the conclusion, that the rudiment of the young animal is perfected in the ovarium; Phil. Trans. for 1797, p. 197; but it must be acknowledged that we are not sufficiently acquainted with the precise nature of the ovum which he observed to draw any positive inference.

[2] Haller decidedly maintains that the formation of a corpus luteum is a proof of the production of a fœtus; El. Phys. xxix. 1, 15, 16. In the catalogue which he gives of his discoveries, appended to his great work, he inserts the following: "Corpus luteum oritur ex conceptione, neque prius paratum adest." Auctarium ad El. Phys. p. 7; see also Op. Min. t. ii. p. 457. Haighton lays down the position, that wherever corpora lutea are found, "they furnish incontestible proof" of previous impregnation; Phil. Trans. for 1797, p. 164, which compare with p. 166; I have already alluded to the error into which he fell on this subject.

[3] Inst. Physiol. § 562, note, p. 312..3; also Comment. Soc. Roy. Scienc. Gotting. t. ix. p. 109..114.

[4] Sir E. Home defines a corpus luteum to be "a solid compact glandular substance in which the fœtus is formed," not as had been previously supposed, a body produced by the ovum, or a consequence of its existence, Phil. Trans. for 1817, p. 256 et seq. The figures which Mr. Bauer has given us of the ovarium in its different states are among the most beautiful of his numerous performances; fig. 8..11; also Phil. Trans. for 1819, pl. 3..9. In the paper to which these plates refer, Sir E. Home brings forward fresh evidence to prove that the corpus luteum is not necessarily preceded by impregnation; see his Lect. on Comp. Anat.; also Beck's Med. Juris. p. 104. The late valuable experiments of Dr. Blundell confirm the most important part of Sir E. Home's doctrine, that corpora lutea are not the necessary result of impregnation; Researches, p. 49, 56 et alibi. Dr. Blundell, however, appears to conceive that either intercouse with the male,

But if we conceive that a corpus luteum is, in any instance, produced without the co-operation of the male, it must follow, that in such case the specific action of the seminal fluid is not exercised upon the ovarium, and hence it would appear more probable that the evolution of the vesicle and the production of the corpus luteum are, in all cases, essentially dependent upon the actions of the female, and that the office of the male does not commence until after this previous step of the process. We may then conjecture that the stimulating influence of the male secretion only acts in increasing the excitement which already exists in the vesicles of the ovarium, by which the ovum is detached from it, and is either simply received by the tube, or is transmitted to the uterus, in one or other of which situations it meets with the seminal fluid, and where the specific action commences, which gives rise to the impregnation of the ovum and the production of the fœtus.

Perhaps the most natural supposition may be, that the ovum is transmitted to the uterus in the unimpregnated state[1]; but there are certain facts which seem almost incompatible with this idea, especially the cases, which not unfrequently occur, of perfect fœtuses having been found in the tubes, or where they have escaped from them into the cavity of the abdomen. Hence it is demonstrated that the ovum is occasionally impregnated in the tubes; and we can scarcely resist the conclusion that it must always be the case. What upon the whole appears most pro-

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or at least a very high degree of sexular excitement is necessary for their production, and the same opinion is adopted by Cuvier; Leç. d'Anat. Comp. No. 29, t. v. p. 57. We have drawings of corpora lutea in Wm. Hunter's great work, pl. 31. fig. 3; pl. 29. fig. 3; and pl. 15. fig. 5. Murat. in an elaborate article, "Ovarie," in Dict. Scien. Méd. supports the doctrine of Haller, p. 5, 6. Valentin also, whose opinion on all points connected with these organs must be considered as of great weight, speaks of them as the "surest sign" of impregnation having taken place; Ed. Med. Journ. v. 45. p. 420.

[1] Haller discusses this hypothesis in El. Phys. xxix. 1. 18..24, and decides against it. The experiments of Cruikshank, to which I have already referred, tend to the same opinion, although they cannot be regarded as actually demonstrating it. See also Fleming's Zoology, v. i. p. 398. Sir E. Home conceives that, in consequence of the general excitement of the female organs, the tubes are so far expanded as to allow the semen to pass along them to the ovum, which is probably detained at their farther end for some time, in order to admit of the impregnation; Phil. Trans. for 1817, p. 257. A case is detailed by Dr. Granville, in the Phil. Trans. for 1820, p. 101 et seq. of a fœtus, which appears to have been lodged in the body of the ovarium itself; and it is considered by the author as a proof that conception always takes place in this organ. In this instance we may presume that it actually did so; but I do not conceive that it necessarily affects the view of the subject which I have given above. We may conjecture that the ovum had been developed, but by some accident, instead of being discharged into the tube, remained attached to the ovarium, and that, contrary to what usually happens, it was impregnated in this situation. We have a case of ovarian conception very fully detailed by Bœhmer, with illustrative engravings; Observ. Anat. Rar. Fascic.

bable is, that a general excitement of the uterine system[1], not necessarily connected with the co-operation of the male, produces the evolution of one of the vesicles; that this is discharged into the Fallopian tube, where, upon meeting with a portion of the seminal fluid, it becomes fertilized and impregnated, and constitutes the first stage of the existence of the fœtus.

The means by which the ovum[2] is propelled along the tube, whether before or after impregnation, like every other step in the process, is involved in considerable obscurity. We know of no power, except that of muscular contractility, which could cause such an action to take place, and yet, notwithstanding the size of the part, no muscular fibres have been detected in it; we can, however, scarcely doubt of their existence. As the body which is transmitted is small, and as it is only on certain occasions that their contractility is called into action, we may conjecture that the fibres are extremely minute, but that by the momentary application of a very powerful stimulus, they are enabled to perform the office which is assigned to them.

We conceive that the ovum has now arrived at the uterus, and is become impregnated; the next step in the process is its attachment to a part of the inner surface of this organ. This is succeeded by a gradual, but very considerable increase of its bulk, and the formation of new parts, by which it may maintain its connexion with the fœtus, composing the maternal part of the placenta. The change which takes place in the ovum consists in the production of a membranous envelop, within which the young animal is contained, surrounded by a quantity of an albuminous fluid, a vascular connexion being established between the body of the animal and a part of this envelop, which constitutes what is termed the umbilical cord and the fœtal part of the placenta[3]. The maternal and the fœtal placenta may be considered as composing temporary appendages to the circulating system of the mother and the fœtus respectively, and preserving the necessary connexion between them, although it appears that we have no evidence of their having any vascular communication with each other. The various complicated operations which compose these successive steps of the process, although well established as matters of fact, appear to be altogether inexplicable. We do not know why the presence of the impregnated ovum should cause it to increase in bulk, and produce the placenta; and we are equally ignorant of the powers by which the fœtus is gradually developed.

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[1] The excited state of the female organs is described by Cruikshank; Phil. Trans. for 1797, p. 197 et seq. See also Boerhaave, Præl. § 673 cum notis. Haighton's experiments, in his 1st section, p. 162..6, as well as those of Cruikshank, seem to prove, that the excitement of coition tends to accelerate the formation of corpora lutea, although we may conceive it not to be essential.

[2] The term ovum is here employed, rather in compliance with general custom, than from any idea of its technical correctness; the researches, however, of the physiologists referred to above tend to show, that there is, at least, a very strong analogy between the egg of the aves and the ovum of the mammals; this is one of the points to which Purkinje especially directed his attention.

[3] We have a concise but perspicuous account of these successive operations in a paper by Mr. Burns, Edin. Med. Journ. v. ii. p. 1 et seq. We have some valuable observations on the fœtal membranes by Dr. R. Lee, in the Med. Chir. Tr. v. xvii. p. 483 et seq., and in Phil. Trans. for 1832, p. 57 et seq. The general conclusion at which Dr. Lee arrives is stated in p. 63.

With respect to the actual change which takes place in the uterus, it appears to consist, in the first instance, in an increased action of the blood vessels, by which a greater quantity of fluid is conveyed to the part. The mass of the solids appears to be augmented at the same time with the fluid; the organ is not only distended to many times its original bulk, but its parietes are much thickened, proving that its increased size is not owing to mere distention[1]. This process is continued until the period of pregnancy proper to the individual is completed, when the muscular power of the organ, which had remained dormant, is now excited into action, and finally succeeds in expelling its contents. But we are not able to explain this part of the operation more satisfactorily than those which preceded it. We do not know why the fœtus, after it had continued to increase for a certain length of time, should cease to grow; or why the uterus, after it has for a number of weeks or months gradually yielded to the increased bulk of the fœtus, now takes upon itself a new kind of action; and why its contractility, which before lay entirely dormant, should now be so suddenly and powerfully excited. That this does not depend upon any mechanical cause acting on the uterus, is proved by those cases of extra-uterine fœtuses, where the uterus undergoes the same kind of change, and has the same disposition induced at the end of the usual period, as if the fœtus had been contained within its cavity[2].

The last of the questions which were proposed for consideration was the mode in which the uterus contributes to the support of the fœtus[3]. The support that will be required in this case must be of two kinds, or directed to two objects; the first, the means by which the body has its contractility maintained; the other, those by which it acquires its supply of what is more strictly termed nourishment, equivalent respectively to the two functions of respiration and digestion. The first of these points has already been considered in the chapter on respiration, where I remarked that, although the subject is not free from difficulties, yet that we have reason to believe, that the placenta serves the purpose of lungs for the fœtus, and that it performs this office by having its blood brought into close proximity with the arterial blood of the mother, in the same way that the venous blood of the pulmonary artery receives its appropriate change by means of the air which is contained in the vesicles of the lungs. It is admitted that the change which is thus effected is inconsiderable, but the wants of the fœtus in this respect are few, and it may be presumed that the supply is equal to the demand[1].

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[1] Haller, El. Phys. xxviii. 2..9; Hunter on the gravid uterus, pl. 1..5, show its great bulk at the latter periods of pregnancy, and the state of its parietes. Pl. 10, fig. 1, affords a good view of its vessels at the part where the placenta is attached to it; pl. 11, shows the great size of the veins; pl. 15, of its arteries; and pl. 16, 17, and 18, of its blood vessels generally. See also Bell's Anat. v. iv. p. 237; Boyer, Anat. t. iv. p. 594, 5; Bichat, Anat. Des. t. v. p. 346..356. We meet with some very good observations on this subject in Malpighi's dissertation De Utero et Viviparorum Ovis; Manget, Bibl. Anat. t. i. p. 683 et seq.

[2] Blumenbach, Instit. Phys. § 598, p. 341; also Comment. Soc. Reg. Scien. Gottin. t. viii. p. 49, 51. We have a valuable paper by Breschet on extra uterine fœtuses in Med. Chir. Trans. v. xiii. p. 33 et seq.; he remarks, that in these cases the uterus enlarges, and the desiduum is formed, as in the natural process. Dr. Elliotson makes the same observation, ibid. p. 51 et seq.; his paper contains a valuable list of references.

[3] We have a very elaborate account given us by Walter Needham of all that was known upon this subject, and of the opinions that were entertained,

We have no direct evidence of the mode in which the fœtus procures its nourishment. We can scarcely doubt that it must be accomplished through the intervention of the absorbents; but whether it be by means of the cutaneous vessels taking up a part of the fluid which is in contact with their extremities, or whether there be some provision for the same purpose, connected with the placenta or any of its appendages, it is perhaps not easy to determine. Some physiologists of the last century supposed that the fœtus, during its immersion in the liquor amnii, received a portion of it into the mouth, and that this passed into the stomach, and was assimilated there by the ordinary process of digestion; but there seems to be sufficient ground for rejecting this supposition[2].

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up to the date of his publication, in 1667, in his essay De Formato Fœtu; this treatise displays considerable acuteness, and contains many remarks which were offered in a conjectural form, but have been confirmed by subsequent discoveries.

[1] P. 409. When we consider the mode in which the fœtus is connected with the mother, it will appear that the opinion formerly entertained respecting the power which the imagination or feelings of the parent has over the physical structure of the offspring is, at least, highly improbable, and very difficult to be explained, if not absolutely impossible. Little can be added to the judicious observations of Haller on this subject; El. Phys. xxix. 2. 21..6. This apparently exploded doctrine has, however, lately received the sanction of Sir E. Home; Phil. Trans. for 1825, p. 75..8. We may feel less surprise that such a doctrine should have formed a part of the creed of Lavater; Essays, by Holcroft, v. iii. p. 156. It has also been lately advocated by Pierquin, and he endeavours to prove, that the effect is not confined to the human species; Magendie's Journ. t. x. p. 364 et seq.

[2] This doctrine was supported by Harvey, De Gen. Ex. 58, p. 267. A doctrine which is at least as old as Aristotle, and which has been generally embraced, is, that in the chick its solids are, in the first instance, formed from the albumen, and that it is afterwards nourished by the yelk; Harvey, p. 77. I have already had occasion to refer to an essay by Monro 1^{us}, the object of which is to prove that the fœtus is nourished by means of the placenta; taking into account the state of our information at the time when it was writ-

A function of the uterus, which is obviously connected with the process of re-production, is the periodical discharges from its arteries to which it is subject. It commences at the period of puberty, continues as long as the power of bearing children remains, and is suspended during pregnancy and lactation [1]; hence we conclude that it serves some useful purpose, either in the production or the support of the fœtus; but I conceive that no plausible explanation has yet been given of the mode in which it operates. It appears now to be fully ascertained, that no female, except the human, is subject to this evacuation; but we are not able to assign any reason, either anatomical or physiological, for this peculiarity [2]. Both the period of its commencement and of its duration differs considerably among different nations, depending principally, as it appears, upon the temperature of the climate. In this country it usually makes its appearance about the age of fifteen, and ceases about forty-five or somewhat later [3].

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ten, it must be regarded as a very learned and judicious performance; Ed. Med. Ess. v. ii. p. 121 et seq. Sir C. Bell supposes that the placenta is an equivalent for both the lungs and the stomach; Anat. v. iv. p. 269..275. Wrisberg's Descrip. Anat. Embr. contains an account of five fœtuses that he had an opportunity of examining during the first months of their existence. We have a minute description by Senff of the successive stages of the growth of the bones of the fœtus during its early period, accompanied by a series of engravings, and a copious list of references. Sœmmering's Icones Embryorum Hum. exhibit views of the fœtus from its earliest stages. For remarks on the nutrition of the fœtus, see Adelon, Physiol. t. iv. p. 381 et seq.

[1] It appears, however, from the observations of Mr. Roberton, that there are not unfrequent exceptions to this rule; Ed. Med. Journ. v. xxxvii.

[2] See Blumenbach, Inst. Phys. note in p. 307.

[3] Haller, El. Phys. xxviii. 3. gives a full account of the phenomena that attend the menstrual discharge, and the various opinions that have been entertained respecting its causes. He attempts to explain it by a reference to Wintringham's experiments on the comparative density and extensibility of the arteries and veins; upon this principle he conceives that we can account for the recurrence of this discharge at a certain period of life only, and from no other female except the human. The reasoning of Haller is ingenious, but I think it may be doubted whether the facts will warrant the conclusion, and perhaps the facts themselves are questionable. As Blumenbach has not referred to these experiments nor to Haller's speculations, we may presume that he did not conceive them adequate to explain the phenomena; he candidly confesses his inability to account for them; sect. 38, § 558. See the remarks of Sir C. Bell; Anat. v. iv. p. 234 et seq. The occurrence of the secretion of milk subsequently to parturition is one of the most obvious and remarkable examples of the adaptation of the powers and functions of the animal to the situations in which it may be occasionally placed. Anatomists have pointed out a curious vascular connexion between the uterus and the mamma; but it may be doubted whether this can assist us in explaining the dependence of these organs upon each other; Eustachius, Tab. Anat. pl. 27, fig. 12; Haller, Icon. Anat. Fasc. 6. tab. 2. No. 30. Blumenbach seems to attach more importance to this structure; Inst. Phys. § 610. I may remark that the nipple affords an instance of what has been termed the erectile texture, similar to that which exists in the corpora spongiosa of the penis; it is, in the same manner, excited both by physical and mental causes; and these

A very curious question connected with the function of generation regards the circumstances which determine the future sex of the fœtus; but as this is a point on which I conceive that we are completely ignorant, I shall not think it necessary to give an account of the various conjectures that have been proposed respecting it[1]. It is a remarkable fact that, although there is no uniform proportion between the number of males and females produced by the same parents, yet that the total number of each sex brought into the world, taking the average of any large community, is nearly the same; or, more exactly, that we have, in all cases, a small excess of males. The data that we possess,

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causes have likewise a specific, and, as it would appear, an inexplicable effect in promoting the secretion of the milk. The occasional secretion of milk by the male, a circumstance which, however singular, it seems scarcely possible not to credit, proves that the action of the uterus is not essential to that of the mamma; See Blumenbach; Instit. Physiol. § 621. p. 349. In that mixture of the sexes, of which I have given some account above, p. 650, we find that animals, in which the male character predominates, occasionally yield milk; this appears to have occurred even in a bull which was capable of impregnating the female; Phil. Trans. for 1799, p. 171..3. The milky or curdy substance which is secreted by the crop of the male pigeon, during the incubation of the female affords a singular instance of the departure from the ordinary analogy of nature; see Hunter on the Anim. Œcon. p. 235 et seq.

[1] The doctrine of Hippocrates on this subject was, that the future sex is determined principally by the prevalence of the male or female semen, either as to the quantity of it which enters into the composition of the fœtus, or what he terms its strength; "De Gener." Opera, t. i. p. 233; and this opinion appears to have been generally embraced by the ancients and the earlier of the moderns. Another opinion which was current among the ancients, and which indeed forms one of the aphorisms of Hippocrates, is, that the different sexes occupy different sides of the uterus; Aphor. sect. 5. No. 48. Opera, t. ii. p. 1255. This notion, fanciful as it may appear, has been adopted by some modern writers, but it is completely overthrown by a case that occurred to Dr. Granville of a female, who had borne children of both sexes, and in whom the appendages belonging to the left side of the uterus were entirely wanting; Phil. Trans. for 1808, p. 308 et seq. pl. 17. Sir E. Home remarks that, in the earliest stages of the fœtus, the parts which determine the future sex are scarcely distinguishable, being so formed as to be easily convertible into each other. In pursuance of this opinion, he supposes, that "the ovum, previous to impregnation, has no distinction of sex, but that it is so formed as to be equally fitted to become a male or a female fœtus; and that it is the process of impregnation which marks the distinction ...." Phil. Trans. for 1799, p. 175. I may remark upon this speculation, that it almost necessarily involves the hypothesis of pre-existing germs; or that the fœtus exists ready formed in the female, independent of the co-operation of the male. Mr. Knight's ingenious researches into vegetable physiology, and especially his experiments on the production of hybrid fruits, lead him to conclude, that the influence of the female over the future offspring is more considerable than that of the male. With respect to the sex, he is disposed to think that this likewise depends more upon the female, because it is observed, in the breeding of animals, that certain females have a tendency to produce one sex in preference to the other, which does not appear to be the case with the male; Phil. Trans. for 1809, p. 392 et seq. See also Prichard's Researches, v. ii. p. 552, 3, and the remarks of Buzareingues, Ann. Sc. Nat. t. xix. p. 354 et seq.

while they prove that this excess exists in all countries, seem, however, to show that the amount of it differs in different countries. From a very extensive examination made by Hufeland, the numbers in Germany are as 21 to 20[1]. The census that was taken in this country in 1821 shows the numbers to be nearly 21 and 20.066[2]. But to whatever cause we may ascribe the relative proportion, it would appear that the greater number of males which are born is compensated by their greater mortality, whether produced by natural or accidental causes, for we find among adults, that the number of existing females rather exceeds the males[3].

SECT. 3. *Account of the Hypotheses of Generation.*

The preceding observations on the structure and action of the organs of generation, although comprehending a few only of the topics which have been investigated by the modern anatomists, will, I conceive, be sufficient to enable me to enter upon the consideration of the next subject into which I proposed to inquire—the nature of the generative process.

The inquiry, considered in the abstract, may be reduced to the following form, in what manner, or by what means, can an organized body produce another organized body similar to itself in its physical properties and its vital functions? After making

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[1] Edin. Phil. Journ. v. iii. p. 296..9.

[2] The following are the number of the two sexes born in England and Wales during the interval between the two last censuses; Popul. Abs. p. 154.

	Males.	Females.
1811	155,671	149,186
1812	153,949	148,005
1813	160,685	153,747
1814	163,282	155,524
1815	176,233	168,698
1816	168,801	161,398
1817	169,337	162,246
1818	169,181	162,203
1819	171,107	162,154
1820	176,311	167,349
	1,664,557	1,590,510

Dr. Cross, in his account of the medical schools of Paris, states, p. 191, that in the Dublin Lying-in Hospital, during a period of fifty-seven years, in which time 58,000 women had been delivered, the proportion of males to females born was about as ten to nine; during the ten years preceding that in which he writes, the numbers were 13,665 and 12,583; this is nearly in the proportion of 21 to 19.33. From some curious observations, made in different parts of Europe, it appears, that the surplus of males is greater in legitimate than in illegitimate births; assuming the number of 50,000 females, we have 52,896 legitimate males, and only 51,249 illegitimates. From some experiments lately made in France on sheep, it would appear that sex depends, in some measure, on the comparative vigour of the parents.

[3] Haller, El. Phys. xxviii. 1. 1; Jameson's Journ. v. xiii. p. 200.

ourselves acquainted with the phenomena which attend the process, with the changes which take place in the organs of the parent, and with the first appearances which we are able to detect of the independent existence of the fœtus, we may inquire, whether we are able to refer this succession of changes to the powers or functions which we have seen operating in the other parts of the animal system; whether, in short, the contractile or sensitive powers, which belong respectively to the muscular fibre or to the nerve, can so far modify or direct the ordinary physical powers of matter as to explain the phenomena. Few persons, perhaps, will be bold enough to assert, that we have any direct facts which can prove this to be the case; yet, on the other hand, I think it would argue at least an equal degree of confidence to conclude, that the generative process depends upon the operation of a different system of laws from those which belong to the other parts of the animal frame. Notwithstanding all the experiments and observations that have been made upon this subject, the parts which are essential to the operation are so minute and so much beyond our most elaborate researches, that the first step of the process seems still to have eluded our observation, so that we are even yet unable to do more than to make our election between one or other of the hypotheses that have been formed, all of which proceed upon the assumption of certain data, which it is extremely difficult either to confirm or to refute[1].

A circumstance which encreases the difficulty of this investigation is, that we are unable to derive much assistance from analogy. In considering the generative function, as it is carried on by the different classes of animals, there would appear to be three, if not four modes, in which the young animal is formed, and which may be regarded as essentially different from each other. The first is that which occurs in the higher orders of animals, where the sexual congress of the two individuals is essential to the production of the fœtus. The second is where the co-operation of two sexes is necessary, but when both exist in the same individual. The third comprehends those animals, where nothing resembling the sexual organs, or indeed any other organs specifically destined for generation, can be detected, but where we merely observe the fœtus to be detached from the body of the parent; while, in other cases, the body of the parent itself is divided into two or more portions, each portion, after the separation, acquiring those parts which are necessary for its perfect existence. It is, however, to the first of these modes that our attention must be exclusively directed on the present occasion; and for the reasons already assigned, I shall proceed to state the leading hypotheses of generation that have been proposed, and shall then consider which of them is the most consonant to the facts that have been ascertained upon the subject[1].

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[1] See the judicious observations with which Haller commences his section on this subject; El. Phys. xxix. 2. 1.

The earliest hypothesis of generation of which we have any distinct account, and one which has also received the support of some of the most eminent of the moderns, ascribes the original formation of the fœtus to the combination of particles of matter derived from each of the parents[2]. The second hypothesis in the order of time is that of Leeuwenhoek, who supposed that the seminal animalcules in the male secretion are to be regarded as the proper rudiments of the fœtus, and that the office of the female is to afford them a suitable receptacle, where they may be supported and nourished, until they are able to exist by the exercise of their own functions. The third hypothesis, that of pre-existing germs, proceeds upon a precisely opposite view of the subject, that the fœtus is properly the production of the female, that it exists, previous to the sexual congress, with all its organs, in some part of the uterine system, that it receives no proper addition from the male, but that the seminal fluid acts merely by exciting the powers of the fœtus or endowing it with vitality. A fourth hypothesis is that of Blumenbach, who conceives that the process of generation is effected by a peculiar principle or power, which he styles the nisus formativus, with which he believes that the living body is provided, for the express and exclusive purpose of re-production. These are the only hypotheses which can have any reference to the human species; but there is a fifth, which has been applied to some of the lowest tribes of animals, and which has been the subject of much discussion, as well as of numerous experiments. This is the doctrine of spontaneous, or, as it has been termed, equivocal generation, where a living organized being is produced, without the co-operation or previous existence of any similarly organized parent.

The first hypothesis, which has obtained the name of epigenesis[3], is the one which naturally presents itself to the mind, as the obvious method of explaining the necessity for the co-operation of the two sexes, the resemblance in external form, and even in mind and character, which the offspring frequently bears to the male parent, and still more the phenomena attending the production of hybrid animals, which it would seem almost impossible to explain, except upon the supposition that the fœtus is equally indebted to both its parents for the materials of which it is composed. It is upon these general considerations that the truth of this opinion must rest, for the facts that have been adduced to prove the existence of an appropriate secretion from the female, analogous to that of the male, would appear to be without sufficient foundation. The principal objections to this hypothesis, independent of the want of any direct proof of a female seminal fluid, are of two descriptions, those which depend upon the supposed impossibility of unorganized matter forming an organized being, and those which are derived from the observations and experiments of Haller and Spallanzani, which they brought forwards in support of their theory of pre-existing germs.

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[1] Drelincourt, who lived in the latter part of the seventeenth century, collected 260 hypotheses of generation; Blumenbach on Gen. p. 4. We have an interesting, although diffuse, account of the different hypotheses of generation in Buffon's 5th chapter of his Nat. Hist. v. ii. p. 410 et seq.; see also Sprengel, Hist. de la Méd. t. i. p. 231 et seq.

[2] See Harvey de Gen. Præf. sub init.; also Haller, El. Phys. xxix. 1. 13. and 2. 2.

[3] The term Epigenesis strictly means no more than the formation of a body by the successive additions of new matter, "partem post partem," according to the expression of Harvey, Ex. 45; although he adopts the hypothesis of epigenesis, he does not suppose that the male furnishes any actual matter to the fœtus, but that it imparts to the ovum the power of assimilating and organizing matter; Ex. 10, 25, 32, 39 et alibi. His explanation of the mode in which the male is supposed to operate, can only be regarded as a metaphorical illustration; Ex. 33, 50. By many of the modern physiologists the term has been employed in a more extended sense, so as to refer both to the source from which the new matter is procured, and to the relation which exists between its origin and the substance which is formed by it. Haller and Spal-

The second hypothesis is founded entirely upon the observations of Leeuwenhoek and others of a similar kind. The observations, as we have seen above, would appear to be correct. The seminal fluid, in all cases where we are able to collect and examine it, is found to contain a number of organized living beings, which, although they differ somewhat in the different kinds of animals, bear a close resemblance to each other, and are unlike any other bodies with which we are acquainted. The occurrence of these peculiar bodies in a fluid of such singular properties, naturally led to the suspicion of some connexion between the two circumstances, and it is not wonderful that the original discoverers should have supposed that these animalcules performed some necessary and specific office in the function of generation. Indeed, we are almost compelled to admit that, in some way or other, this must be the case; and yet the extreme improbability that they should be the rudiments of beings which are so totally dissimilar to them, has been supposed, by all the modern physiologists, to be of itself a sufficient refutation of Leeuwenhoek's hypothesis[1].

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lanzani seem disposed to restrict the word to the speculations of Needham and Buffon. I have not been able to ascertain who it was that first employed the term, or rather who used it in this sense.

[1] See El. Phys. xxix. 2. 6. for the objections that have been urged against the hypothesis of seminal animalcules; it seems to have been first formally opposed by Valisneri, who examined it in all its parts in his elaborate dissertation "Della Generazione," par. 1. cap. 3..13. I may refer to Morgan, as affording a proof of the general assent which was given to the existence of these animalcules and of their agency in generation a century ago, (1735,) and it is amusing to observe how he reasons upon the subject. Taking it for granted that one of these bodies is the origin of the future fœtus, he calculates at what rate it must grow from the period of conception to parturition. He estimates that the bulk of the animalcules is to that of the fœtus at birth

It is, however, contrary to all the analogies of nature to suppose, that bodies so peculiar in their properties and in their situation should not serve some specific purpose of utility in the actions of the animal œconomy. The conjectures that have been formed upon this subject are, indeed, for the most part, so fanciful, as to be scarcely deserving of notice, yet there is one which acquired such a degree of temporary celebrity, as to be entitled to distinct consideration. I refer to the peculiar modification of the hypothesis of epigenesis, which was proposed by Needham and Buffon. These distinguished naturalists conceived that there exists in all animated beings, what they term a vegetative force, which enables them, when placed in suitable situations, to produce or generate vital particles, which have an attraction for each other, and by their union compose living organized bodies.

The speculation, which seems to have been originally conceived by Buffon [1], was not only embellished by the charms of his eloquence, but was supported by a series of what appeared to be the most minute and elaborate observations, made by himself and by Needham, independently of each other. Upon examining the seminal fluid of various animals, they perceived a number of bodies to be floating in it; these however, they did not admit to be animals possessed of independent existence and regular organization, according to the opinion of Leeuwenhoek, but to be merely what they termed organic molecules, or vital particles, according to their hypothesis, produced spontaneously in the fluid, and which gave it its generative power. But, notwithstanding the authority of these authors, each of them eminent for their genius and their science, it is now universally admitted, that their speculations are entirely without foundation, sanctioned neither by facts nor analogies, and that many of the observations of Buffon are altogether incorrect [2].

as 1 to 19,200,000,000,000; hence it must double its bulk during every six days for forty-four successive periods, or for 266 days, which he considers the period of uterine gestation; Mech. Practice of Phys. p. 282, 3.

[1] With respect to the share which these two learned naturalists had in the formation of the new hypothesis, we learn from Needham, in the preface to his "Nouv. Obs. Micros.", in his letter to Folkes, Phil. Trans. for 1748. v. xlv. p. 615 et seq. § 18. and in the translation to the same subjoined to the above work, p. 184, that Buffon originally conceived the hypothesis, which he communicated to Needham, and that they afterwards, both of them performed experiments for the purpose of establishing and illustrating it. It would appear probable, from the above date, that Needham's letter actually appeared before the first part of Buffon's Natural History, which was published in 1749. Haller's account of the hypothesis is in El. Phys. xxix. 1. 13, 4. It is very fully discussed by Spallanzani in the first volume of his "Opuscules de Phys." where he controverts Needham's reasoning, and endeavours to prove that his experiments were incorrect, or at least inconclusive. Buffon's account of his hypothesis in his Nat. Hist. v. ii. part 2. c. 4..8.

[2] See Haller, El. Phys. xxix. 2. passim, sed precipue in § 5. Spallanzani,

It is perhaps not very easy to determine who it was that first proposed the hypothesis of pre-existing germs [1], but it is to the arguments and experiments of Bonnet, Haller, and Spallanzani, that it was principally indebted for the favourable reception which it met with in the middle of the last century. The first of these authors seems to have embraced it from an impression of the inadequacy of the other hypotheses to account for the facts, for we do not find that anything is urged by him in its favour, except those general considerations which were founded upon its comparative probability. He conceived that no mechanical or chemical operation, with which we are acquainted, bears the least analogy to the power by which a body is originally formed and organized, and therefore, in order to avoid this difficulty, he concluded, that when the first animal was created, the creation of all its future offspring took place at the same time, and that nothing farther was afterwards necessary but to evolve, or put into action, those beings which were previously existing in a dormant state [2]. A great defect in this mode of reasoning is that it is entirely founded upon our ignorance; and yet, at the same time, it so far presumes upon our perfect acquaintance with the subject, as to entitle us to employ a supposition perhaps more extraordinary than any which had ever been advanced in physiology. And this is the less excusable in this instance, because the whole of Bonnet's reasoning, as originally brought forward by him, rested not upon any direct facts or experiments, which he adduced in its favour, but solely upon its accordance with the general principles which direct the operations of nature, and upon the circumstance of its not involving any of those absurd or improbable positions which he thought were attached to the speculations of Buffon and Needham.

Opus. de Phys. t. ii. was at considerable pains to detect the sources of Buffon's error; he seems to have clearly proved that at least one of them depended upon Buffon having mistaken the animalcules which are produced in putrescent fluids for the seminal animalcules.

[1] Some of the older physiologists maintained that the rudiments of the fœtus were principally derived from the female ovum, this was the case with Malpighi and Harvey, and the opinion is very elaborately defended by Valisneri in the second part of his treatise " Della Generazione."

[2] Bonnet defines the germ " un corps organizé reduit extrèmement en petit;" he supposes that there is, in all cases, " un fond primordial dans lequel les atomes nourriciers s'incorporent ou s'incrustent, et qui détermine par lui-même l'ordre suivant lequel ces atomes s'incrustent et l'espèce d'atomes qui doivent s'incruster. Je suppose par-tout que ce fond primordial pre-existe dans le germe;" Œuvres, t. vii. p. 295. Palin. Part 11. c. 3. See also Mém. sur les Germes, Œuvr. t. 5. p. 1 et seq. This hypothesis obtained the appellation of " Emboitement;" Œuvr. t. iv. p. 273. note 3; Tabl. des Considerations, § 71 et alibi. Spallanzani always defends the opinion that the germ contains every organ that is subsequently found in the fœtus. Bonnet's account of his hypothesis and his defence of it against the objections of his opponents, will be found in his " Considérations sur les Corps Organ." ch. 58, 9. and in his " Contemplation," par. 7. chap. 8, 9, 10. There is a prolixity and diffuseness in the works of this author which make it difficult to obtain a correct knowledge of his opinions.

The arguments that were adduced by Haller in favour of the doctrine of pre-existing germs are of a different description; depending upon a very ingenious inference which he deduced from his observations on the gradual evolution of the chick in ovo[1]. He remarks that the greatest part of the matter which constitutes the egg, is obviously the production of the female, and that although the fœtus, when it first becomes visible, is extremely minute, yet that, in its earliest stages of existence, it must have been very much more so, and that we can assign no limits to its minuteness, except those of the imagination. From a careful examination of the structure of the chick, and the mode in which it is connected with the different parts of the ovum, Haller concludes that the membrane of the yelk is continuous with that of the intestine of the chick, and that they are in fact parts of the same substance; hence it follows, that as the yelk existed in the egg before impregnation, the intestine, and consequently the embryo generally, must have done so likewise[2]. The accuracy of Haller's observation, as far as the anatomical fact is concerned, has not, I believe, been called in question, viz. that the vessels of the chick are continued into certain parts of the yelk, constituting them portions of the same organized substance. The fact was eagerly seized by Bonnet, as a most powerful argument in favour of his hypothesis, and regarded as the most complete triumph over his opponents, and indeed it was generally regarded by all the contemporary physiologists, as a very convincing proof of the pre-existence of the germ in the uterine system of the female, independent of the congress of the male.

We may, however, remark upon this argument of Haller's, that it is not until the embryo has attained a certain size that this continuation can be observed, so that its existence before this period is an assumption, which does not form a part of the anatomical fact, nor is it a necessary consequence of it. It is quite consonant to what we daily observe in the operations of the animal œconomy, that the vascular continuation, which was described by Haller, should be produced by a connexion between the two parts in the early stage of the existence of the fœtus, the vessels of the fœtus extending themselves into the previously organized substance of the yelk. This circumstance is less extraordinary than that two surfaces, not originally belonging to each other, should be able to form a vascular connexion, merely by being placed in accidental contiguity, because we must conceive that, in the former case, although the yelk and the chick had no original communication with each other, yet that the parts are so constructed, that a provision is laid for the communication afterwards taking place. Under these circumstances, there is nothing singular in the vascular connexion between the two bodies being produced in the most complete manner, so as to render them, in the ordinary acceptation of the term, parts of the same structure; but it does not necessarily follow that they were so originally. It is to be remarked, that Haller himself explains the union of the ovum with the internal surface of the uterus upon this principle.

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[1] In El. Phys. xxix. 2. 7. we find a summary of the arguments which induced Haller to embrace this hypothesis; in the two next sections he candidly states the difficulties which attend it, yet in spite of these circumstances, he seriously argues in its favour. The subject is farther considered and additional arguments brought forward in the paragraphs 27..37; see also Op. Min. t. ii. p. 399 and 418.

[2] Senebier gives a view of Haller's experiments and the deductions from them in the Introd. to Spallanzani, Opusc. de Phys. p. xcii. et seq. See the experiments in Haller, Op. Min. t. ii. passim. It is curious to compare the earlier opinions of Haller on this subject, as they are stated in his Prim. Lin. § 881 et seq., with those which he afterwards adopted. Even after making his observations on the chick in ovo, he at first only spoke of them as favouring the hypothesis of pre-existing germs, nor was it until after Bonnet had adopted them with so much zeal, that Haller himself regarded them as decisively proving the doctrine; see the remarks of Blumenbach, de Gen. p. 31 et seq. and Inst. Phys. § 584, 5.

Spallanzani's support of the hypothesis of pre-existing germs is principally derived from his microscopical observations, which on this, as on so many other occasions, he appears to have prosecuted with unwearied industry, and the results of which, we have every reason to believe, he has related with perfect fidelity. The facts which he brought forward may be reduced to two heads, those in which he shows that the ova of animals exhibit precisely the same appearances before and immediately after impregnation, and those in which he made observations similar to Haller's in which he attempts to prove, that at the very earliest period when we can behold any trace of a fœtus, it possesses a complete vascular connexion with those parts of the ovum which must evidently have pre-existed in the female. But, however valuable we may consider these observations, as exhibiting to us the condition of the fœtus in its very earliest stages of existence, all beyond this is merely conjectural.

Spallanzani remarks, that the size of the fœtus, when it first becomes visible, is such as to prove that it must have previously existed for some time, although, in consequence of its transparency, it could not be perceived. Hence he argues, that as it had existed for a certain length of time in an invisible state, it may have done this for an indefinite period, and draws the general conclusion, that the circumstance of a part not being visible, is no proof of its non-existence[1]. This position is certainly true in the abstract, but it is a principle which we must employ with caution, and we must bear in mind, that it requires a high degree of antecedent probability, and the concurrent support of various independent considerations, before we can admit of the existence of a body, which we are, under no circumstances, able to detect.

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[1] Haller remarks, that when the chick first becomes visible, it is a line in length, and justly infers that its not being sooner visible, does not depend upon its minuteness but upon its pellucidity; El. Phys. xxix. 1. 26. The extreme minuteness of the parts, and the consequence deduced from this, of their existing before they are visible, are insisted upon by Spallanzani in various parts of his works on Generation.

We have a series of curious experiments by Spallanzani on artificial fecundation, which were regarded by himself and by Bonnet as affording a strong confirmation of their favourite hypothesis. Having ascertained that in many of the oviparous quadrupeds impregnation takes place out of the body of the female, he was induced to try whether he could not effect the operation, by applying a portion of the seminal fluid to some of the detached ova. He found that the quantity which was necessary was very small, and upon diminishing it in order to obtain the minimum, he diluted the semen with water until the mixture contained not much more than $\frac{1}{3000}$ of the secretion, when he found that a drop of this diluted fluid was capable of producing impregnation[1]. But he contended that it was impossible that so minute a quantity of the seminal fluid could afford any actual addition of matter to the ovum, and that it could only operate as a stimulus to the heart, or to an eminently contractile part, and thus produce a commencement of that train of actions which constitute the proper life of the fœtus[2].

It must be admitted that these results of Spallanzani's are curious and unexpected; but, although we might have supposed, that a considerably greater quantity of matter from the male would have been required to have produced impregnation, there can be no limits assigned to the powers of nature on this subject, nor have we any analogy to guide us in the opinions that we may form respecting it. As far, therefore, as this point is concerned, it becomes a question of relative probability, whether it be more difficult to conceive of this minute particle of matter from the male uniting with a certain part of the ovum appropriated to this purpose, with the magnitude of which we are totally unacquainted, and thus constituting the fœtus, or whether we are to prefer the "emboitement," as it has been styled, of Bonnet, where hundreds and thousands of individuals lie one within the other, each of which possesses a complete series of organized parts, where, in many cases, the rudiments of the second generation alone are visible, and this only so by the aid of the most powerful microscope.

But it is not merely a question of relative probability. I have already observed, that it seems impossible to reconcile the hypothesis of pre-existing germs with the resemblance which the fœtus bears to the male parent, or with the phenomena of the production of hybrids; to account for the operation of accidental circumstances upon the fœtus during pregnancy, or for the power which many animals possess of repairing lost parts, and of being multiplied by division, unless we suppose that there is in the animal itself a formative power, a faculty of producing what did not previously exist, and, if this be admitted to be necessary in one instance, we destroy the ground upon which the whole structure rests. From these considerations, I feel authorized in concluding, that the hypothesis of pre-existing germs is deficient in proof, is inconsistent with acknowledged phenomena, and is antecedently in the highest degree improbable[1].

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[1] Experiences sur la Generation, Mém. ii. p. 125 et seq.

[2] A similar difficulty attends the generation of the warm-blooded oviparous animals; Harvey maintains, as the result of his own observations, that there is no perceptible difference between an impregnated and an unimpregnated egg; Ex. 6, p. 22 et seq. And the same opinion is implied in the remarks of Fabricius.

Blumenbach appears to have been the first physiologist who was strongly impressed with the difficulties and contradictions of this hypothesis, and his reasoning tended materially to its downfall[2]. Yet he agreed with Bonnet and his friends in the idea that the ordinary powers of vitality are not adequate to explain the process of generation; and he therefore, according to his usual custom, ascribed it to the operation of an imaginary agent, which he created for the purpose of executing this particular office, and denominated the nisus formativus[3]. The celebrity which is so justly attached to the name of Blumenbach, and our respect for every thing which issues from his pen, can alone render this speculation deserving of our attention. It will be sufficient to remark concerning it, that it affords an instance of that incorrect method of introducing new terms into science, which as they do not express the generalization of facts, throw no real light upon the subject in question, and which must therefore retard the progress of knowledge, by inducing the mind to remain satisfied with the acquisition of a new language, without having acquired any new ideas. The error with which Blumenbach is chargeable, consists not in his having failed to give a satisfactory explanation of the difficulty, which probably the present state of our knowledge will not allow us to do, but upon his having proceeded upon a wrong principle in the mode in which he has made the attempt[1].

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[1] The principal arguments in favour of the doctrine of pre-existing germs are, 1st, the difficulty of conceiving of the original formation of an organized body, as no one part can exist without the simultaneous existence of other parts; 2, the fact that several successive generations can be actually detected in certain classes of animals and in plants; 3, the visible evolution of the fœtus, which can in some cases be distinctly traced; 4, the extremely minute quantity of male semen which is sufficient to produce impregnation; 5, the analogy of the various species of inferior animals, where there is no co-operation of the male; 6, that oviparous animals produce eggs, and plants seeds, without the co-operation of the male, which, although not fertile, do not differ in their physical structure from those that are impregnated; and, lastly, the anatomical arguments of Haller, founded upon the mode in which the yelk of the egg is connected with the viscera of the chick. The objections to the doctrine are the difficulty of conceiving how the ovaria of the first created female could contain the germs of all her descendants; the phenomena of hybrids, the resemblance which, in certain cases, the offspring bears to the male parent, the existence of monstrosities of various kinds, and the re-production of mutilated or lost parts.

[2] Essay on Gener. sect. 2, where the subject is treated with singular acuteness.

[3] Inst. Phys. sect. 40. p. 333..9; also De Gen. Hum. Nat. Var. sect. 2. § 33. p. 82 et seq. Those very curious cases, in which certain organized bodies have been found in the ovaria of unimpregnated females, have been supposed by some physiologists to be a proof of the existence of a certain generative power in the female, which may be referred to the nisus formativus. But I think that this hypothesis affords no real explanation of the facts in question, nor am I acquainted with any which throws any light upon the process. Cases of this kind are not uncommon, and there appears to be no doubt of their occurring in unimpregnated females; the following may be adduced as specimens; Haller, El. Phys. xxx. 1. 14. and Phil. Trans. for 1745, p. 71 et seq.; Baillie, in Phil. Trans. for 1789, and Morbid Anat. p. 293.. 5. and series of engravings, p. 199. Fasc. 9. pl. 7. fig. 1. and works by Wardrop, v. i. p. 137, 142, v. ii. p. 348. The Edin. Journ. Med. Scien. v. ii. p.

From these considerations it will follow that we must have recourse to that modification of the hypothesis of Epigenesis which supposes the fœtus to be formed, in the first instance, by matter derived from each of its parents, and that the germ, when thus produced, continues to receive additional matter, for a certain period, from the mother alone, until its own functions are sufficiently established to enable it to absorb and secrete what is necessary for its growth and increase[2]. It must be admitted that the original congress of the particles, by which the first rudiments are brought together, is an operation which is not analogous to any other with which we are acquainted in the animal œconomy, but it would appear that the subsequent stages do not essentially differ from the ordinary processes of vitality.

Although the hypothesis of Epigenesis does not afford us any satisfactory explanation of the generative function, it is the only view of the subject which we can take, that does not involve some position either absolutely contradictory to the laws of nature, or, which appears in the highest degree improbable, if not altogether beyond our conception[3].

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227 et seq. contains an account of four cases that were examined by Velpeau.

[1] See Comment. Soc. Reg. Scien. Gotting. t. viii. p. 41 et seq. also Essay on Gen. sect. 3. The very circumstance which Blumenbach seems to consider as the essential merit of his hypothesis, its uniting physical with what he terms teleological principles, in the formation of a physiological hypothesis, is in fact its radical error; see his Inst. note to § 587. His account of what takes place in the female organs, when disencumbered of hypothesis, is probably correct, and contains all that we actually know upon the subject; see § 588 et seq. I may refer my readers to the judicious remarks of Dr. Prichard on Blumenbach's hypothesis; Essay on the Vital Prin. note 4, p. 213 et seq.

[2] For Blumenbach's definition of the process of Epigenesis, see p. 5 of his essay.

[3] Our opinions on the hypothesis of generation must be, to a certain extent, influenced by the observations which have been lately brought forwards on the progressive formation of the fœtus, to which I have already had occasion to refer. According to this view of the subject there is not merely a de-

There is one hypothesis, on which it remains for me to offer a few observations,—an hypothesis which, although now very generally exploded, was at one period, almost universally prevalent; I refer to what has been termed spontaneous or equivocal generation [1]. It is sufficiently obvious, that in the higher orders of animals generation never takes place except by means of a parent; but it was contended, that in many of the lower tribes this intervention is not necessary, but that, under certain circumstances, an animal is formed from matter not previously organized. The proofs of this position were supposed to be both numerous and palpable, in the appearance of animals in various situations, where it seemed to be impossible to account for their production in the ordinary mode of generation.

A variety of experiments were performed upon this subject about the conclusion of the seventeenth and the earlier part of the eighteenth century, in which the Italian naturalists took the lead [2], and they very satisfactorily proved, that in many cases of what had been conceived to be spontaneous generation, it was possible to point out the means by which the ova of the young animals had been deposited, and to prevent their appearance. Yet still considerable difficulty remains on this subject, and there are many individual instances, which it is difficult to explain, where we are either obliged to confess our ignorance, and wait until some new light be thrown upon the subject, or to suppose that the analogy which we employ so frequently in physiology is here not applicable. The argument against equi-

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velopment or extension of parts previously existing, but an addition of absolutely new organs, of which there were no rudiments or preparatory structure, and of course nothing analogous to what is conceived to constitute the pre-existing germ. On this subject it may be sufficient to refer to the remarks of Dutrochet, in Mém. Soc. d'Emul. t. viii. p. 62, 3.

[1] Fabricius commences his treatise "De Formatione Ovi," by observing, "animalium autem fœtus, alius ex ovo, alius ex semine, alius ex putri gignitur . . . . ;" and it would appear that Harvey admitted of this doctrine by the following observation, as well as others of a similar tendency: "sive ab aliis generantibus (animalia) proveniunt, sive sponte, aut ex putridine nascuntur;" De Gen. p. 385.

[2] Redi appears to have been among the earliest physiologists, whose experiments on this subject were performed with the due degree of accuracy; he clearly pointed out the source of the ova of many animals, which had been previously ascribed to spontaneous generation. We are indebted to Müller for a very complete account of the infusory animals; see also Haller, El. Phys. xxix. 2. 12. and art. "Animalcule," in Brewster's Encyc. The experiments of Trembley, Saussure, and Spallanzani, have added many important facts to our knowledge respecting the production of these animalcules. It would appear that, after the doctrine of equivocal generation was given up, it was still the opinion of some distinguished naturalists, that the ova of animals and vegetables were convertible into each other. Needham supported this doctrine, founding it upon some experiments of Baron Munchausen, (a somewhat inauspicious name,) who is said to have produced animals from the seeds of funguses, and funguses from the ova of animals. It is even asserted that Linnæus, for some time, gave credit to these experiments; Spallanzani, Opusc. t. i. p. 165. The same doctrine is maintained by Dr. Grant; see Brewster's Journ. v. viii. p. 110.

vocal generation is, however, merely analogical, and therefore can have but a certain degree of strength to whatever extent it be carried; negative experiments on such a subject, although of little avail when put in competition with positive results, yet become important if sufficiently multiplied and varied.

The cases to which I more particularly refer are the various instances of intestinal worms, and still more of the seminal animalcules. The appearance of the former may perhaps be explained upon the supposition, that their germs are contained in our food, and that they are conveyed into the intestinal canal, and developed there, as being the situation specifically adapted for their subsistence. But we cannot extend this explanation to the production of the seminal animalcules; it does not seem possible to conceive that their ova can have entered the blood, passed through the secretory organs, and been finally deposited in the testes, without admitting a series of events, which appear as difficult to comprehend as the actual formation of the bodies in the situation where they are found to exist. Upon the whole, it will be prudent to regard this as one of those mysteries which the present state of our knowledge does not enable us to explain, or even to comprehend[1].

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[1] See the observations of Lamarck, Anim. sans Vert. t. i. p. 178; in this, as well as in other parts of his great work, he advocates the hypothesis of spontaneous generation; he conceives that his first order of animals "Infusoires nus," are produced in this mode; see remark in p. 432, 3, also t. iii. p. 140, 1, for his account of Intestinal Worms; also his Recherches sur l'organization des Corps vivans, p. 100 et seq. Adelon admits the possibility of spontaneous generation; Physiol. t. iv. p. 2. Rudolphi also defends this hypothesis in the introduction to the Entozoorum Hist. Nat. See also the remarks of Bourdon, and St. Hilaire; Ann. Sc. Nat. t. xvii. p. 153. Cuvier, on the contrary, when speaking of the existence of the various species of entozoa, which are sometimes found even in compact cellular texture, remarks that many persons suppose them to be produced spontaneously, in consequence of the difficulty of accounting for their mode of entrance into these parts. But, he adds, as some of them are known to produce ova, or to bring forth young, it is more probable that they proceed from germs, so small as to be admitted through the most minute passages, or even that "les jeunes animaux où ils vivent en apportent les germs en naissent;" Regne Animal, t. iv. p. 27. Virey also decidedly opposes the doctrine of spontaneous generation; Dict. Scien. Méd. art. "Generation," t. xviii. p. 10, 2. and Senebier, in his translation of Spallanzani's Opusc. de Phys. speaks of it as having been completely refuted by microscopical observations, Introd. p. lx. The late investigations of Ehrenberg on the Infusoria have clearly proved the existence of generative organs in these animals, which were previously supposed not to be provided with them. Analogy may induce us to conceive that a similar discovery may be made with respect to other animals, in which these organs have not been detected, but it will be prudent to suspend our judgment until farther observations have been made. See Ann. Sc. Nat. t. i. (2d ser.) p. 129 et seq. See also the remarks of Dr. Willis, in the Cyc. of Anat. v. i. p. 122, note. In connexion with this subject I may refer to an elaborate paper by Dujardin, entitled "Recherches sur les organismes inférieurs," and especially to a quotation from Müller, p. 363, 4. Ann. Sc. Nat. t. iv. (2d ser.) p. 343 et seq.

APPENDIX TO CHAPTER XII.

collected in this Appendix some of the principal passages in the
Leeuwenhoek, where he gives an account of his successive disco-
specting the seminal animalcules; I have thought it desirable to refer
in this connected form, in order that any one may readily learn what
ally the opinion of Leeuwenhoek on this point. Opera, t. i. p. 49
an epistle to Wren, he describes the animalcules as found in the
the frog, gives a plate of them, and estimates their thickness at
of a human hair. P. 1 et seq. (2 ser.) in a letter to Grew he de-
hem as seen in fishes, in a hare, a dog, and a rabbit; it is in this
8, that he informs us that Hooke exhibited them to Charles II., a
ance which is only important, as it proves that Hooke gave full credit
scovery. Leeuwenhoek gives a very decided opinion concerning the
hese animalcules; he remarks, p. 6. "testiculos nullum alium usum
nisi ut animalcula in illis formata, tamdiu in illis sustententur,
d ejaculationem sint idonea." With respect to their size he remarks,
000 of them may exist in a space not larger than a grain of sand, and
melt of a cod may contain 15,000,000,000,000,000. P. 24 and p.
., in two letters to Hooke, we have a further account of them; in a
dressed to the R. S., p. 149 et seq., he attempts to show how the ani-
is converted into a fœtus, and he goes so far as to suppose, p. 163,
possess different sexes, and thus give rise to the sex of the future
an opinion which he supports in a letter to Wren, t. ii. p. 28. In p.
gives the figures of the animalcules from a rabbit. In a letter to
ii. p. 26 et seq., he argues against the ovarian hypothesis of genera-
maintained by De Graaf and Harvey, and also in a letter addressed to
., p. 398, and in one to Garden, p. 400. When pressed by his oppo-
explain what he conceived to be the use of the ovarium in the mam-
e replies by the following question, p. 414; "cui enim usui inser-
bis cognitæ papillæ in quadrupedis masculinis? Imo etiam cui usui
illæ quas viri in pectore gerunt?" In the 3d vol. we have farther re-
nd observations in a letter to Van Zoelen, p. 57 et seq., to which I
eady referred; in a letter addressed to the R. S., p. 96 et seq., where
ats some objections of Lister; in an epistle to Sloane, p. 281 et seq.;
ddressed to the R. S., p. 304 et seq., where he again insists upon the
inary minuteness of the animalcules; in an epistle to Ld. Somers, p.
eq., where we have figures of the animalcules from a common fowl,
iffer a little from some of those previously given. In the fourth vol.
an epistle to Leibnitz, p. 164 et seq., in which he refers to the objec-
Valisneri, also to those of another author, whom he does not name:
ame letter he informs us that Boerhaave assents to the existence of
alcules. In a second epistle to Leibnitz, p. 206 et seq., he opposes
ian hypothesis, and states that he has frequently examined the fallo-
es of dogs and rabbits after coition, and has seen the animalcules, in
, but never an ovum. In a third letter to Leibnitz, p. 287 et seq., he
o the opinion referred to above, that he could observe a difference of
he animalcules, upon which depended the future sex of the fœtus.
olume we have four letters to Boerhaave, in which he discusses differ-
nts connected with the form, size, or state of the animalcules; he
s that he has been able to detect a difference in them according as
in a more or less perfect state of growth, p. 281, 305. I have

referred to Leeuwenhoek's works in their collected form, rather than to the letters or communications which successively appeared in the Phil. Trans. or other periodical works. Haller, in his Bibl. Anat. t. i. p. 606..613, gives an account of all Leeuwenhoek's successive publications; he states that the earliest notice of the animalcules is contained in a paper inserted in the Phil. Trans. No. 142, (called by mistake No. 143,) written in 1667. I have subjoined a list of some of the most respectable authors, who have given their full assent to the accuracy of Leeuwenhoek's descriptions, some of them, as it would seem, from personal communication with him, or from their own observations.

Valisneri's account of his own observations, and those of Lancisi, Della Gener. par. 1. cap. 2. Op. t. ii. p. 105 et seq. tab. 18. fig. 14; Huygens, Diop. prop. 59 et Op. Rel. t. 1. p. 176; Andry, De la Gener. des Vers. t. 1. p. 152 et seq.; Morgagni, Advers. 4. p. 9; Baker on Microscopes, v. 1. ch. 16. p. 152 et seq.; Monro, sec. De Test. in Smellie's Thes. t. ii. p. 373; Boerhaave, Præl. § 651. t. v. p^{s}. 1. p. 156, 7, and note in p. 159, 0; Haller, El. Phys. xxvii. 2. 3. Bonnet, in his letter to Spallanzani, inserted in "Opusc. de Phys." t. ii. p. 95 et seq. Morgan, an author of some reputation in the middle of the last century, thus expresses himself, "that all generation is from an animalculum pre-existing in semine maris, is so evident in fact, and so well confirmed by experience and observation, that I know now of no learned men, who in the least doubt of it;" Mech. Practice of Phys. p. 281. I quote this passage to show what may be considered as having been the popular opinion when this work was published, 1735.

CHAPTER XIII.

OF VISION.

s now gone through the functions, which I have styled ctile or physical, such as depend more immediately upon ntractility of the muscular fibre, the result of which is duce either a mechanical or a chemical change, and where tion of the nervous system, although frequently called into se, is not essential to the effect. We must now proceed to er the second class of functions, the sensitive, in which rvous system bears the principal share, being the part ily affected, and where the change that is produced is not arily either of a mechanical or a chemical nature.

sensitive functions may be naturally divided into two s; the first, those that depend upon the operation of a al agent on some part of the nervous system; the second, that are produced by the action of the different parts of rvous system upon each other, where no physical power is liately concerned. As an example of the former, I may ce what are usually termed the external senses, and of the the faculty of sympathy. In both cases a certain change place in an appropriate set of nerves, which is attended onsciousness, while there is a corresponding change in the ium, through the intervention of which the ultimate effect luced.

take the case of the eye; the rays of light pass through iours to the retina, on which the impression is received: the ion is transmitted by the optic nerve to the brain, where a tion of sight is produced, and gives rise to all the various s which depend upon the ideas and feelings that we derive his sense. Upon the same principle, in the second divi- f these functions, a certain affection of one part of the s system leads to an affection of some other part: this on is transmitted to the sensorium, and excites there a f ideas and feelings, which become the immediate cause ious other changes, both mental and corporeal. To the these classes we may give the title of the physico-sensi- nctions, and to the second that of the simply-sensitive ns.

physico-sensitive functions may be again subdivided into where the physical cause consists in some external agent, ndent of the system, but which acts directly upon it; and, ly, where the physical cause is itself necessarily connected he system, depending upon its previous condition, or some antecedent train of actions. The most important of the first class constitute what are termed the five senses, in which light, the undulations of the air, the effluvia of odorous, those of sapid substances, and the contact of a solid body, are respectively the physical causes employed.

The second subdivision consists of certain perceptions, where the cause producing them, although of an appropriate and specific nature, is not derived from any external agent; such, for example, are the feelings of hunger and thirst, which depend respectively upon certain states of the stomach and fauces, and that which attends the motion of the joints, where the contraction of the muscular fibre produces a certain impression upon the nerves, which is conveyed to the sensorium.

What I have termed the physico-sensitive functions agree not only in the circumstance of their having each of them a physical cause, which is necessary for their production, but also in this cause being of a specific nature, appropriated to each particular function. They have likewise each of them an instrument, constructed for the express purpose of receiving the impression of this physical cause, and being acted upon by it, and which, as well as the cause itself, is exclusively appropriated to each particular function, constituting what are termed the organs of sense. Sight is the appropriate cause of vision, and no organ except the eye can receive the impression of light; the undulations of the air are the appropriate cause of the sensation of sound, while, at the same time, no organ of the body except the ear can receive the impressions of these undulations.

The effects which are thus produced upon the sensorium, through the intervention of the different organs of sense, as they agree in the mode of their production, have received a common appellation. Different terms have been employed for this purpose, but the one which, upon the whole, appears to me the most appropriate, is perceptions of impressions, as this is expressive both of their origin and of their operation [1].

In giving an account of the external senses, I shall begin with that of vision, as it produces the most extensive class of our perceptions, while, at the same time, we are the best acquainted both with the action of the exciting cause, and with the mechanism of the organ by which it operates. The remarks upon vision will be arranged under three heads; I shall, in the first place, give a brief description of the eye, its structure, the connexion of its different parts with each other, and the uses which

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[1] See Reid on the Human Mind, p. 185, and on the Intellectual Powers, Ess. 2. ch. ii. p. 79..82. They are denominated by Locke, Ideas of Sensation, Essay, Book 2. passim; by Hume, Impressions, Essays, v. ii. p. 31; by Hartley, Sensations, on Man, Introd. p. 1. I shall refer my readers in this place to the philosophical work of Blainville, "De l'Organization des Animaux," the principal object of which is to give an account of the comparative physiology of the different senses in the various classes of animals. It treats in successive chapters of the organs of touch, taste, smell, sight and hearing.

ve been supposed to serve. In the second place, I shall
me observations on the nature and cause of vision, on
de in which light acts upon the eye, so as to produce the
te effect, first upon the retina and subsequently upon the
ium. Having thus become acquainted with the direct
of vision, we must, in the third place, inquire into the
ion which subsists between the perceptions of sight and
f the other organs of sense, as well as into the modifica-
which are produced by association and sympathy, giving
those curious and important affections, which may be
the acquired perceptions of vision.

SECT. 1. *Description of the Eye*[1].

eye may be regarded as an optical instrument, which
s of three orders of parts. The first are those by which
ys of light are received, and so far modified, as to render
ubservient to vision; the second are certain productions
nervous system, which receive the impressions of light,
nvey them to the sensorium, while the third are a number
essory parts, which preserve the eye in a state proper for
rformance of its functions, and enable it to execute them
most perfect manner.

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ong the numerous descriptions of the eye that have been published,
wing may be selected, as probably being the result of actual observa-
allopius, Instit. Anat. Op. t. i. p. 454..6; Fabricius, De Oculo, Op.
t seq.; Kepler, Paralipomena, cap. 5. p. 158..168; Briggs, Ophthal-
hia; Cheselden's Anatomy, ch. 6. p. 290 et seq.; Winslow's Ana-
ect. 10, art. 2. v. ii. p. 284 et seq.; Porterfield on the Eye, v. i. book
rhaave, Prælectiones, § 508 et seq., t. iv. p. 44 et seq.; Cowper, Anat.
lum. tab. 11; Camper, De quibusdam Oculi Part.; Haller, El. Phys.
sect. 2; Oper. Min. t. iii. p. 218, et Arter. Oculi. Hist. cum. tab. in
Anat. Fas. 7; Warner's Description of the Eye; Zinn, Descrip.
culi Hum.; et in Comment. Gott. t. iv. p. 192; Sœmmering's elabo-
n. Oculi Hum. and the transcript of the same by Caldani, pl. 93..5;
Sœmmering, de Oculis Hominis Animaliumque Commentatio; Blu-
h, Instit. Phys. sect. 17. § 255..268; Monro's (sec.) Three Treatises;
Anat. Descript. t. ii. p. 416 et seq.; Cuvier, Leç. d'Anat. Comp. No.
p. 264 et seq.; Young's Lect. No. 38. v. ii. p. 447 et seq.; Bell's
iii. part 2. book 1. p. 224 et seq.; Monro's (tert.) Elements, part vi.
ect. 2. p. 392 et seq.; Travers on the Eye, p. 1..44; and Cloquet,
. 337..362. To these references may be added the beautiful figures
Bauer, in Phil. Trans. for 1822, pl. 6..12, in which many points
ed with the peculiar anatomy of the organ are admirably illustrated.
o Cloquet, Man. pl. 139..143, principally taken from Sœmmering.
account of the circulation of the eye and its vascular connexions, I
er to an elaborate essay by Ribes; Mém. Soc. d'Emul. t. viii. p. 631
I may also recommend to the attentive perusal of the student of
ogy the 6th chapter of Dr. Roget's Treatise, which contains an admi-
count of the anatomical structure and physiological relations of the
f vision. Ehrenberg has extended his microscopical researches to the
a and the echinodermata, the eyes of which, with their accompanying
he has detected; Ann. Sc. Nat. t. v. 2d ser. p. 299..1.

What is termed the globe or ball of the eye is a body of nearly a spherical form, which is composed of three transparent substances, styled humours, enclosed in membranes of suitable strength and thickness to preserve the form of the organ, and to attach it to the neighbouring parts. The vitreous humour constitutes the main bulk of the globe[1]: its consistence is nearly that of the white of the egg, but it seems to be composed of a fine tissue of membranous cells, in which a slightly albuminous fluid is deposited[2]. There is a depression in its anterior part, in which is lodged the second of the humours, the crystalline. This is a body of considerable density and firmness, having the form of a double convex lens, which is placed perpendicularly behind the aperture of the pupil, so that all the rays of light which enter the pupil must necessarily pass through the crystalline. By maceration in water, it is found to be composed of laminæ, which progressively increase in density from the circumference to the centre[3]. A large proportion of its solid contents are stated by Berzelius to consist of a peculiar matter, which contains neither albumen, nor jelly, nor muscular fibre, and which, except in the absence of colour, has all the properties of the red particles of the blood[4].

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[1] Petit informs us, that the weight of the human eye, deprived of the muscles and fat, is 142 grains; the vitreous humour being 104 grains, the aqueous and crystalline each four grains, and the membranes 30. In the eye of a boy these numbers were 132, 95, 4, and 29; in an ox, 615 the whole, 360 the vitreous humour, 38 the aqueous humour, the crystalline 52, and the membranes 165; Mem. Acad. pour 1723, p. 38 et seq., and pour 1728, p. 206 et seq. See also Porterfield on the Eye, v. i. p. 132..4; and Haller, El. Phys. xvi. 2. 18. Winteringham made some accurate experiments on the form and dimensions of the eye of the ox; Exper. Inq. Ex. 53. § 1, 2, p. 282..4. In Martin's Phil. Brit. we have measures of the different parts of the eye, v. iii. p. 26 et seq.

[2] Porterfield on the eye, v. i. p. 242..5; Blumenbach, Inst. Physiol. § 264; Bell's Anat. v. iii. p. 314. The humours of the eye have been successively analyzed by Chenevix, Phil. Trans. for 1803, p. 195 et seq.; Nicholas, Ann. Chim. t. liii. p. 307 et seq., and Berzelius, Med. Chir. Tr. v. iii. p. 253..5. See also the remarks of Dr. Young, Med. Lit. p. 521, 2.

[3] Chenevix found the external part of the crystalline of an ox to have the specific gravity of 1·1940, while the specific gravity of the whole was not more than 1·0765, ubi supra; see also Young's Lect. v. i. p. 448. Winteringham found the specific gravity of the whole crystalline, compared to that of the internal part, to be nearly as 26 to 27; Exper. Inq. p. 239.

[4] Med. Chir. Tr. v. iii. p. 254, 5; this opinion, is, however, directly opposed to that of Chenevix. His experiments would lead to the conclusion, that the humours all consist of water, united with different proportions of albumen, the aqueous and vitreous also containing a portion of muriate of soda, which is not found in the crystalline. He states the specific gravity of the three humours of the eye to be, the aqueous 1·0053, the vitreous the same, and the crystalline 1·0765, ubi supra; see also Porterfield on the eye, v. i. p. 232. Monro (sec.) informs us, as the result of experiment, that the crystalline is more refractive than its specific gravity would indicate; its power of refraction is stated to be midway between that of glass and water; Three Treatises, p. 86. An account of all that was known respecting the structure of the crystalline, its composition, figure, dimensions, specific

Before the crystalline is lodged the aqueous humour, consisting almost entirely of water, holding in solution a small quantity of saline matter, with a trace of albumen, being perhaps the only substance to which the term humour strictly applies. The iris is altogether immersed in the aqueous humour, which it divides into two unequal parts or chambers, as they have been styled; the anterior, or the one between the iris and the cornea, being considerably more extensive than the posterior, or that which lies between the iris and the crystalline[1].

The greatest part of the globe of the eye is inclosed by a strong, dense, opake membrane, termed the sclerotic coat; at the fore-part it is wanting, and the space thus left is occupied by the cornea, a transparent membrane, which, composing a greater part of a smaller sphere, produces more or less prominence of this part of the globe. The cornea possesses a laminated texture, and is provided with a greater number of blood-vessels than the sclerotic. Within the sclerotic lies the choroid coat, a membrane which is considerably thinner, and more vascular. The choroid is also wanting at the fore-part, leaving a circular opening, to which is attached the iris or uvea[2], the colouring ring that surrounds the pupil. The choroid is lined with an expansion of nervous matter, termed the retina, and this is connected with the optic nerve, which passes from the posterior part of the globe to the central portion of the brain. The convexity of the cornea and the density of the different humours are such, that when parallel rays of light fall upon the cornea, and pass through the pupil, they are brought to an exact focus upon the retina[3].

gravity, refractive power, &c. before the time of Haller, may be found amply detailed in his notes to Boerhaave, Prælectiones, § 527, t. iv. p. 92..4; also in El. Phys. xvi. 2. 19..21. The form and structure of the crystalline was a topic which was very minutely investigated by Petit, Mém. Acad. pour 1730, p. 4 et seq.

[1] The comparative size of these chambers has been made the subject of much discussion, principally as affecting the operations which are occasionally performed upon the eye. Petit bestowed much attention upon this subject; he states the comparative sizes to be 2.5 and 1.6; see his papers in Mém. Acad. pour 1723, p. 38 et seq. and pour 1728, p. 289 et seq. Haller also made it the subject of experiment; Notæ ad Boerhaave, Prælect. § 526. t. iv. p. 89, and El. Phys. xvi. 2. 23. See also Porterfield on the Eye, v. i. p. 146, and Bell's Anat. v. iii. p. 309, 0. In Sœmmering's 8th plate we have perpendicular sections of the eye, so as to present a view of the form of these chambers.

[2] There has been some uncertainty in the language of physiologists respecting the appropriation of the terms iris and uvea, but the most correct method seems to be to consider the iris as the anterior, and the uvea as the posterior lamina of the ring which surrounds the pupil. It would appear that the muscular fibres, to be described hereafter, are found in the iris alone, while the substance, whatever it be, which gives the colour to the eye, is situated either between these parts, or in the uvea. See Bell's Anat. v. iii. p. 263, note.

[3] For an account of the coats of the eye, see Winslow, Anat. sect. 10. art. 2. § 2, 3; Haller, El. Phys. xvi. 2. 5..10; Blumenbach, Instit.

The principal refraction of the rays of light takes place when they enter the cornea; it is increased by their passage from the aqueous to the crystalline humour, and continues to increase until they arrive at the centre of the lens; as they pass on to its posterior part, and when they afterwards enter the vitreous humour, the convergence of the rays is somewhat diminished[1]. We are indebted to Kepler for an interesting experiment, in which, by removing a portion of the membrane from the back part of the eye, and covering the opening with oiled silk, we are presented with an inverted picture of the object towards which the eye is directed[2]. In this process the organ acts altogether mechanically, the rays of light being affected by the humours through which they pass, precisely in the same manner as if they had been transmitted through a succession of physical media of the same density.

Between the choroid coat and the retina there is a thin stratum of a black viscid substance, which is termed Pigmentum nigrum, and is probably a secretion from the vessels that are dispersed over the surface of the choroid. From the experiments of Dr. Young, it would appear to consist of a mucous substance united to a quantity of carbonaceous matter, upon which its colour depends[3]. Its use has been supposed to be to absorb the superfluous rays of light, that might otherwise oppress the sight or render objects indistinct. This effect is illustrated by what takes place in the eyes of certain classes of animals, which are not provided with the pigmentum nigrum, as well as by the peculiar condition of the eye of the albino. In these cases the organ is unable to bear the strong light of day without experiencing uneasiness; while, at the same time, it can discern objects distinctly by a very small quantity of light[4]. Hence we find, that those animals which seize their prey by night, or whose habits lead them to spend their time principally

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Physiol. § 256..263; Bell's Anat. part 2. book 1. ch. ii. v. iii. p. 244 et seq.

[1] The latest, and, we may presume, the most accurate experiments on the comparative refractive powers of the different humours of the eye were performed by Dr. Brewster, in conjunction with the late Dr. Gordon; the refractive power of water being taken at 1·3358, the different parts of the eye were as follows:

Aqueous humour	1·3366
Vitreous ditto	1·3394
Outer layer of crystalline	1·3767
Middle ditto ditto	1·3786
Central part of ditto	1·3990
Whole of ditto	1·3839

Brewster's Journ. v. i. p. 43.

[2] Paralipomena, p. 177, 8; Dioptrice, Prop. 60; Haller, El. Phys. xvi. 4. 2; Porterfield, Ed. Med. Ess. v. iv. p. 126, 7, and On the Eye, v. i. p. 360.

[3] Med. Lit. p. 521. See Haller, El. Phys. xvi. 2. 14, for the opinions entertained by himself and his contemporaries concerning this substance.

[4] Blumenbach, Instit. Physiol. § 274; also De Gen. Hum. Var. p. 276.

in darkness, are either without this substance or have it of a lighter colour[1].

It has been customary with anatomists to class the retina among the coats of the eye, but it properly belongs to the second order of parts, those by which the sensation of sight is received and conveyed to the sensorium commune. It is described as being an expansion of the optic nerve; perhaps, however, it would be more correct to speak of it as an expansion of nervous matter connected with the optic nerve, because the parts differ materially in their structure, and we are not able to demonstrate the actual passage of the one into the other. It consists of a number of fine fibrils of nervous matter, disposed in a reticulated or radiated form, among which are interspersed a minute net-work of blood vessels, so as to be adapted to receive the most delicate impressions of external objects[2].

From the analogy which it bears to the other parts of the nervous system, and from its connexion with the optic nerve, we cannot doubt that the retina is the part on which the impressions of sight are received, or that it is, what has been termed, the immediate seat of vision. It appears, however, that it is not equally sensitive in all its parts, the centre being the most so, and its power in this respect progressively diminishing as we recede from this point. A curious discovery was made by Marriotte, that the portion of the retina which lies over the commencement of the optic nerve, is altogether insensible to the impression of light[1]. From this discovery he deduced the singular hypothesis, that the choroid, and not the retina, is the immediate seat of vision, arguing that in the part of the eye which is insensible, the retina is present while the choroid is wanting. The fact, as stated by Marriotte, appears to be correct, and as the hypothesis coincided with the doctrine which was insisted upon by the Stahlians, as well as by some other sects of physiologists, that the membranes are among the most sensitive parts of the body, this opinion, improbable as it appears, gained many supporters, but it is so entirely at variance with all our notions of the respective uses of these parts, as to be now altogether discarded[2]. The only conclusion which we can draw from the fact is, that the functions of the nervous matter differ according to its mechanical disposition; that when it is in the form of a thin expansion it is better adapted for receiving the impression of objects, and that when it is condensed into a firm cord, it is more suited to the transmission of impressions to the sensorium commune.

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[1] Haller, El. Phys. xvi. 2. 12; Monro's Three Treatises, p. 100; Bell's Anat. v. iii. p. 255, 6. A different view of the subject has, however, been lately taken by Desmoulins; he observes, that the choroid, in different animals, exhibits every variety of colour and shade, and this without any relation to the perfection of their sight; that it is of the brightest colour in many animals that see with the greatest accuracy in a strong light, and that its use may be to reflect the light from the surface of the choroid to the back part of the retina, and thus increase the effect; Magendie's Journ. t. iv. p. 89 et seq. Prof. Müller of Bonn, from his observations on the pigmentum of the eyes of insects, is led to conclude, that there is no relation between its colours and the powers or functions of the eye. Some naturalists have supposed that certain classes of insects are without the pigmentum, but the observations of Müller are not in favour of this opinion; Ann. Sc. Nat. t. xvii. p. 225 et 365; et t. xviii. p. 73. Dr. Roget states that the pigmentum is wanting in certain nocturnal insects; Bridg. Tr. v. ii. p. 490, note. For an account of the colour of the choroid in various animals, see Cuvier, Leç. d'Anat. Comp. t. ii. p. 402. We have a number of interesting observations in Hunter's Essay, "On the Colour of the Pigmentum Nigrum of the Eye," Anim. Œcon. p. 243..253, in which he points out the connexion which is so generally found to exist between the colour of this part and that of the hair and skin, and the relation which this bears to the other functions and structures. I may remark that Hunter had formed a speculation, concerning the effect produced on vision by a light-coloured pigmentum, nearly similar to that subsequently brought forwards by Desmoulins; see p. 252.

[2] The extreme vascularity of the part led to an opinion, which was maintained by some eminent anatomists of the last century, that the retina consists entirely of a net-work of vessels; see Albinus, Anat. Acad. lib. 4. cap. 14. Haller, although he gives a very minute account of this part, does not very explicitly state its fibrous texture; El. Phys. xvi. 2. 15. See also, Zinn, De Oculo Hum. cap. 3. § 3. Monro (sec.) says that it is not fibrous, but that it is composed of a uniform layer of cineritious matter; Three Treatises, p. 93. Fontana has given us the magnified figure of the retina of a rabbit; Sur les Poisons, t. ii. pl. 5. fig. 12. We are indebted to Dr. Knox for many valuable observations on the minute anatomy of the retina; Edin. Phil. Trans. v. x. p. 232..7.

We may presume that the specific and sole purpose of the optic nerve is to convey the visible impressions received by the retina, for it may be presumed, that all the other functions to which the nervous system is subservient are performed by the other nerves, with which the eye is so plentifully furnished[1]. But as is the case generally with nerves which possess specific powers, we do not observe any peculiarity in the structure or fabric of the optic nerve, which could have led us previously to form any conclusion respecting the nature or mode of its action.

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[1] Marriotte's original account of his discovery is contained in a letter to Pecquet, and is inserted in Phil. Trans. v. iii. No. 35. p. 668, for May 1668, also in Mém. Acad. t. i. p. 68, 9. and p. 102, 3. Pecquet's answer is also given, in which he admits the correctness of the fact, as stated by Marriotte, but argues against the conclusion which he deduces from it. Marriotte defends his hypothesis in Phil. Trans. for 1670, No. 59. p. 1023 et seq. For a full account of the subject and the discussion to which it gave rise, see Haller, Notæ ad Boerhaave, Prælect. § 543. t. iv. p. 128, 9, also El. Phys. xvi. 4. 4, 5; Porterfield on the Eye, v. ii. p. 224 et seq.; Priestley on Light and Colours, per. 4. sect. 5. ch. 2; Bell's Anat. v. iii. p. 283..8.

[2] Marriotte's hypothesis was warmly defended by Le Cat, Traité des Sens, p. 166..179. The only advocate of any eminence which it has met with of late years is Priestley, Hist. of Light and Colours, ubi supra, p. 189 et seq. The curious discovery made by Sœmmering, of a foramen in the centre of the retina, has been supposed to throw some light on the experiment of Marriotte; yet it can scarcely be applicable to it, as the size and position of the insensible spot are said not to be exactly coincident with this peculiar structure, and do not correspond with the entrance of the optic nerve into the retina. For the original account of it, see Sœmmering, "De Foramine Centrali Limbo luteo cincto Retinæ Humanæ." Comment. Gottin. t. xiii. 1795, p. 1, et seq. cum fig., and his Icones Oculi Hum. tab. 5. fig. 4, 5, 6; also Sir E. Home's "Account of the Orifice in the Retina of the Human Eye, discovered by Prof. Sœmmering; to which is added Proofs of this Appearance being extended to the Eyes of other Animals." Phil. Trans. for 1798, p. 332 et seq. pl. 17. Dr. Knox has lately confirmed and extended the observation; Mem. Wern. Soc. v. v. p. 1 et seq. and p. 104 et seq. pl. 4., also Edin. Phil. Trans. v. x. p. 233..6.

The third order of parts, those which may be termed accessory or auxiliary, are very numerous, and are adapted to a variety of useful purposes. One of the most important of these is the iris. Its principal use is to regulate the quantity of light which enters the pupil, and for this purpose it possesses the power of contracting in a bright light, and of expanding when the light is feeble, so as to allow the exact number of rays to fall upon the retina, which is, in all cases, the best suited for distinct vision. It also contributes to distinct vision, especially when we view near objects, by limiting the spherical aberration of the lens; this it effects by excluding the more divergent rays which pass through the cornea, and which, from their falling on the extreme parts of the crystalline, could not be brought to a correct focus on the retina[1].

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[1] See Sir C. Bell's paper on the appropriate functions of the different nerves that are sent to the eye; Phil. Trans. for 1823, p. 289 et seq. Respecting the use of the optic nerve, and of the other nerves which belong to the organs of the external senses, we have an opinion advanced by Magendie, very different from the one which is generally adopted, but which professes to be the direct result of experiment. By dividing the different nerves in the living subject, he conceives himself to have discovered, that the senses of sight, hearing, smell, and taste, are not exercised through the medium of the optic, the auditory, the olfactory, and the gustatory nerves, as they have been named from their supposed offices, but that the impressions, in all these cases, are conveyed to the sensorium by certain branches of the fifth pair, which are distributed in greater or less quantity over the respective organs. With respect to the eye in particular, he found that the division of the fifth pair always produced blindness, although the optic nerve was untouched. The division of the optic nerve also produced blindness, so that this last appears to be essential to the sight, although not capable of producing it without the co-operation of the fifth pair. Magendie likewise informs us, that all these nerves are insensible to mechanical stimuli, while the fifth pair is exquisitively sensible to them. The experiments and deductions are contained in various papers in Magendie's Journ. t. iv. p. 170 et seq., p. 176 et seq., and p. 302 et seq. We have some observations on the comparative anatomy of the olfactory nerves by Desmoulins, t. v. p. 21 et seq., and a morbid dissection by Serres, t. v. p. 37 et seq., which are supposed to confirm Magendie's doctrine. Magendie also informs us, that he has found the retina in the human subject to be insensible to mechanical stimuli; Journ. ubi supra. This fact is in accordance with the observations which have been lately made upon the specific functions of the different parts of the nervous system. There are three cases related in Magendie's Journal which seem to favour his doctrine of the respective offices of the nerves connected with the eye. The first is by Montault, t. ix. p. 113 et seq., where the compression of the fifth pair destroyed the sight; the second is by Jadin, t. xi. p. 20 et seq.; where the compression of the fifth pair destroyed the external senses generally, the other nerves being not affected; and the third, by Piorry, t. ix. p. 53 et seq., where there was an atrophy of various parts of the brain, and among others, of the optic nerve, but where the sight was not affected. I may remark, that Desmoulins, in the fifth chapter of his work on the nervous system, coincides generally with Magendie as to the nerves of vision.

Physiologists were, for a long time, unable to explain satisfactorily, either the principle upon which the iris acts, or the mechanism by which the action is effected. It appeared, indeed, to differ so essentially from the ordinary operations of the animal œconomy, that Blumenbach regards it as affording an example of what he terms the vita propria[2], a phraseology on which I have already had occasion to animadvert. It had, indeed, been stated by Winslow[3] and by Porterfield[4], that the iris contains muscular fibres, and the circumstance of its being occasionally under the control of the will, rendered it highly probable that it was a muscular organ. It was not, however, until the investigations of the late Prof. Monro[5], and more recently of Mr. Bauer[6], that the actual existence of the muscular fibres was satisfactorily demonstrated; we learn from these observations that there are two sets of fibres, one circular and one radial, the action of which must be respectively to contract and expand the aperture of the pupil[7].

We learn from an experiment of Fontana's, that when light stimulates the iris, it does not act directly upon the part itself, act upon the retina. His experiments consisted in throwing a small pencil of rays upon the iris, which was found to produce no effect upon it; but when the same pencil was directed to a part of the retina, the contraction of the iris immediately ensued[1]. This effect may be regarded as analogous to what we observe in other parts of the body, where a stimulus being applied to a portion of the nervous system, produces contraction in a set of muscular fibres that are more or less directly connected with it. Although it appears that the iris possesses radial as well as orbicular fibres, which by their contraction, must tend to expand the pupil, yet we may conclude, that the expansion is, in a great measure, produced by the mere relaxation of the circular fibres, when the stimulus of light ceases to act upon them. We are altogether ignorant of the nature of the stimulus which causes the contraction of the radial fibres. It is scarcely probable that light is the agent in this case; perhaps it may be referred to some mechanical condition of the organ into which it is brought by the contraction of the circular fibres[2].

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[1] Young's Lect. No. 38. v. i. p. 451.

[2] Comment. Gottin. t. i. p. 43 et seq. and Instit. Physiol. § 273. p. 154. Barclay has made some very judicious remarks on this supposed power; On Life, sect. 14. See also Rudolphi's Elem. of Physiol. by How, § 225. v. i. p. 219.

[3] Anatomy, sect. 10. art. 2. § 220. v. ii. p. 287, 8.

[4] On the Eye, v. i. p. 153. and v. ii. p. 117.

[5] "Three Treatises," On the Eye, ch. 5. sect. 3. p. 110 et seq. tab. 3.

[6] Phil. Trans. for 1822. p. 78, 9. pl. 6, 7, 8. See also the observations of Mr. Jacob, in Med. Chir. Tr. v. xii. p. 509..4, pl. 9. (by mistake numbered 10) fig. 1, 2, 3, 4.

[7] Mery, who seems to have minutely examined the iris, could not detect the muscular fibres; Mém. Acad. pour 1704, p. 261 et seq., and pour 1710, p. 374 et seq. Zinn does not believe in the existence of the circular fibres, because he could not detect them, and because the functions of the part he conceives do not correspond with what might be supposed to take place from their action; De Moto Uveæ, in Comment. Gottin. Antiq. 1778. t. i. p. 55 et seq.; see also his work, Descrip. Oculi Hum. cap. 2. sect. 3. § 3, in which, although he admits the existence of fibres in the iris, he does not think they are entitled to the appellation of muscular. Blumenbach likewise opposes the doctrine of its muscularity, as we have seen above, and Dr. Knox informs us that he is unable to detect the fibres; Edin. Phil. Trans. v. x. p. 71. We have a further account of Dr. Knox's observations in the Edin. Journ. Med. Scien. v. ii. p. 103..5, the result of which still leads him to doubt of its proper muscularity. Sir C. Bell, on the other hand, Anat. v. iii. 265, 6, and Magendie, El. Physiol. t. i. p. 61, 2, admit the part to be muscular. The opinions that were entertained on this point before the time of Haller may be found in the notes to Boerhaave, Prælect. § 520. t. iv. p. 72. and in El. Phys. xvi. 2. 10..12; his opinion was against the muscularity of the parts; see his experiments in Op. Min. t. i. p. 372..4.

Among the accessory parts of the eye are certain secretory glands, of which there are two species: the lachrymal gland, by which the tears are secreted, and the sebaceous glands, named after their discoverer Meibomius. The lachrymal gland is situated under the anterior part of the upper eyelid; it is of the conglomerate class, and is provided with a number of excretory ducts, which gradually discharge the fluid over the surface of the cornea. Its office would seem to be to preserve the part in a moist state, to remove the extraneous bodies which may accidentally enter the eye, and to prevent the friction of the lids upon the ball[3]. The glands of Meibomius are situated between the duplicature of the lids and secrete the semi-fluid substance, which has been generally supposed to be of an unctuous nature, but which we are informed by Magendie possesses the characters of albumen[1]. In ordinary cases, we may therefore presume, that it is mixed with, or dissolved in the tears, and renders them better adapted for their office of lubricating the cornea. The motion of the lids is so contrived as to diffuse the tears over the surface of the eye, while the superfluous quantity is carried off by the puncta lachrymalia, and conveyed along the ducts into the nostrils.

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[1] Dei Moti dell' Iride, cap. 1. p. 7 et seq. The same essay is inserted in Journ. Phys. t. x. p. 25 et seq. and p. 85 et seq. Mr. Cooper, however, informs us, that he has met with "several cases of complete gutta serena in both eyes, in which there was the freest dilatation and contraction of the pupil;" Dict. of Surgery, Art. "Cataract," p. 296. Magendie informs us that the iris does not contract by the application of a mechanical stimulus; Elem. Physiol. t. i. p. 73.

[2] Fontana, in the work referred to above, cap. 2. p. 17 et seq., endeavours to prove, that the expanded state of the iris, and consequent contracted state of the pupil, is its natural condition; one of the principal arguments which he employs is, that the pupil is in the state of extreme contraction during sleep, when all the parts of the body are supposed to be relaxed. Mr. Walker has lately published an essay on the Iris, the object of which is to show, that the action of light on the retina is not the cause of the contraction of the iris, thus apparently contradicting the result of Fontana's experiment. Mr. Walker conceives that the nerves connected with the 3d and the 5th pairs give the iris its general sensibility to light; those of the 3d being connected with the radiating fibres, and therefore expanding the pupil; those of the 2d with the circular fibres, and contracting it.

[3] Blumenbach, Inst. Physiol. § 268. p. 152, with Dr. Elliotson's note.

It is unnecessary to offer any observations upon the palpebræ, the cilia, and the supercilia, farther than to remark, that they are all well adapted for their office of protecting the eye in the different circumstances in which it is placed, and for enabling it to exercise its functions in the most perfect manner[2].

One of the most elaborate parts of the mechanism of the eye is the system of muscles which are attached to the globe. Of these there are six to each eye; four, named from their form and position, straight, and two oblique. The minute description of these muscles falls under the province of the anatomist. The effect which will be produced by their contraction may be learned by an inspection of the mode in which they connect the globe of the eye with the contiguous parts; it is sufficient to say, that by their means we are enabled to move the eyes in all possible directions, with the greatest facility and correctness[3].

Besides the optic nerve, which I have noticed above as belonging to the second order of parts, the eye and its appendages are plentifully provided with nerves derived from other sources[4].

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[1] El. Physiol. t. i. p. 46.

[2] These parts are fully described by Porterfield, in his Treatise on the Eye, v. i. book 1; by Haller, under the denomination of "Oculi Tutamina," in El. Phys. lib. xvi. sect. 1; and by Magendie, under that of "parties protectrices de l'œil," El. Physiol. t. i. p. 40..7; they are figured by Sœmmering with great minuteness in his Icones Oculi Hum. tab. 2.

[3] The first correct descriptions of the muscles of the eye were given by Fallopius, in his "Observationes Anatomicæ," Op. t. i. p. 379, 0; by Fabricius, "De Oculo," cap. 11. Op. p. 194, 5. tab. 1. fig. 6, 7, 8; and afterwards by Winslow, Mém. Acad. pour 1721, p. 310 et seq. and Anatomy, sect. 10, art. 2. § 5. A very full account of these muscles, as well as of every other part connected with the eye, may be found in the elaborate treatises of Porterfield, Ed. Med. Ess. v. iii. p. 163..177, and "On the Eye," v. i. p. 79..95. See also Haller, El. Phys. xvi. 2. 24, 5; and Hunter on the Animal Œcon. p. 253..7. We are indebted to Sœmmering for an accurate delineation of these muscles, Icones Oculi Hum. tab. 3, 4; also in Cloquet, pl. 64, and to Sir C. Bell for a minute investigation of the effects which are produced by their contraction; Phil. Trans. for 1823, p. 172 et seq. pl. 21.

[4] It is remarked, that in all animals which are provided with a nervous system, a great proportion of the nervous matter is appropriated to the eye: in fishes, according to Haller, as much as nine-tenths of the nerves are distributed to this organ; El. Phys. xvi. 2. 26. An account of the various sources from which the different parts of the eye and its appendages receive their nerves is contained in § 27..30. See also Sir C. Bell's plates of the nerves, No. 1. In Phil. Trans. for 1823, p. 289 et seq., he has applied his ingenious hypothesis of the double office of the nerves to those of the eye, and has pointed out the functions which the different nerves that are sent to this organ respectively perform. We have a good view of the nerves of

These serve, some of them, to give the muscles the power of voluntary motion, and others to assist in the various functions which depend, either directly or indirectly, upon the co-operation of the nervous system. The researches of Sir C. Bell have enabled us to appropriate, with considerable correctness, the respective offices of the different nerves that are sent to the parts about the eye; while we, at the same time, derive from his remarks a strong confirmation of the opinions which he entertains upon the subject of the nervous system generally, to which I have already had occasion so frequentlyto refer[1].

Before we dismiss this part of the subject, it will be proper to make some observations on the use of the crystalline lens. The structure of this body, its situation, and its connexion with the contiguous parts, many of which are, like itself, of a very elaborate organization, would seem to point it out as serving some important purpose in the œconomy of the eye. It was accordingly supposed by many of the older anatomists to be the immediate seat of vision, until Kepler demonstrated its refractive power, and showed by the experiment related above, that the retina is the part on which the visible impression of objects is received. But it is not probable that the sole use of the crystalline depends upon its refractive power; for, had its place in the eye been occupied by the vitreous humour, and, more especially, had this humour received only a slight increase of density, the rays of light would have been brought to a focus on the retina, without the aid of the crystalline. And we find that even in those eyes, where, in consequence of disease, the crystalline has been artificially extracted, if the operation be successfully performed, and no displacement of the parts be produced, the vision is but little impaired, or at least is rendered nearly perfect by the use of a convex lens. Still it is inconsistent with the views which we entertain of the nature of the animal economy to suppose that such an organ should not serve some specific purpose, and accordingly physiologists have assigned three different uses for which this part would seem to be adapted, while, at the same time, they have conceived these points to be necessary for the perfect exercise of the function of vision[2].

The three objects to which I refer are to correct the spherical aberration, to prevent the unequal refraction of the differently coloured rays, and to assist in the adaptation of the eye to distinct vision at different distances. With respect to the first of these objects, when a number of parallel rays of light pass

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the eye, as well as of the nerves that are sent to the other organs of sense, in the splendid "Icones Anatomicæ" of Langenbeck, fascic. 2. tab. 2. See also Cloquet, p. 177. fig. 3..6, taken from Scarpa.

[1] See the remarks of Mr. Mayo; Comment. pt. 2. p. 5, 6; on the appropriate uses of the different nerves connected with the eye.

[2] M. Magendie, in opposition to all other modern physiologists, appears disposed to limit the use of the crystalline to increasing the brightness and clearness of the image by diminishing its size; El. Physiol. t. i. p. 67, 72.

through a spherical lens of equal density in all its parts, according to the laws of optics, the focus will be imperfect, but, if the lens be of the nature of that of the crystalline, composed of layers gradually increasing in density as we approach its centre, we shall have the rays brought to a proper focus.

The same remarks, both as to the defects arising from a sphere of uniform density, and the mode of rectifying the defect, apply to what has been termed the Newtonian aberration: the progressively increasing density of the different layers of the crystalline will, as it is supposed, render the eye an achromatic instrument, and thus prevent the confusion of colours which would be produced without this contrivance. Although these remarks are founded upon correct optical principles, it has been questioned, both on theoretical grounds, and by a reference to experiment, how far they will apply to the eye, or at least how far we are able to detect their operation. It has been calculated by Maskelyne, that no perceptible aberration, depending upon the different refrangibility of the prismatic colours, will take place in the eye[1], and it is stated as a matter of fact, that in those eyes, where the crystalline has been removed, we do not perceive the defects which are supposed to be remedied by its presence.

The third use which has been assigned to the crystalline is to assist in adapting the eye to the distinct vision at different distances. In the natural and perfect state of the organ, those rays only can come to an exact focus on the retina which enter the cornea in a parallel direction, as proceeding from distant objects. When, therefore, we wish to view near objects, we use a voluntary exertion, by which the shape or conformation of the eye is altered[2]. If we accurately attend to our sensations, we shall find that a specific effort is necessary for this purpose, and that a certain length of time is required for its accomplishment. The nature of this power, or the mode in which the change is effected has been the subject of much ingenious discussion, and has given rise to many curious experiments. Three methods have been suggested for this purpose; first, by bringing forward the crystalline nearer to the cornea, without altering the form either of the whole eye or of the crystalline itself; secondly, by changing the figure of the globe of the eye, so as to increase the distance between the cornea and the retina; or, thirdly, without altering the general form of the eye, by increasing the sphericity of the crystalline, and thus giving it an increase of refractive power [1].

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[1] Euler advanced the opinion, that the different humours of the eye are so adjusted to each other, as to render it achromatic; Mém. Berlin, pour 1747, p. 279. Maskelyne, however, argues that the reasoning of Euler is not correct, as applied to the actual constitution of this organ. He further calculates the amount of the aberration which would necessarily take place in the eye, and concludes that "the real indistinctness...... will be fourteen or fifteen times less in the eye than in a common refracting telescope, which may be easily allowed to be imperceptible." Phil. Trans. for 1789, p. 256 et seq. Porterfield, on the contrary, supposes that the aberration arising from the different refrangibility of the rays of light is very much more considerable than that depending upon the mere form of the lens; On the eye, v. i. p. 378, 9. And we have an experiment of Dr. Wollaston's, by which the eye is sensibly proved to be not achromatic; Young's Lect. v. ii. p. 584. We have some judicious observations on this point by Dr. Hall, in Quart. Journ. v. v. p. 253. See Haller, El. Phys. xvi. 4. 11; Blumenbach, Inst. Physiol. § 270. p. 153; Young's Lect. v. i. p. 448; also the remarks of Sir D. Brewster, in Phil. Mag. v. 6. 3d ser. p. 161 et seq.

[2] I must remark that both M. Magendie and Sir C. Bell do not admit that the eye possesses this power. Magendie founds his opinion upon an observation which he made on the eye of an albino animal; this is so transparent as to enable us to see the picture at the back of the globe, and he informs us that it is equally distinct for near, as for distant objects; Elem. Physiol. t. i. p. 70.,3. Sir C. Bell, after reviewing all the hypotheses that have been

The first of these hypotheses appears to have been generally adopted by the earlier physiologists, and is the one which was maintained by most of the contemporaries of Haller. Porterfield endeavoured to prove that the accommodation of the eye is effected by means of a contractile body which is attached to the crystalline, and which has the power, when necessary, of bringing it nearer to the cornea [2]; but this opinion is controverted by Haller, who shows that the body is not contractile, and that, even if it were so, it could not produce the effect which is assigned to it. And this seems to be the case with all the speculations of a similar kind, that neither the nature of the individual parts nor the general structure of the eye admit of that action, which would be adequate to bring about the necessary alteration in the relative position of the different parts of the organ [3].

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brought forwards to account for the effect, thinks that they are none of them adequate to the purpose, and concludes that much of what has been "attributed to mechanical power is the consequence of attention merely." Anat. part. 2. b. 1. ch. 11. v. iii. p. 334 et seq. The same opinion is maintained by Desmoulins, Anat. Syst. Nerv. p. 650; he derives his opinion from the state of the eye of the cetacea, which does not admit of the change of figure or position, although they see equally well in air and in water.

[1] The subject is discussed by Haller, with his usual learning and candour, and the hypotheses of the various writers who had preceded him are briefly detailed in El. Phys. xvi. 4. 20..7. He is himself inclined to adopt the opinion, that the power of seeing distinctly at different distances depends upon an alteration in the size of the pupil, § 27; an idea which was originally brought forward by Delahire, Mém. Acad. t. ix. p. 620 et seq.; but this opinion is generally supposed to have been disproved by Porterfield; Ed. Med. Ess. v. iv. p. 124 et seq. also book 3. ch. 3. v. i. p. 389 et seq. of his elaborate treatise on the eye. Le Roy, however, again advocated Delahire's hypothesis in opposition to the observations of Porterfield; Mém. Acad. pour 1755, p. 594 et seq. This opinion is likewise adopted by Caldani, Instit. Physiol. p. 211..3, principally, as it appears, on the authority of Haller, and, to a certain extent, by Dr. Knox, whose investigations on the structure and action of the eye appear to have been conducted with peculiar accuracy. See also the remarks of Mr. Mayo; Physiol. p. 293..5.

[2] Ed. Med. Ess. v. iv. p. 197 et seq. and "On the Eye," v. i. p. 446 et seq.

[3] An hypothesis nearly resembling that of Porterfield has been lately brought forward by Dr. Knox. In the prosecution of his delicate researches

The second hypothesis, that which supposes the adjustment of the eye to be effected by some cause producing a change in the form of the globe, has met with many able advocates, and, among others, Prof. Blumenbach[1]. The four straight muscles of the eye, the tendons of which are applied over a part of its surface, are supposed to be the agents in effecting this change of figure. When these muscles contract, it is supposed that they must compress the ball in such a manner, as to cause a certain degree of protrusion of the cornea, and a consequent increase of the distance between this part and the retina[2].

A series of well contrived experiments on this subject were performed by Sir E. Home, in conjunction with the late Mr. Ramsden, in which they attempted to prove the actual existence of this increased convexity of the cornea, as well as to show, that an eye from which the crystalline had been extracted was capable of adjusting itself to near objects[3]. Could this have been proved, it would have afforded us an unequivocal demonstration of the truth of the hypothesis; but simple as the experiment may appear, and however easy it might have been supposed to obtain satisfactory evidence on the subject, the question respecting the power of the eye after the removal of the crystalline, appears to be scarcely yet decided[4].

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into the anatomy of the eye, he conceives that he has discovered the annulus albus, the part which unites the choroid and sclerotic coats, to be muscular, and accordingly terms it the ciliary muscle: from its structure, and especially from its comparative anatomy, he regards it as a principal agent in the adjustment of the eye; Edin. Trans. v. x. p. 52..6, and p. 250..2. Part of the effect he ascribes to the contraction of the pupil, p. 57; see also Edin. Journ. Med. Scien. v. ii. p. 110, 1. There are some judicious remarks by Winteringham, on these supposed motions of the internal parts of the eye, in his Exper. Inq. p. 286..0. Mr. Crampton has announced the discovery of a muscular structure in the eye of the ostrich, the operation of which, it is conceived, must be to alter the convexity of the cornea, and thus assist in the adjustment of the eye; Thomson's Ann. v. i. p. 170 et seq.

[1] Inst. Physiol. § 276, p. 155.

[2] This hypothesis was supported by Dr. Hossack; Phil. Trans. for 1794, p. 196 et seq. Monro (sec.) conceives that both the straight and the oblique muscles, and likewise the orbicularis palpebrarum, by their action contribute to lengthen the axis of the eye; Three Treatises, p. 137. In pl. 4. we have a good view of these muscles. Blumenbach supposes that the change depends upon the action of the straight muscles alone. See Dr. Knox's observations on the insufficiency of this hypothesis, in Edin. Trans. v. x. p. 50. According to Mr. Owen, the power of accommodation in the eye of the bird, where it exists in an unusually great degree, depends upon a change in the figure of the globe; Cyc. Anat. v. i. p. 304.

[3] Phil. Trans. for 1794, p. 21 et seq., for 1795, p. 1 et seq., and for 1796, p. 1 et seq., in which he supports his position by various facts in comparative anatomy; and for 1797, p. 1 et seq., where he farther illustrates it by the morbid actions of the muscles. A good view of this discussion is contained in Nicholson's Journ. v. i. 4to. p. 303 et seq.

[4] The affirmative is maintained by Haller, El. Phys. xvi. 4. 25. and is supported by many respectable authorities to which he refers; but, without impeaching his general accuracy, it may be presumed that, upon this point, he was not sufficiently informed. Porterfield had previously given a distinct

The third hypothesis, that which attributes the power of
djustment to a change of figure in the crystalline itself, may be
onsidered as having originated with Leeuwenhoek, who con-
eived that he had detected muscular fibres in the lens, which,
y their contraction, would render it more convex, and con-
equently increase its refractive power[1]. Descartes adopted
is opinion, but it does not appear that he added any new
cts in support of it[2]. It has been lately embraced by Dr.
oung, and defended by him with his accustomed ingenuity
nd acuteness[3]. He rests his opinion partly upon the struc-
ure of the crystalline, in which he conceives that he has de-
ected the same fibrous appearance which was described by
eeuwenhoek, but more perhaps from his experiments, in which
e shows, that the faculty of adjustment is not prevented by
aving the eye immersed in water, in which situation its re-
ractive power could not be affected by any alteration in the
onvexity of the cornea. He also maintains that an eye from

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nd apparently correct account of a case, in which the removal of the crys-
alline deprived the eye of the power of accommodation; Edin. Med. Ess.
. iv. p. 182..6. Dr. Knox, however, supposes that the removal of the
ns does not destroy the power of accommodation; Edin. Trans. v. x. p. 56;
hile we have the high authority of Mr. Travers for the opposite opinion;
n the Eye, p. 62. In the 2d number of the new series of the Bibliotheque
e Geneve we have an account of a case of cataract, which was operated
pon by M. Maunoir, where, by the employment of a properly adjusted con-
ex lens, the patient appears to have obtained the complete use of the eye for
ll the ordinary purposes of life. The case affords ample proof of the skill
nd dexterity of the operator, but it will scarcely warrant the conclusion of
. Maunoir, that the eye possessed the complete power of adjustment. It is
o be regretted that this point was not ascertained by the application of Dr.
oung's optometer.

[1] Phil. Trans. v. xiv. No. 165. p. 170 et seq. with the accompanying
late; see also his account of the crystalline of a whale; Phil. Trans.
. xxiv. p. 1723 et seq. tab. 1. fig. 5, 6. Speaking of the crystalline on
nother occasion, he terms it "crystallinum musculum." Opera, v. i. p.
02.

[2] In his Dioptr. cap. 3. "De Oculo," § 5, we have the remark, "Humanum
rystallinum esse masculi instar .... qui totius oculi figuram mutare potest."
p. t. ii. pars. 2. p. 66. In his Tract. de Homine, it is stated, that by means
f the ciliary ligament, the crystalline can be rendered more or less convex,
p. t. iii. p. 75, but in this passage the muscularity is rather implied than
xpressed. Pemberton, in his inaugural dissertation, published at Leyden in
719, argued in favour of the muscular structure of the crystalline, and en-
eavoured to point out, by a series of mathematical propositions, the mode in
hich the fibres act in producing the requisite change; "De Facultate Oculi,
ua ad diversas Rerum conspectarum Distantias se accommodat." In Haller,
isp. Anat. t. vii. par. 2. p. 139 et seq.

[3] Phil. Trans. for 1793, p. 169 et seq. pl. 20. fig. 2, 3. and Lect. v. ii. p.
23 et seq. Phil. Trans. for 1801, p. 53..83. and Lect. v. ii. p. 573 et seq.;
lso Lect. v. i. p. 450, 1. and Med. Lit. p. 98, 9. Monro (sec.) admits of
he fibrous structure of the crystalline, but he conceives that we have no
vidence of its muscularity; On Fishes, ch. 11. p. 79; and Three Treatises,
n the Eye, ch. 2. sect. 2. p. 85..7. We have a delineation of the
ctual appearance of the lens in Sœmmering, Icon. Oculi Hum. tab. 5. fig.
6..9.

which the crystalline has been extracted is incapable of adjusting itself to near objects, but upon this question I am inclined to think that the experiments, considered as leading to a negative result, have not been sufficiently numerous to admit of so important an inference [1]. From a general review of all the facts that we possess on the subject, I feel much disposed to coincide in the opinion of Dr. Young, but at the same time I think it would be desirable to repeat the experiments upon a greater number of eyes that have been deprived of the crystalline, before we can regard the question as decided [2].

The peculiar formation of the eye which produces short-sightedness, where its refractive power is so considerable, as to cause those rays to form a distinct picture on the retina, which enter the cornea, in a diverging state, has been supposed to be analogous to the condition to which the organ is brought when we employ a voluntary effort to view near objects. And as this state of the eye is thought to depend upon the cornea being unusually convex [3], it has been conceived that the same change of figure must be produced by the adjustment to near objects.

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[1] See the experiments of Sir E. Home referred to above, and note 6. We learn from Wells, Phil. Trans. for 1811, p. 381..5, that after the middle period of life, the eye, in its ordinary state, loses the power of adjustment; hence it follows, that in experiments of this kind, the eyes of young persons only should be employed. He found that the effect of belladonna, when applied to the eye, was not only to expand the pupil, but likewise to destroy the power of adjustment, p. 382..4, and 387, 8. It may be presumed, however, that the loss of the power of adjustment, although contemporary with the expansion of the pupil, is not the effect of this expansion, but that it rather depends upon a paralysis produced in some part of the organ. He argues against the hypothesis of the muscular structure of the crystalline being the medium of the adjustment, because he could never produce any appearance of contraction in this part by the application of stimuli, and because he conceives that its physical properties are not suited to the purpose of contraction, p. 390, 1. See also the remarks of Sœmmering in the description of his plates of the Eye, p. 67, 8.

[2] A new mode of accounting for the change of the eye has been recently advanced by Mr. Travers, which may be regarded as a combination of the first and third hypotheses; he considers "adjustment as a change of figure in the lens," not, however, from a contractile power in the part itself, but in consequence of the lamellæ of which it is composed sliding over each other, when acted upon by external pressure, while upon the removal of this pressure, its elastic nature restores it to its former sphericity. The iris is supposed to be the agent in this process: the pupillary part of this organ Mr. Travers conceives to be a proper sphincter muscle, which, when it contracts and relaxes, will tend, by the intervention of the ciliary processes, to effect a change in the figure of the lens, which will produce a corresponding change in its refractive power, ".... by the steadily contracted state of the pupil suited to the nearest extremity of the focal range, they" (the radiated fibrous processes connected with the iris) "will be closed and braced together; and bearing upon the circumference of the crystalline at every point, will necessarily elongate the axis of the lens." On the Eye, p. 62..7.

[3] Porterfield, Ed. Med. Ess. v. iv. p. 128, 9, 229, and Treatise on the Eye, v. ii. p. 36; Smith's Optics, § 89; Haller, El. Phys. xvi. 4. 15..17; Nicholson's Nat. Phil. v. ii. p. 348; Blumenbach, Instit. Physiol. § 275. p. 155; Bell's Anat. v. iii. p. 233.

But however strong the analogy may appear, its force will be entirely destroyed if we admit the correctness of Dr. Young's experiments mentioned above.

With respect to the state of the eye which produces short-sightedness, we have sufficient evidence that it is hereditary. It is, however, rather the tendency to it than the actual malconformation which is so, for we find that very young children are seldom, if ever short-sighted, but that the affection generally commences at the period when they first begin to apply themselves to books. It is much more frequent among the higher than the lower classes of society, a circumstance which depends partly upon the former being more devoted to literary pursuits, and partly upon the too early and frequent use of glasses, by which any natural tendency which the eye might have to assume this form is confirmed, while the efforts are prevented which it would otherwise make to acquire a distinct view of remote objects. There are also certain occupations, which require the eye to be constantly adapted to the view of minute bodies, where this state of the vision almost universally prevails, while a different mode of life is observed to produce a contrary tendency. Daily observation on the eyes of the short-sighted, proves that this defect is generally connected with an obvious projection of the cornea, but this appears not to be universally the case. There are instances in which it seems that the natural state of the eye, or that which it assumes when no voluntary effort is employed, is the one adapted for viewing near objects, and that, upon whatever cause this state depends, the eye permanently retains it, so that it is, in a great measure, deprived of the power of adjustment. This power is likewise, for the most part, lost in eyes of all descriptions as age advances, but here the eye remains permanently adapted to distant vision[1].

SECT. 2. *Of the Nature and Cause of Vision.*

With respect to what may be termed the cause of vision, I have little to observe in addition to what has been said on the subject of nervous action generally. We know that when the impression has been received on the retina, it is transmitted by means of the optic nerve to the sensorium commune, an effect which the older physiologists ascribed to the agency of the animal spirits, which has been more lately referred to a vibration propagated along the part, and still more recently to the operation of the electric fluid. So far as the particular case of the eye is concerned, it may perhaps seem more favourable to the doctrine of vibrations, and indeed it was from some observations made upon the sense of vision that the hypothesis was originally formed[1]. As we have its specific cause so entirely under our control, we are enabled to make our experiments and observations upon it with more precision than on the other external senses, and one important point which we have been enabled to ascertain is, that when an impression is made upon the extremity of a nerve, the effect remains for some time after the cause is removed. There are many facts which prove this to be the case with respect to the action of light upon the eye. If a burning body be rapidly whirled round, it will produce the appearance of a complete circle of fire[2]. Upon the same principle, if the seven prismatic colours be painted upon a card, which is made to spin upon its centre, no individual colour will be seen, but the eye will receive the general sensation of whiteness, from the combined impression of the whole. These effects depend upon the principle, that the eye retains the impression of the object in each particular part of the circle, until it arrives again at the same point, so that the different or successive impressions are all blended together.

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[1] These positions are confirmed by an interesting paper of Ware's, in Phil. Trans. for 1813, p. 31 et seq., to which we have, in the same volume, a valuable appendix by Blagden, p. 110 et seq. See also the paper of Wells referred to above. In connexion with this part of the subject, I may refer to two papers in the 1st and 2d numbers of the Journal of the Royal Institution, entitled, Contributions to the Physiology of Vision. They are essays of much learning and research, and are peculiarly valuable for the numerous references to the Continental writers.

There is another very curious series of phenomena, which are somewhat analogous to the above, as far at least as they depend upon the permanency of the effect after the exciting cause is removed. They were first minutely described by Buffon, who named them accidental colours[3], they were afterwards successively examined by Scherffer, Æpinus, and Darwin, and are now known by the name of ocular spectra[4]. If the eye be steadily directed, for some time, to a white spot upon a dark ground, and be then turned aside, we shall perceive a well defined image of the spot, but the effect will be reversed; the spot will now appear dark and the ground white, and the opposite effect will be produced if we view a dark spot upon a white ground. The same kind of alternation takes place between different colours as between different degrees of light; if, for example, we look at a blue object, the eye acquires a yellow spectrum, while a yellow object produces a blue spectrum. In the same manner red and green alternate with each other, and in short every colour has its appropriate spectral colour, the sensation of which is always produced in the eye, when the primary colour has made a sufficiently strong impression upon it. It may be presumed that a considerable share of what is termed by painters the harmony of colouring and the richness of effect, as exhibited either in pictures, or in the arrangements of drapery and furniture, depend upon this affection of the eye, the brilliancy of the colours being much increased by the position in which they stand with respect to each other.

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[1] Newton's Optics, Quær. 12..4. Op. t. 4. p. 220, 1.

[2] Newton's Optics, Quær. 16. Op. t. iv. p. 222; Porterfield on the Eye, v. ii. p. 223; Hartley on Man, v. i. p. 9, 0; Musschenbroek, Elem. Phys. ch. 33. § 998. p. 418. The general principle is clearly stated by Cullen; Physiol. § 48. p. 45.

[3] Mém. Acad. pour 1743, p. 147 et seq. They had been previously described by Jurin, but only in an imperfect manner; see his Essay at the end of Smith's Optics, § 260..6. p. 169.

[4] Journ. Phys. t. xxvi. p. 175, 273 et seq.; Do. p. 291 et seq.; Phil. Trans. for 1786, p. 313 et seq. See also an essay by Plateau, in Ann. Chim. t. lviii. p. 337 et seq. on the theory of accidental colours.

Besides the effect arising from the permanency of the impression after the removal of the exciting cause, there is another principle, to which this peculiar affection of the retina may be partly referred, that a nerve is unable to persevere in the same kind of action beyond a certain period, in consequence of the occurrence of what has been termed exhaustion[1]. The term was originally derived from the hypothesis of the animal spirits, proceeding upon the idea of there being a limited supply of these spirits in the nerve, which, by a too long continuance of the action, was suspended. The hypothesis itself being without foundation, the explanation that is derived from it must necessarily be so likewise, but in whatever manner we may explain it, the fact is one of constant occurrence, and it frequently assists us in determining whether an action is to be originally referred to the operation of the muscles or the nerves.

By combining these two principles or properties of the nervous system, we seem to obtain an easy method of explaining the various appearances which are presented by the ocular spectra. In the first case, where we have simply the effect of a greater or less degree of illumination, we may naturally ascribe the effect to the exhaustion of those parts of the retina which had been more strongly excited by the greater force of the impression made upon them. And in the same way we may explain the variations of colour that occur in the second case; for we shall find that the spectral colour is, in every instance, that which would result from a union of all the prismatic colours, except the one to which the eye had been previously exposed, and to the action of which it had consequently become more or less insensible.

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[1] Darwin classes the spectra under the two heads of direct and reverse, the first depending upon the permanence of the impression, the second upon exhaustion; Phil. Trans. for 1786, p. 313 et seq.; and there appears a real foundation for this distinction. See the art. "Accidental Colours," by Brewster, in his Encyclopædia, where the subject is fully discussed. Perhaps the phenomena that are described by Dr. Brewster, designated "affections of the retina, as exhibited in its insensibility to indirect impressions, and to the impressions of attenuated light," Journ. of Science, v. iii. p. 280 et seq., may be, partly at least, explained by a reference to the effects of exhaustion and re-action. It is in some measure, to this principle that we are to refer those curious cases, which occasionally occur, where a visible appearance of an object, which had become no longer perceptible, may be reproduced by various circumstances, some physical and others mental; see Brewster's Journ. v. iv. p. 75 et seq.

It is probable that the formation of these spectra in the eye have frequently given rise to a belief in supernatural appearances. In certain diseased states of the nervous system, the retina is more than usually disposed to retain these impressions, so that, for a long time after the exciting cause has been removed, the spectrum will still remain visible[1]. The same causes which tend to weaken the nervous system, frequently produce a similarly debilitating influence over the mental powers, so as to render them peculiarly susceptible of being affected by superstition and credulity. The surprise which such appearances must occasion to those totally ignorant of their nature, the terror which is often associated with darkness, concurring with the weakened state of the mind and body, must be conceived, in many cases, adequate to produce the effect, without having recourse to the idea of any intentional deception on the part of the individual concerned, or of the miraculous interference of supernatural agency[2].

Another circumstance which regards the operation of the nervous system, and which has been thought to favour the hypothesis of vibrations, is, that the power of a nerve in transmitting impressions is destroyed by pressure, while, by the removal of the pressure, the part regains its power, provided its structure be not injured. Now, as we have no proof of the existence of any substance being connected with the nerve or attached to it, which can be regarded as the efficient cause of sensation, it would seem that the effect must be referred to the relation of the different parts of the nerve to each other, and this, it is conceived, may be ultimately resolved into a certain kind of motion among the particles, which motion is successively propagated from one to the other, and is counteracted by pressure[3].

It has been further urged in support of the opinion, that nervous action essentially consists in vibrations, that besides light, which is the specific and appropriate cause of vision, the sensation of sight may, under certain circumstances, be produced by other causes, which may all of them be ultimately referred to motion. A smart blow on the eye, friction and pressure upon the ball[1], and electricity, all produce this effect. It is difficult to conceive how a ray of light, mechanical violence, and electricity, can all have the same action upon the eye, and it may be inferred, that the only common principle on which they can operate, is the production of a certain kind of motion in the retina and the optic nerve. Of the nature of this motion, however, either as inferred from experiment or from hypothesis, it is impossible for us to form any conception; the attempt of Hartley to reduce it to a regular system of vibrations does not tend to throw any real light upon its nature, while I conceive that it is clearly disproved by the discovery of Dr. Philip, that the action can be propagated across the interval of a divided nerve[2].

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[1] Dr. Alderson has made use of this principle in his ingenious "Essay on Apparitions," and it has been since employed in the same way by Ferriar, and by Dr. Hibbert, in their works on the same subject. We are by this means not unfrequently enabled to explain certain supposed supernatural appearances, the evidence of which is too direct for us to doubt of their actual occurrence, without setting aside all human testimony. The first of the papers in the Royal Institution Journal referred to above, contains an analysis of Purkinje's "Essay on the substantive phenomena of vision," i. e. of perceptions which are produced in the eye, but which do not originate in external objects. It is an essay of much interest, although I am disposed to dissent from some of the positions of the writer.

[2] The remarks of Dr. Brewster, referred to above, tend to illustrate this subject; Journ. v. iii. p. 290, 1.

[3] Dr. Alison conceives that something like motion exists in the nerves during their action, Physiol. p. 159.

There is a singular state of vision, which must be noticed in this place, where the eye exercises its function in a perfect manner, as far as respects the form and position of objects, and even the quantity of light that falls upon their different parts, but produces only an imperfect conception of colour. It would appear, that in this condition of the organ, there is not properly a confusion of colours, but that there is either a total incapacity of perceiving colour generally, or an insensibility to perceive certain colours, while there is a sufficiently distinct perception of others.

Numerous cases of this kind are upon record[3], and we have a minute description given us by Dr. Dalton of this peculiar defect, as existing in his own eyes. He informs us, that when he looks at the prismatic spectrum, he can only distinguish three colours, which would appear to be blue, yellow, and purple, while he is incapable of perceiving either the green or the red rays[4]. The cause of this defect is not known; we are not acquainted with any physical state of the organ which could have this effect upon the rays of light, nor does it appear, that we have any facts derived from the other senses, which can guide us in our explanation. It has been attributed to a deficiency in the perceptive powers of the eye, similar to what occurs in the ear of those who are incapable of distinguishing musical sounds. But I conceive it would be difficult to show the analogy betwen the two cases, nor if it were established, would it throw any light upon the nature of the efficient cause.

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[1] Newton's Optics, Quær. 16. Op. t. iv. p. 222. A curious, and, as it would appear, an accurate account of the effect of strong pressure upon the eye-ball is given us by Elliott, in his "Observations on the Senses of Vision and Hearing," p. 2, 3.

[2] See p. 523.

[3] One of the earliest is in Phil. Trans. for 1777, p. 260 et seq. by Huddart; the person of whom he gives an account, seems to have had a very clear conception of figure, and of light and shade, but probably no idea of colour of any description.

[4] Manch. Mem. v. v. p. 28 et seq. Dr. Dalton ascribes the defect in his vision to one of the humours of his eye being "a coloured medium, so constituted as to absorb red and green rays principally;" p. 42; but I believe that this explanation is not considered as satisfactory. He gives an account of another case, p. 37..41. We have two cases by Dr. Nicholls, Med. Chir. Tr. v. vii. p. 477 et seq. and v. ix. p. 359 et seq.; and one by Dr. Butter, in Edin. Phil. Journ. v. vi. p. 135 et seq.; he conceives it to be a physiological and not an optical defect, while Dr. Brewster, in his remarks on the case, supposes that it depends upon a want of sensibility in the retina, analogous to the insensibility of the ear to certain sounds. We have also a case by Mr. Harvey, in Edin. Phil. Trans. v. x. p. 253 et seq.; see also two cases in Brewster's Journ. v. x. p. 153 et seq.

SECT. 3. *Acquired Perceptions of Sight.*

My next object must be to give some account of the acquired perceptions of sight, and the associations which are formed between this sense and the other classes of perceptions of impressions. The most important and curious subject for inquiry which here presents itself, respects the means by which we judge of the distance, magnitude, and position of bodies, or how far we are able to connect the visible impressions which we receive by the eye with the actual condition of the objects [1]. With regard to the method by which we judge of distance, it was formerly supposed to depend upon an original law of the constitution [2], and to be independent of any knowledge gained through the medium of the external senses. This opinion was attacked by the celebrated Berkeley, in a treatise remarkable for its acuteness and strength of reasoning, in which he clearly demonstrated, that our knowledge on this subject is acquired by experience and association [3]. This conclusion is fully warranted by many circumstances of frequent occurrence, where we fall into the greatest mistakes with respect to the distance of objects, when we form our judgment solely from the visible impression made upon the retina, without attending to the other circumstances which ordinarily direct us in forming our conclusions [4].

Although Berkeley, in the establishment of his theory, adduced a variety of facts in its favour, still he was not able to bring forwards any decisive experiment, from which he could directly deduce its truth. Fortunately, however, the means of making an experiment of this kind occurred to Cheselden, the result of which very remarkably coincided, at least in the most important particulars, with the doctrine of Berkeley. I refer to the well known case, in which this eminent surgeon operated on the eyes of an individual who was born blind, and whose sight was not restored until he had attained a sufficient age to give a correct account of his feelings, and of the impressions which he received after he had acquired his new sense [1]. It clearly appears that, in the first instance, he had no correct ideas of distance, and we are expressly told that he supposed all objects to touch the eye, until he had learned to correct his visible, by means of his tangible impressions, and thus gradually to acquire more correct notions of the situation of surrounding bodies with respect to his own person [2].

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[1] We meet with many valuable and judicious observations on this subject in Reid's Treatise on the Mind, ch. 6. sect. 6; particularly as illustrating the position, that our perceptions bear no necessary resemblance to the impressions made on the organs of sense.

[2] When physiologists speak of certain functions or powers as produced by instinct, it may be presumed that they do not essentially differ from those who consider them as depending upon what have been termed laws of the constitution; see Young's Lect. v. i. p. 449; Monro's Three Treatises, c. 6. sect. 3.

[3] "Essay towards a new Theory of Vision." He thus announces the object of the essay in the first paragraph: "My design is to shew the manner, wherein we perceive by the sight the distance, magnitude, and situation of objects. Also to consider the difference there is betwixt the *ideas* of sight and touch, and whether there be any *idea* common to both senses." p. 1.

[4] Smith's Optics, § 160. and Remarks, § 311..320.

Proceeding then upon the principle, that our ideas of distance are all of the class which I have named acquired perceptions, it remains for us to investigate the circumstances which assist us in forming our judgment respecting them. We shall find that they may be arranged under two heads, some of them depending upon certain states of the eye itself, and others upon various accidents that occur in the appearance of the objects. With respect to distances that are so short as to require the adjustment of the eye in order to obtain distinct vision, it appears that a certain voluntary effort is necessary to produce the desired effect; this effort, whatever may be its nature, causes a corresponding sensation, the amount of which we learn by experience to appreciate, and thus, through the medium of association, we acquire the power of estimating the distance with sufficient accuracy[1].

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[1] Phil. Trans. for 1728, No. 402, p. 447 et seq.; also Anat. p. 300 et seq.; and remarks by Smith, Optics, § 132..5.

[2] In the present improved state of surgery, instances are not rare in which persons who are born with cataracts have them afterwards removed, so as to acquire the power of vision, yet it will be found upon inquiry, that cases equally adapted for the experiment with that of Cheselden are seldom to be met with. In a great majority of them, although the state of the eye renders it completely useless with respect to all the purposes of life, still it is sensible to the impression of light, and admits of an indistinct perception of objects, from which an imperfect idea of distance is obtained. It generally happens that the cataracts are removed at an earlier age than in Cheselden's case, or that the individual, from the nature of his education, or the state of his mental powers, is not able to give a correct account of his feelings and perceptions. The case that is related by Ware, Phil. Trans. for 1801, p. 382 et seq., also in Nicholson's Journal, v. i. p. 57 et seq., must either have been one where the cataract had been incomplete, or where the patient, who was only seven years of age, was not fully able to comprehend the nature of the questions which were proposed to him. For if we receive the account literally, as it is given us by the writer, we must conclude, not only that the patient had correct ideas of visible distance, but of the relative position, and even of the shape and colour of objects; ideas which must either be intuitive or have been acquired by experience. The author has, however, unfortunately overlooked these circumstances, and endeavoured to invalidate the force of Cheselden's reasoning. See the remarks of Prof. Stewart on this case in Edin. Trans. v. vii. p. 2..4. In a late case of cataract, which was operated upon by Mr. Wardrop, the observations may be regarded as confirming those of Cheselden, and are so considered by the author; Phil. Trans. for 1826, p. 529 et seq. In the two cases upon which Sir E. Home operated, the patients had certain indistinct ideas of visible form and colour previous to the operation, yet in the one where the vision was the least distinct, in consequence of the greater opacity of the lens, the author considers the results of his experiments as substantially confirming Cheselden's doctrine; Phil. Trans. for 1806, p. 83 et seq.

When objects are placed at only a moderate distance, but such as not to require the adjustment of the eye, when we direct the two eyes to the object, we incline them inwards, as is the case likewise with very short distances, so that what are termed the axes of the eyes, if produced, would make an angle at the object, the angle varying inversely as the distance. Here, as in the former case, we have certain perceptions excited by the muscular efforts necessary to produce a proper inclination of the axes, and these we learn to associate with certain distances[2]. As a proof that this is the mode by which we judge of those distances where the optic axes form an appreciable angle, when the eyes are both directed to the same object, while the effort of adjustment is not perceptible, it has been remarked, that persons who are deprived of the sight of one eye, are incapable of forming a correct judgment in this case[3].

When we are required to judge of still greater distances, where the object is so remote as that the axes of the two eyes are parallel, we are no longer able to form our opinion from any sensation in the eye itself. In this case we have recourse to a variety of circumstances connected with the appearance of the object; for example, its apparent size, compared with what we know to be its real size, the distinctness with which it is seen, the vividness of its colours, the number of intervening objects, and other similar accidents, all of which obviously depend upon previous experience, and which we are in the habit of associating with different distances, without, in each particular case, investigating the cause on which our judgment is founded[4].

It is generally admitted that we judge of the magnitude of objects by experience and association. We know that, according to the laws of optics, the farther an object is removed from the eye, the smaller must be its image on the retina. We find, however, that our opinion respecting the magnitude of bodies is quite independent of the size of this image, but that we deduce our ideas of its size entirely from our supposed knowledge of its distance [1]. We often commit the most singular mistakes respecting the size of bodies, when we are ignorant of their distance from us, and, more particularly, when we are prevented from correcting our mistakes respecting the distance by the peculiar situation in which the body is placed. The arts of landscape and architectural painting, and, still more remarkably, the science of perspective, depend entirely upon the principle, that we judge of the size of bodies by their distance. If the artist is able to convey to our minds a correct conception of the position in which the different objects are supposed to stand with respect to each other, we immediately conceive of them as presenting the size that they actually possess, without any relation to the space which they occupy upon the canvass.

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[1] These cases fall under the remarks of Berkeley in § 16, although he has not entered upon the consideration of the nature of the effect; Essay, p. 17.

[2] This is the case to which the remarks of Berkeley in § 16, particularly apply; Essay, p. 9.

[3] See Reid on the Human Mind, ch. 6. sect. 22, 3; also Magendie, El. Phys. t. i. p. 87, 8.

[4] We have an elaborate examination of this subject by Smith; Optics, § 138, and Remarks, § 235..248. See also Haller, El. Phys. 16. 4. 31. Porterfield enumerates six methods which are employed, according to circumstances, in the judgments which we form of the distance of objects; "their apparent magnitude, the vivacity of their colours, the distinction of their smaller parts, the necessary conformation of the eye for seeing distinctly at different distances, the direction of their axes, and the interposition of other objects;" Ed. Med. Ess. v. iv. p. 282; also "On the Eye," v. ii. p. 409.

The third problem which we proposed to investigate, the means by which we judge of the position of bodies, is one that has been supposed more difficult to solve. We know, both from the laws of optics and from the experiment of Kepler mentioned above, that when the rays of light pass through the eye, and are brought to a focus upon the retina, the image is reversed, yet we form a conception of it as existing in its natural position. The question has then been asked, why do the reversed images give a correct perception? When we speak of two points in space, as being one above the other, or one to the right of the other, do we mean to express that there is some natural and necessary connexion between these points and their visible position, depending on the structure of the eye, or on any innate or intuitive perception, or do we acquire our knowledge of visible position, like that of distance and magnitude, by the gradual influence of experience and association? In the case of a blind man suddenly restored to sight, as in that of Cheselden, would he perceive objects in their erect position, or would he conceive them to be reversed? Berkeley, in conformity with his system, extends his hypothesis to visible position, as well as to distance and magnitude, and supposes that our perceptions respecting it are acquired by experience [2]. The blind man, according to his doctrine, would have no conception of the relative position of the two points, until he had exercised his touch, or had learned from some other source, that one of them was more distant from the surface of the earth than the other, and thus associated his visible with his tangible perceptions.

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[1] Berkeley's Essay, § 55..64, p. 60..71 et alibi. There are many correct and judicious remarks on the means by which we judge of the distance and magnitude of objects in Hartsoeker's Essai de Dioptrique, art. 13..7, p. 85..8.

[2] Essay, § 88..100, p. 103..118. Haller, El. Phys. 16. 4. 7. agrees with Berkeley; yet he seems to consider it as a difficulty. Smith also refers it to association with the touch; Optics, § 135, 6.

Porterfield supports the contrary opinion, and endeavours to prove, that there are certain ideas of position implanted in the mind, independently of experience, or of any association with the touch, and which necessarily directs us in forming our conclusion respecting the relative situation of objects[1]. A similar doctrine is maintained by Reid, who, like Porterfield, lays down certain positions, which he conceives to be original laws of the constitution, and what is a stronger ground, he endeavours to show that we have no evidence of any case in which objects appeared reversed, while both the eye itself and the nerve connected with it were in a sound state[2].

If I were required to give a direct answer to the question under discussion, I should feel disposed to decide in favour of Reid's opinion, principally from the considerations mentioned above. It may be farther remarked, that Cheselden's case, although perhaps not unequivocal, favours this view of the subject; for it is not probable that a person so intelligent as his patient appears to have been, and who was able to give so full and clear an account of his sensations, would not have been aware of the inverted position of objects, and of their gradually assuming the erect position, had he been obliged to correct his ideas on this point by the operation of experience. The effect that is produced by applying pressure to the ball of the eye seems also in favour of the opinion of Reid; for we find that, upon whatever part the pressure be applied, we have the impression of an obscure circle of light precisely on the opposite side of the eye.

But I conceive that the discussion concerning the supposed want of correspondence between the mental perception and the picture upon the retina, is founded altogether upon an incorrect view of the subject. It seems to proceed upon the principle, that in receiving the impressions of sight, we ourselves view the image on the retina, whereas all that we know is, that the impression is in some way conveyed by the optic nerve to the brain, and constitutes a perception; but we are totally ignorant of the process by which this is effected, nor do we see the nature of the connexion which subsists between the two events[3].

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[1] On the Eye, v. ii. p. 329, 0; and Ed. Med. Ess. v. iv. p. 129, 0.

[2] On the Human Mind, ch. 6, sect. 11, 2. His general proposition is, that we see objects in "the direction of the right line that passes from the picture of the object upon the retina to the centre of the eye." p. 169.

[3] See Young's Lectures, v. i. p. 449. Reid has very satisfactorily shown that our perceptions do not bear any necessary resemblance to the impressions that are made upon the organs of sense, from which they are derived; On the Human Mind, sect. 6. A curious question, which, I conceive, may be referred to this part of our subject, has been lately made the topic of investigation by Dr. Wollaston, the cause of the apparent direction of the eyes of a portrait. By a series of plates, in which, while the eyes remain unchanged, the lower parts of the face are altered, it would appear evident that our conception of the direction of the eyes is, in a great measure, derived from the disposition of the other features, proving that we form these con-

There seems, therefore, no reason why the inversion of the image should lead to the conception of an inverted object rather than the contrary, and hence the question that has been so frequently asked, why do we not see objects inverted? may be answered by asking in return, why should we expect this to be the case? The problem that was proposed by Berkeley, respecting the means by which we acquire our ideas of visible position, is of a more general nature, and one that is highly deserving of our attention[1]; but I conceive that we are scarcely yet in possession of any facts or arguments which can lead to a satisfactory solution of it[2].

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ceptions more by association than by the absolute state of the eye itself; Phil. Trans. for 1824, p. 247 et seq. pl. 9..11. Probably, however, a part of the effect depends upon the small scale on which the drawings are made; were they painted the size of life, I conceive that they would exhibit a very distorted appearance.

[1] Berkeley distinctly states, that when the blind man first acquired his sight he "would not think, that any thing which he saw was high or low, erect or inverted." § 95. p. 112. This reasoning proceeds upon the principle, that he would have no conception of visible position, until it was gradually acquired through the medium of the touch; see also § 115..9. p. 134..140; this, it may be observed, is a totally different state from the conception of an inverted object.

[2] Sir C. Bell has endeavoured to prove, that we judge of the position of objects by the feelings attendant upon the motion of the muscles of the eye. "When an object is seen," he says, "we enjoy two senses; there is an impression upon the retina; but we receive also the idea of position or relation, which it is not the office of the retina to give. It is by the consciousness of the degree of effort put upon the voluntary muscles, that we know the relative position of an object to ourselves." Phil. Trans. for 1823, p. 178. He illustrates and endeavours to prove his doctrine by a series of experiments, in which, after obtaining an ocular spectrum in the eye, he found that the apparent position of the spectrum followed the motion of the ball, as long as this motion was affected by the contraction of the muscles, but that when the motion of the ball was produced by pressure with the finger, the association no longer existing, the spectrum did not appear to move; p. 178..0. Sir C. Bell's experiments, and the hypothesis which is derived from them, have been controverted by Sir D. Brewster, who alleges that, according to the known laws of optics, the apparent motion of the spectrum, when the eye ball is pressed aside by the finger, should be much less considerable than Sir C. Bell has supposed it to be, and that this small motion of the spectrum may actually be observed; Edin. Journ. Scien. v. ii. p. 1 et seq. I will not venture to decide upon this point; I am aware of the delicacy of the experiment, and of the great skill and sagacity of Sir D. Brewster in investigations of this nature; but I may be allowed to state, that in repeating the experiments, as I conceived with the necessary precautions, my results appeared to agree with Sir C. Bell's. But allowing the correctness of Sir D. Brewster's observations with regard to the effect produced upon the spectrum by pressing aside the ball of the eye, I still do not perceive that it will influence our conclusion, that in the *ordinary* actions of the organ, our judgment of the relative position of external objects is much influenced by associations formed with the contraction of the muscles of the orbit. The circumstances mentioned by Sir D. Brewster, viz. the spectrum following the motion of the head, or that of the whole body, when either the head alone or the whole body is moved, prove no more than that the motion of the muscles of the eye is not the only source whence we derive our ideas of visible position or of visible motion. We have some observations by Mr. Shaw, on the sensations pro-

I must now offer some remarks upon a subject which has given rise to much discussion and to numerous experiments and upon which it appears that we are still unable to form any decisive opinion; the cause of single vision with two eyes. When the eyes are both of them directed to an object, a separate image is formed upon each of the retinæ, yet the mind forms the conception of only one object. The same question here presents itself as in the former case, is there any thing in the nature of vision, or in the constitution of the eye, which causes the object to appear single, or does the effect depend upon association and experience? Are two distinct impressions actually conveyed to the mind, or is there in reality only one perception received by the sensorium?

The opinions that have been formed on this point may be arranged under four heads. It has been maintained by some physiologists, that although a separate impression is made upon each retina, yet in consequence of the conjunction of the optic nerves, these impressions become united, and as it were amalgamated, before they arrive at the sensorium commune, so as to produce only one perception [1]. An idea of this kind seems to have been generally adopted by the older physiologists, derived partly from the fact of our being conscious of only one impression, and partly from the apparent union of the optic nerves in their passage from the retina to the brain, the use of which it was otherwise difficult to explain. I have already had occasion to offer some remarks on the nature of the connexion which exists between the optic nerves [2]. The subject has since been farther investigated by Dr. Wollaston, who conceives that what he terms a semi-decussation of them takes place, a portion of the fibres of each nerve crossing at the part where they come into contact, and passing on to the opposite side of the brain [3].

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duced by the muscular motions of the parts connected with the eye, which, as he conceives, assist us in the determination of the position of objects; Instit. Journ. v. ii. p. 239 et seq.

[1] This opinion may be considered as sanctioned, to a certain extent, by the authority of Newton; Optics, Quær. 15. Opera, t. iv. p. 221; and was the one supported by Briggs; Nov. Vision. Theor. p. 17..31. The tendency of Kepler's reasoning on this subject appears to be, that when the two retinæ are similarly affected, we cannot distinguish between the two impressions, and therefore conceive of them as constituting only a single impression; Diop. Remarks upon Prop. 62.

[2] For an account of the opinions previously entertained upon this subject, see Porterfield, Ed. Med. Ess. v. iii. p. 196..207, and On the Eye, v. i. p. 189 et seq. B. 2. ch. 9; Boerhaave, Præl. not. ad § 516. t. iv. p. 62, 3; Haller, El. Phys. 16. 2. 2.

[3] Phil. Trans. for 1824, p. 222 et seq. Future observations must determine how far the anatomical facts that have been brought forwards in support of the distinct course of the two optic nerves can be reconciled with the pathological arguments; see Briggs, Nov. Vis. Theor. p. 10, 1; Porterfield on the Eye, v. i. p. 191, 2; Cheselden's Anat. p. 294, 5; Zinn, Descr. Oculi Hum. cap. 9. § 2. We have, however, equally, or even more powerful evidence brought forward by Sœmmering in favour of the decussation; De Decussa-

The pathological facts from which Dr. Wollaston derived his opinion, prove that a certain consent or sympathy exists between the functions of the retinæ; yet it may be doubted how far it will apply to the explanation of the case now under consideration.

A second opinion that has been maintained on the subject is, that we do not actually receive the perception of the two impressions at the same time, but that vision consists in a rapid alternation of the eyes, according as the attention is directed to one or other of them by accidental circumstances. This hypothesis was embraced by Dutours, who attempted to prove it by

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tione Nerv. Opt. in Ludwig. Script. Neur. t. i. p. 127 et seq. The experiments and observations of Magendie would tend to the opinion that there is a complete decussation; El. Phys. t. i. p. 63; while the dissections of Mr. Mayo seem to demonstrate that it exists to a certain extent; pl. 7. fig. 3. Mr. Twining, on the contrary, has adduced various cases of disease in the optic nerves, from which he argues against their union or semi-decussation: Brewster's Journ. v. ix. p. 143, et seq. We are informed that Treviranus has lately investigated this subject, and that by examining the optic nerves, after their consistence had been hardened by the action of alcohol, he finds that a part of their fibres pass on from their origin to the retina of the same side, while those of the interior and lower part of the nerve appear to unite together; but it could not be perceived that any of them actually crossed; Lond. Med. Phys. Journ. v. L. p. 516. According to this account they may be considered as forming what may be termed a commissure, analogous to what is supposed to take place in certain parts of the brain; we may suppose that in the case of the optic nerves, as well as in that of the other parts of the nervous system, the mere apposition or contact of the parts may serve for the transmission of the nervous influence, without the actual continuation of the fibres by means of the decussation. On this subject see Adelon, Physiol. t. i. p. 402, 3; and Desmoulins, Anat. des Syst. Nerv., p. 334, where we have some remarks on the state of this part in the various classes of animals. There are certain animals, as for example the chameleon, which are said to have the power of turning the two eyes in different directions, and a great variety of animals have their eyes so situated, that they must, in most cases, be necessarily directed to different objects; it would be desirable to examine the optic nerves of these animals, whether they decussate or unite. Since writing the above I took an opportunity of mentioning the subject to Mr. Owen, and requested him to give me an account of the state of the optic nerves in those animals, where the eyes are necessarily directed, for the most part, to different objects. He accordingly favoured me with the following interesting observations.

"Coll. of Surg., July 22.

"My dear Sir—According to your request, I have proceeded to examine the brains of birds, with reference to the decussation of the optic nerves, and find, in every case, that it is, to external appearance, complete. Having a brain, preserved in spirits, of the largest of the class, viz., the ostrich, I have dissected this part, and find that the decussation is more general than in man; there is not, for example, any external band, passing from the optic nerve along the side of the chiasma to the continuation of the nerve of the same side, while some of the faciculi go quite across to the outer side of the opposite nerve, and change their level as they proceed across. Believe me very faithfully yours, RICHARD OWEN."

The circumstance of the decussation being more complete in those cases, where the eyes are so situated as to be, in most cases, incapable of being directed to the same object, would appear to be almost decisive against the hypothesis referred to in the text.

experiment[1], and it is the one to which Haller inclines[2]; but it is supposed to be entirely overthrown by an observation of Jurin's, that when we direct both the eyes to an object, we see it with more vividness than when viewed by one alone. This increased vividness he found to be a constant quantity, which, in a sound eye of the ordinary degree of power, he estimated at one-thirteenth of the whole effect[3]. The experiment of Dutours, to which I refer, consisted in directing the sight of the two eyes through two tubes, to the ends of which two glasses of different colours are respectively attached; in this case we have not a perception compounded of the two colours, but we see first one and then the other, or sometimes one appears to be placed over the other, or to be seen with more vividness, but they always remain more or less distinct.

These two hypotheses, although they properly come under our consideration in this place, must be regarded, strictly speaking, as not offering any solution of the proposed question, but rather as showing, that the difficulty which was supposed to attach to it does not really exist. The two remaining hypotheses, however, proceed upon the idea that the impressions are both of them separately conveyed to the brain, but that they produce there only one perception. According to the first of these, the effect depends upon some law of the constitution, or some general principle of vision, which enables us to see the object single, independent of any mental impression; while, according to the other, the single perception is not supposed to take place in the first instance, but to be the gradual result of habit and association.

One of the first writers who entered upon the discussion of this question is Porterfield; he endeavoured, by an elaborate and learned investigation of the laws of vision, to show that, from the natural constitution of the organ, we always see objects in their proper situation, and that therefore, as each eye must see the object in the same place, we can have no conception of more than one object[4]. Reid has satisfactorily shown that Porterfield's reasoning is fallacious, and has pointed out various circumstances which are in direct opposition to his conclusion[5]. He, however, adopted the opinion of Porterfield, that single vision is the result of a natural law of the constitution, but he explains it upon a different principle. He endeavours to show that whenever the impressions of objects are received upon what he terms corresponding points of the retinæ, such points are similarly situated with respect to their centres, and that, in this case, a single perception is necessarily excited[1]. It is admitted that Reid supports his position by many plausible arguments, but Wells has proved, by an ingenious train of experiments, that it does not hold good in all cases, and that it cannot therefore be considered as a general law of the constitution[2]. He likewise remarks, with great justice, that Reid's hypothesis is not strictly conformable to the anatomical conformation of the eye, the crystalline not being situated exactly in the centre of the organ[3]; and moreover, that it is contrary to the analogy of the general structure of the body, according to which these corresponding points should be both of them within, or both of them without the centres of the retinæ, not as Reid supposes, both of them on the same side of the centres. The experiment of Dutours, which was mentioned above, has also been supposed to be adverse to Reid's hypothesis. If we look through a single tube, to the end of which both a blue and a yellow glass are attached, we perceive a green colour. Now it has been argued, that if the corresponding points of the retinæ have a natural sympathy with each other, when we look through two tubes, one of which has a yellow and the other a blue glass, the impressions ought to become united and produce the perception of green; but this is never the case[4].

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[1] Mém. présentées à l'Acad. t. iii. p. 514 et seq. and t. iv. p. 499 et seq.

[2] El. Phys. xiv. 4. 10; he seems disposed to refer it, in part at least, to the principle, that the mind is unable to distinguish between two perfectly similar impressions of one kind, whether on the nerves of the eye, the ear, or any other of the organs of sense.

[3] Smith's Optics; remarks, § 697; Porterfield, On the Eye, v. i. p. 71 et seq.

[4] Ed. Med. Ess. v. iii. p. 208 et seq.; On the eye, v. ii. p. 279 et seq.

[5] On the Mind, ch. 6. sect. 10.

The hypothesis which is advanced by Wells to account for single vision, although differing from that of Reid, may be placed in the same class, as supposing it to be derived from a law of the constitution, and to be independent of any mental operation. He performed a series of experiments from which he deduced the conclusion, that objects appear single where they are seen in the direction of the optic axes, and as it appears that this single vision is occasionally produced under circumstances different from those in which the eye is ordinarily placed, it seems to follow, that it cannot be the result of habit or association[5].

The last hypothesis is the one which supposes that we naturally see objects double, but that, finding by experience, that one object only exists, we learn to disregard the actual perception conveyed to the mind, and conceive of the object as single. The principal writer who has supported this doctrine is Smith. He proceeds upon the principle of the Berkeleyan theory, extending it to the case of single vision, to which it appears not to have been applied by its author [1]. But notwithstanding the clearness with which it is stated by Smith [2], I conceive that it is successfully combated both by Reid [3] and by Wells [4]. It is asserted that there is no instance on record, where any one ever acquired the power of single vision, when the optic axes were not similarly directed. It is moreover urged against Smith's doctrine, that in Cheselden's case, and others of a similar kind, double vision has never been observed to occur, while it is remarked, that in infants, and even in blind persons, the eyes always move together, unless from some mechanical or morbid cause, which obviously affects the action of the muscles, or deranges the general functions of the organ.

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[1] Ch. 6. sect. 13. It is necessary to remark, that Reid uses the term corresponding points physiologically; see p. 285. Smith, on the contrary, employs it anatomically, to designate points that are similarly situated with respect to the centres.

[2] Essay on Single Vision, p. 18..32. p. 382 et seq.

[3] Essay, p. 21..4.

[4] Wells's Essay; p. 45. note. It would appear that Reid, notwithstanding his habitual candour, was so far influenced by hypothesis, as to affirm that this composition of colours actually takes place; On the Mind, p. 203. Scherffer conceived that he produced a violet colour by looking through a blue and a red glass; Journ. Phys. t. xxvi. p. 284; but the change in this case was not sufficiently decisive to warrant the general conclusion.

[5] Essay on Single Vision, part 2. p. 34..62.

The effect of delirium and of intoxication have been adduced in favour of Smith's hypothesis; for it is said that in these cases, where the usual train of associations is interrupted, we have double vision produced. But to this it may be answered, that we have here not a mental, but a physical defect, for if we examine the state of the eyes in these instances we shall observe, that they do not move in a parallel direction, and that, consequently, the impressions are not made upon corresponding points in the retinæ. Upon the same principle, double vision is always produced, when, from accident or disease, the eyes are prevented from moving in concert, and, however long the irregular motion is continued, it is found that the defect of vision remains, and that we never acquire the power of conceiving the impressions to be single, until the physical defect of the eye be remedied [5].

In considering this question, we are naturally led to inquire into the cause of the tendency which we observe in the eyes to move in the same direction; is this a natural propensity, or is it acquired by habit? Smith, in conformity with his general principle, argues in favour of the latter opinion; he remarks, that when both eyes are directed to the same object, we see it more distinctly than when viewed by one, and, finding this to be the case, we insensibly acquire the custom of moving the eyes together[1]. The contrary doctrine is adopted by Reid[2], and I conceive that it is sanctioned by experience; for it would appear that where the organ is sound, and there is no malconformation of the neighbouring parts, the eyes will be found to have a natural tendency to move in the same direction, and that this parallelism of motion is observed in the eyes of very young children, and even of blind persons, or of those who have the sight of only one eye, in which cases we cannot suppose that it has been acquired by any operation of habit or association[3].

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[1] Reid, indeed, affirms that Berkeley directly maintains this opinion; On the Human Mind, p. 332; but, I believe, it will not be found in his Essay, although it may be supposed to be a necessary consequence from his general principles.

[2] Optics, § 137.

[3] On the Mind, ch. 6. sect. 17.

[4] Essay on Single Vision, p. 9..18.

[5] Cheselden, indeed, mentions an instance, where a person, in consequence of an injury, had the eyes distorted, and consequently experienced double vision, who afterwards gradually acquired the power of seeing objects single, although the distortion was not removed: Anat. p. 295, 6. But it may be questioned, whether in this case the sight of the distorted eye was not so far impaired as that the patient ceased to attend to the impression. I must remark, however, that Camper admits the correctness of Cheselden's observation, and advocates the hypothesis of Smith; De Visu, in Haller, Disp. Anat. t. iv. p. 243.

Now if the muscles of the eye are so constituted and so connected with the nervous system, that in their natural state they have a tendency to place the eyes in such a position with respect to each other, as that the impressions of an object are formed upon corresponding parts of the two retinæ, it would seem to follow as a probable inference, that some farther purpose was to be obtained, and that there must be a natural sympathy or connexion between the corresponding parts of the retinæ, which, without any mental effort, produces only a single perception. Notwithstanding, therefore, the anatomical arguments that have been adduced against Reid's doctrine of corresponding points, I am disposed to regard it as not without foundation, and that even if this cannot be maintained, that there are stronger and more direct objections against the hypothesis, which accounts for single vision upon the principle of habit and association.

It may be proper to notice in this place that peculiar state of the eyes which produces squinting, as it has been supposed to throw some light upon the theory of single vision. In the individuals who are the subjects of this defect, the eyes do not move in the same direction, and it was supposed by many of the older physiologists, that it depended upon a want of correspondence between the retinæ; and that, in order to produce the same effect upon each of them, it was necessary that the impressions should be made upon different parts of the surfaces[4].

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[1] Optics, § 137. Porterfield also conceives that it depends upon habit, and, in proof of his opinion, remarks, that the eye-lids and other neighbouring parts have the same tendency to move in corresponding directions, a circumstance which, I apprehend, is rather unfavourable to his opinion; Ed. Med. Ess. v. iii. p. 255, also On the Eye, v. i. p. 118, and v. ii. p. 326.

[2] On the Mind, ch. 6. sect. 10.

[3] Dr. Wollaston supposes that the parallel motion of the eyes is connected with the partial union of the optic nerves; Phil. Trans. for 1824, p. 229.

[4] Delahire supposed that squinting depends upon the most sensible parts of the two retinæ not being similarly situated with respect to their centres;

There are two points to be ascertained before we can form a correct judgment concerning the cause of squinting; do the persons who squint use both their eyes at the same time? and, if so, do they see objects double? To both these questions it seems that we must answer in the negative, as we find that, when they look attentively at an object, they never use more than one eye. The immediate cause of the other eye not being directed to the object, or rather being drawn away from it, appears to depend upon its vision being imperfect[1], so that if it were directed to the object together with the sound eye, it would produce a confused impression, and it is to prevent this defect that the habit of turning the eye aside is unconsciously acquired. This view of the subject was proposed by Buffon, and our subsequent observations seem to justify his opinion[2]. Hence we perceive that the idea which was formerly entertained respecting the cause of squinting, as depending upon a want of correspondence between the different parts of the two retinæ, is without foundation, and that consequently it throws no light upon the nature of single vision.

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see his treatise, Accidens de la Vue, § 10, in Mém. Acad. t. ix. p. 530 et seq.

[1] A case which is related by Darwin, in Phil. Trans. for 1778, p. 86 et seq., seems, however, to prove that there are occasional exceptions to this general principle.

[2] Mém. Acad. pour 1743, p. 231 et seq. Jurin had previously refuted Delahire's hypothesis referred to above; Essay attached to Smith's Optics, § 178..194; he ascribes it to a habit acquired early in life of directing only one eye to the object. Porterfield, who considers in detail the phenomena and causes of squinting, enumerates six different circumstances, by which he conceives it to be produced; one of these is that assigned by Buffon; Edin. Med. Essays, v. iii. p. 237 et seq. Dutours proposed a modification of this hypothesis; he supposes that one of the retinæ is, in these cases, more sensible to light than the other, and is consequently turned away from the object; Mém. Présent. à l'Acad. t. vi. p. 470 et seq. Reid, On the Mind, sect. 16; Priestley, On Vision, per. 6, sect. 12. ch. 3; Sir Ev. Home, Phil. Trans. for 1797, p. 12..8; and Sir C. Bell, Anat. v. iv. p. 456 et seq., adopt the opinion of Buffon.

CHAPTER XIV.

OF HEARING.

After the sense of sight, that of hearing will next claim our attention, both in consequence of its real importance in the various concerns of life, and of the elaborate structure of the organ by which it is exercised. We have also a tolerably correct knowledge of the nature of its specific cause, of the mode in which the ear receives the impressions of sound, and of the manner in which they act upon it. We have, however, a much less perfect idea of the use of the different parts of the ear than of the eye, we have less command over the cause of sound, when we attempt to make experiments upon it, and we are also less able to obtain a perfect knowledge of the acquired perceptions of hearing. In this chapter I shall first give some account of the structure and functions of the ear, and shall afterwards make some remarks upon the acquired perceptions of hearing.

Sect. 1. *Account of the Structure and Functions of the Ear.*

Sound[1] is excited by the vibration or oscillation of the particles of certain bodies, which, from this circumstance, are termed sonorous. They are of different kinds, and are found in all the three mechanical states in which bodies exist, of solid, fluid, and aeriform. These vibrations are capable of being transmitted from one body to another, either of the same or of different kinds, and are increased or diminished according to the nature of the body by which they are successively received. The air of the atmosphere is the medium by which sound is, in most cases, conveyed to the ear, although we find that both fluids and solids are, under certain circumstances, capable of transmitting it, and probably even with greater force and velocity[2]. If a gun be fired at sea, and the ear be, at the same time, immersed in the water, we receive two impressions of sound, the first being the one which is conveyed by the water, in consequence of this body being a better conductor of sound than the air. In the same way, if a loud sound be produced at one extremity of a long series of metallic rods, and the ear be placed at the other extremity, two sounds will be heard, the first of which is conveyed along the metal, and the second through the air.

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[1] For an account of the production of sound, its transmission from one body to another, and the various modifications which it experiences, it will be sufficient to refer to the learned and elaborate work of Dr. Young; Lect. v. i. No. 31..4. An ample list of references is contained in v. ii. p. 264 et seq. The authors who have treated on the "Ear and Hearing," are enumerated in p. 271, 2.

[2] It appears, indeed, that in the ear, the medium by which the undulations of the air are ultimately conveyed to the auditory nerve, is probably a fluid; as it is well known to anatomists, that the internal cavities of the organ, which we presume to be the seat of the perceptions of sound, are filled with a substance of this description. Its existence seems to have

Sound, like light, is capable of being reflected from a body at a definite angle, and concentrated into a focus, although in a less precise manner. Upon this principle it is that echoes are produced, and that the vibrations which constitute sound are increased by speaking-trumpets, domes, whispering-galleries, &c., which may be regarded as analogous in their operation to convex lenses or mirrors. In the same way sounds are increased by hearing-trumpets, and we presume that the cartilaginous folds of the external ear have a similar effect in receiving sounds, and transmitting them to the internal parts of the organ.

The ear may be considered as consisting of three orders of parts. The first is composed of the external ear, consisting of a cartilaginous body of a peculiar form which is attached to the integuments of the head, and receives the undulations of sound, and of a tube, called the meatus externus, which conveys the undulations to the second order of parts. These consist of a cavity in the temporal bone, named the tympanum, or drum of the ear, which contains the minutely organized bodies that serve to modify the vibrations of sound, as well as the nervous expansion, which is to be regarded as the immediate seat of the sensation, analogous to the retina of the eye. Lastly, we have the Eustachian tube, a passage which extends from the posterior part of the tympanum into the fauces[1]. The use of the

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been first ascertained by Schellhammer; it was distinctly recognized by Valsalva and others, but the subject was so much elucidated by the researches of Cotunni, that his name is generally attached to the receptacles in which it is lodged; see his treatise De Aquæductibus Auris Humanæ internæ Anatomica Dissertatio; also Meckel's dissertation De Labyrinthi Auris Contentis, where we find the subject amply considered, as well as various other parts of the anatomy of the ear. Breschet's late researches on the ear have led him to the discovery of a second fluid, in addition to the one mentioned above, to which he gives the name of vitrine auditive; the former he names perilymphe; see his work, Sur l'Organe de l'Ouie, the same inserted in Ann. Sc. Nat. t. xxix. p. 129 et seq.

[1] The first complete account of the structure of the ear was given by Duverney, in the year 1683; the work contains a number of plates; see Mém. Acad. t. i. p. 256..9. In the following year a valuable treatise on the ear was published by Schelhammer. Valsalva's treatise, De Aure Humana, is regarded as one of the most accurate productions of the anatomists of the seventeenth century. It not only comprehends a very full account of all that was known respecting the ear at the time of its publication, in 1704, but contains many original observations, and is accom-

external parts of the ear in receiving the vibrations of the air is more peculiarly exemplified in some of the inferior animals, in whom it is of considerable size, and is furnished with muscles, which are under the control of the will, and by which its orifice is turned towards the sounding body[1]. By this means the impressions of sound are received in their full force, while the elastic nature of the part tends to increase the vibrations, and to convey them in the most advantageous manner to the tympanum.

The tympanum of the ear, and the parts connected with it, exhibit a minute organization of various structures, osseous, membranous, and nervous; all of which, we conclude, serve some specific purpose in the modification of the vibrations, the reception of them by the sensitive part, and the transmis-

panied with a number of engravings, which, although not executed in a style of great elegance, are well adapted to illustrate the text. Eustachius's letter, De Auditus Organis, written in 1562, contains an account of the tube which bears his name; Opusc. Anat. p. 138..0. Fallopius, in his Observ. Anat. p. 364..6, briefly describes the ear, and Fabricius more copiously in his treatise, De Aure, Op. p. 249 et seq. To these we may add the following works as deserving our attention: Perrault, on the Organ of Hearing, in Mém. Acad. t. i. p. 158..161; Winslow, Anat. sect. 10. art. 4. v. ii. p. 312 et seq.; Boerhaave, Prælect. § 547..565, t. iv. p. 139..201 cum notis; Haller, El. Phys. lib. xv. in the three sections of which are considered the structure of the organ, the theory of sound, and the sense of hearing; Cassebohm's Five Treatises, accompanied with numerous figures; Martin's Description of the Ear, in his Philos. Brit. v. ii. p. 219..4; Sabatier, on the Internal Ear, in his Anatomie, t. ii. p. 127..148; Boyer, on the same, in his Anatomie, t. iv. p. 136..169; Scarpa, Disquisitiones de Auditu et Olfactu; Monro, sec. in his Three Treatises; Bichat, Anat. Des. De l'Oreille et de ses Dependances, t. ii. p. 472 et seq.; Caldani, Icon. Anat. pl. 96..0, several of these are original; Saunders, on the Ear, the three first chapters, with the plates; Bell's Anat. v. iii. part 2. book 2. ch. iv. p. 399 et seq.; Young's Lect. pl. 25, fig. 349..351; Esser, on the functions of the different parts of the auditory organs, Ann. Sc. Nat. t. xxvi. p. 1 et seq.; the researches of Breschet referred to above; Cloquet, Anat. p. 362..379, pl. 128..131; Roget's Bridgewater Treatise, v. ii. p. 420 et seq.; and the elaborate plates of Sœmmering, Icones Org. Aud. Hum.; the same inserted in Cloquet's Man. pl. 144..8. For the comparative anatomy of the organ, see Cuvier, Leç. d'Anat. Comp. No. 13, t. ii. p. 446 et seq.; and Pohl, Expositio Anat. Org. Aud. per Classes Anim.

[1] In man the auricle is seldom moveable; it has, however, its appropriate muscles, and there are certain individuals who possess a degree of voluntary power over it; Haller, El. Phys. xv. 1. 4. It would seem to be one of those parts, in which an organ, that is eminently useful in some tribes of animals, in others is of little importance, and is therefore imperfectly developed. We are informed, however, by Sœmmering, in his essay, on the Comparative Anatomy of the European and the Negro, that "savages can move their ears at pleasure, and possess the sense of hearing in great perfection." Appendix to White, on the Gradation in Man, p. cxliii. True external ears are only found among the mammiferous quadrupeds, and in some of these they are wanting. For figures of this part, see Cowper, Anat. Corp. Hum. tab. 12. fig. 1; Albinus, Acad. Annot. lib. 6. tab. 4, and his work on the Muscles, tab. 11. fig. 3, 4, 5; Sœmmering's Icones, tab. 1.

sion of them, by means of the auditory nerve, to the sensorium commune. The small bones or ossicles of the ear are of a very peculiar form and elaborate structure. There are also many singularly-shaped canals, excavated in the temporal bone, which communicate with the ear; but of the specific use of these parts it may be said that nothing is certainly known[1]. These canals are lined with membranes, and there is a membrane of considerable size, called the membrana tympani, which is stretched across the cavity of the ear, so as entirely to separate the meatus externus from the internal parts of the organ. It is upon this membrane that the chain of ossicles is disposed, being attached one to the other, and connected with the orifices of the canals, so as to render it probable, that the vibrations of sound are first received upon the membrana tympani, are communicated by this part to the ossicles, and by them to the bony canals[2]. The auditory nerve, which receives the vibrations of the ossicles and canals, and which constitutes the immediate seat of the sense of hearing, is expanded upon the surface of these canals, or on the parts immediately contiguous to them.

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[1] Blumenbach differs from most of the modern anatomists in supposing that the number of ossicles is three only, the small lenticular bone being generally absent; Inst. Phys. § 248. p. 143. We have a view of the minute muscles belonging to the ossicles in Albinus's great work, tab. 11, fig. 3, 4, 5. Magendie has pointed out some circumstances in the arrangement of these muscles, which appear not to have been previously noticed; Journ. Physiol. t. i. p. 341 et seq. Mr. Chevalier has lately published some minute observations on the ligaments of the ossicles; Med. Chir. Tr. v. xiii. p. 61 et seq. He states the particulars of a case in which the loss of these bones did not destroy the sense of hearing, p. 68, note. We have a series of curious experiments by Flourens, which consisted in dividing the semicircular canals of the ears of birds: It appeared that, according to the particular canal which was divided, or the direction in which the section was performed, whether vertical or horizontal, that quick and violent motions of the head took place, up or down, or from the right or the left, or the contrary; Expér. sur le Syst. Nerveux, p. 42 et seq.; also in Mém. Acad. Sc. t. ix. p. 455 et seq. et p. 467 et seq., and Ann. Sc. Nat. t. xiii. p. 113 et seq. Breschet's observations on the various cavities of the ear are contained in the 6th chapter of his "Etudes;" Ann. Sc. Nat. t. xxix. p. 315 et seq. In connexion with this subject I must not omit to mention the hypothesis of St. Hilaire, that the bony or scaly appendages to the branchiæ of fish, which are concerned in the mechanism of respiration in these animals, are analogous to the four ossicles of the ear in the mammalia, birds, and reptiles. These appendages, which were formerly known by the general name of opercula, have received from Cuvier the distinctive appellations of opercule, pre-opercule, inter-opercule, and sub-opercule, names derived from their respective positions. The hypothesis is detailed in the 5th memoir of his "Philosophie Anatomique," and forms a part of what has been called the theory of analogies, the fundamental position of which is that the organization of all vertebrated animals may be reduced to a uniform type, and that every part which is found in each class has an analogous part in the other classes. The author brings forward various proofs of his position, but I conceive that it still remains doubtful how far it is to be regarded as applicable in all cases. I may refer on this subject to Blainville's valuable work, "De l'Organisation des Animaux;" p. 553.

[2] Scarpa, who devoted much attention to the minute anatomy of the internal parts of the ear, has described a membrane attached to the orifice of the fenestra rotunda, to which he has given the name of tympanum secunda-

We are so far acquainted with the nature of sound, and with the physical properties of the minute parts of the ear, as to enable us to assign their general use, but we do not seem to be warranted in carrying our explanation beyond this point. The use of the membrana tympani has been made a particular object of inquiry; its situation in the centre of the organ, and its connexion with the other parts, as well as its size and structure, seeming to mark it out as serving some peculiarly important purpose in the action of the organ. Sir E. Home had an opportunity of examining the membrana tympani of an elephant, and was able to detect a muscular structure in it, by which it would possess the faculty of contracting or relaxing, according to circumstances, and from the delicacy of its organization he was led to conjecture, that it might be the part of the ear which was appropriated to the reception of musical sounds[1]. But this hypothesis was overthrown by a case which occurred to Sir A. Cooper, in which a patient, who had the membrane of one ear entirely destroyed, and of the other considerably injured, still retained his power of perceiving musical sounds unimpaired[2]. There are, indeed, certain facts in comparative ana-

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rium: this he conceives to be of considerable importance in the functions of the organ, and especially in those cases where the membrana tympani is destroyed or injured; De Struct. Fenest. Rotund. &c. cap. 1, 2. in Roemer, Dilectus, v. 1, p. 1 et seq. In connexion with the ossicles we may mention the otoconies of Breschet, to which I have already referred; the chemical analysis of these bodies shows them to be compounded of animal matter and the carbonate of lime; their use is supposed to be to check the sonorous vibrations, analogous to the dampers of the piano; Ann. Sc. Nat. t. xxix. p. 144 et 362. In the same work, t. v. (new ser.) pl. 2, fig. 11, we have a representation of these peculiar bodies from the ear of the Chouette, Strix flammea.

[1] Phil. Trans. for 1800, p. 1 et seq. This paper contains a number of observations on the minute structure of the several parts of the ear, their relation to each other, and the nature of the mechanism by which they contract or relax the membrana tympani. The conclusion which the author deduces is, that "the difference between a musical ear and one which is too imperfect to distinguish the different notes in music, would appear to arise entirely from the greater or less nicety with which the muscle of the malleus renders the membrane capable of being truly adjusted;" p. 12. He had afterwards an opportunity of examining the membrana tympani of the elephant in a more perfect state, from which he confirmed the general accuracy of his former observations on its muscularity, at the same time that he showed that the dispositions of its fibres differed from that of the human organ; Phil. Trans. for 1823, p. 23, et seq. pl. 3..5. From the remarks in this paper, it may be inferred, that the author still maintains his former opinion respecting the specific use of the part. Sir E. Home found also that the membrana tympani of the whale is muscular, but that the fibres are not disposed in the same manner as in the elephant; Phil. Trans. for 1812, p. 83 et seq.

[2] Phil. Trans. for 1800, p. 151 et seq. and for 1801, p. 435 et seq. Che-

tomy, which would lead us to suppose, that some of the bony canals are the immediate organs for receiving the impression of musical sounds, if we are to suppose that there is any part specifically adapted for this purpose. It must be confessed, however, that we have little more than mere conjecture to guide us in our attempts to investigate the uses of the different parts that are connected with the tympanum[1].

There are many pathological facts which prove that the integrity of the Eustachian tube is essential to the perfect function of the ear. When, from any cause, this passage becomes closed or obstructed, the hearing is very materially impaired, while it is restored by removing the obstruction. It is, perhaps, not very easy to ascertain in what mode it acts,

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selden was aware that the integrity of this membrane was not necessary for the perfect accuracy of the sense of hearing; Anat. p. 305, 6. See also Haller, El. Phys. xv. 3. 2.

[1] Speaking of the ossicles and the other minute parts of the internal ear, Magendie remarks, " .... on ignore absolument la part que prend à l'audition chacune des parties de l'oreille interne;" Elém. Physiol. t. i. p. 121. We have a paper by Vicq-d'Azyr, on the Ears of Birds, compared with those of the mammalia; the circumstances the most worthy of notice are, that the cochlea and three of the ossicles are wanting, from which we may infer, that these parts are not necessary for the distinct perception of sound; Mém. Acad. pour 1778, p. 381 et seq. See also Cuvier, Leç. d'Anat. Comp. t. ii. p. 505, and Scarpa's work referred to above, cap. 5. § 10, p. 80, 1. He conceives that the mechanism of the internal parts of the ear in birds, indicates the tympanum secundarium to be essential to the most perfect state of the sense of hearing; § 29, 0. p. 96..8. Mr. Owen remarks that in birds " the ossicula auditus are supplied by a single bone, analogous to the stapes, and some cartilaginous processes representing the rudiments of a malleus and a stapes;" Cyclop. of Anat. v. i. p. 308. Dr. Young remarks, " the use of the semi-circular canals has never been satisfactorily explained: they seem, however, to be very capable of assisting in the estimation of the acuteness or pitch of a sound, by receiving its impression at their opposite ends, and occasioning a recurrence of similar effects at different points of their length, according to the different character of the sound; while the greater or less pressure of the stapes must serve to moderate the tension of the fluid within the vestibule, which serves to convey the impression. The cochlea seems to be pretty evidently a micrometer of sound;" Med. Lit. p. 98; Lect. v. i. p. 387, 8. On the use of the cochlea we have a treatise in two parts by Brendel, in Haller, Disput. Anat. t. iv. p. 399 et seq. and p. 405 et seq., entitled De Auditu in Apice Cochliæ. Sir A. Carlisle has published an elaborate paper on the structure and use of the stapes; Phil. Trans. for 1805, p. 198 et seq. His conclusion is, that " it is designed to press on the fluid contained in the labyrinth by that action, which it receives from the stapedeus muscle, and the hinge-like connexion of the straight side of its basis with the fenestra vestibuli; the ultimate effect of which is an increase of the tension of the membrane crossing the fenestra cochliæ;" p. 206. Savart supposes that the use of the ossicles is to convey the vibrations of the membrana tympani to the labyrinth; Magendie's Journ. t. iv. p. 183 et seq. The investigations of the comparative anatomists show us, that as we ascend from the lower to the higher classes of animals, the semicircular canals are among the parts that are the first developed, and which are the most constant in their form and organization; hence we must regard them as essential to the perfection of the faculty of hearing, although we are, in a great measure, ignorant of their use.

but it may be concluded, that the proper vibration of the membrana tympani is, in some way, connected with the state of the air in the tube, as it is observed, in examining the ears of different kinds of animals, that the membrane and the tube are always found together, or that one of them never exists in the absence of the other.

The ear is provided with an elaborate structure of muscles, some of which are connected with the external part, and although seldom capable of being used in the human species, are constantly employed by some of the inferior animals. Besides the muscular fibres which are disposed over the membrana tympani, there are delicate muscles attached to the ossicles, which seem intended for the motion of these bodies; we are, however, unable to explain the purpose which is served by their motions, or the effect which these motions will have upon the sense of hearing[1].

There are two sets of nerves appropriated to the ear, one for the immediate purpose of receiving the impression of sound, and the other for the general purposes of the nervous influence. The first of these is termed the portio mollis of the seventh pair of the cerebral nerves; its ultimate filaments are dispersed over the internal parts of the organ, and more especially through the bony canals which communicate with the tympanum, constituting, as we may presume, the immediate seat of the sense of hearing, analogous to the retina of the eye[2]. The general nerves of the ear are derived from the fifth pair; they are principally dispersed over the muscular parts, and give the ear the vital properties which preserve it in a due state for executing its various functions[3].

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[1] Magendie has given an account of the muscular and other organs which relax the membrana tympani in man; he finds them to differ from those of the other mammalia, even the simiæ; Journ. t. i. p. 341 et seq.

[2] For an account of this nerve I may refer to Meckel's (Ph. Fr.) Dissertatio de Labyrinthi Auris Contentis, § 22..4. p. 37..42; to Scarpa's Anat. Disquis. de Auditu et Olfactu, tab. 8. fig. 2. for the distribution of the acoustic nerve through the cochlea; to Sœmmering's Icones Organi Auditus Humani; to his treatise, De Basi Encephali, § 76..0; and to Bell's Anat. v. iii. p. 437..9. Mr. Mayo informs us that some of the fibrils of the auditory nerves unite with each other, like those of the optic nerves, thus establishing an anatomical as well as a physiological relation between these parts. Hunter remarks, concerning the distribution of the nerves in the ears of fishes; "the nerves of the ear pass outwards from the brain, and appear to terminate at once on the external surface of the enlarged part of the semi-circular tubes described above. They do not appear to pass through these tubes so as to get on the inside, as is supposed to be the case in quadrupeds; I should therefore very much suspect, that the lining of the tubes in quadrupeds is not nerve, but a kind of internal periosteum." Phil. Trans. for 1782, p. 383. and Anim. Œcon. p. 84. Hunter's idea does not appear to have been confirmed.

[3] It would be impossible to describe the complicated ramifications of the fifth pair of nerves in this place. I shall only remark concerning it, that whereas the specific sensibilities of the organs of sense are generally connected with a specific nerve, the fifth pair gives them their general sensibility,

SECT. 2. *Acquired Perceptions of the Ear.*

The acquired perceptions of the ear are less numerous and less distinct than those of the eye. This is partly in consequence of the vibrations which constitute sound being less completely under our control, and partly from their physical effects being less understood than those of light. There seems, however, to be sufficient cause to believe, that blind persons judge of the distance, magnitude, and position of objects entirely by experience and association, and it is often very remarkable to observe what precision they acquire in this respect, without any assistance from the sight, the sense which, under ordinary circumstances, we almost exclusively employ on such occasions[1].

With respect to what may be termed audible ideas of distance, they are gained by comparing the strength of the impression with a previous knowledge of the space which exists between the ear and the sounding body. The audible ideas of magnitude are principally concerned in acquiring a knowledge of the size of apartments, which blind persons are often able to estimate with considerable correctness[2]. This knowledge they acquire by attending to the force of the reverberation which is produced from the walls, and it depends upon their comparing the effect thus produced upon the ear in the case under consideration, with their previous experience in similar circumstances.

It was formerly supposed that we acquire our knowledge of the position of sounding bodies from the direction in which the vibrations enter the ear. But it was correctly remarked by Mr. Gough, that from the formation both of the external ear and of the meatus auditorius, before the undulations of the air can arrive at the tympanum, they must suffer many successive

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and serves to connect them with the other parts of the system. We have an elaborate account of the anatomy of this nerve in Meckel's treatise, "De Quinto Pare Nervorum Cerebri;" its origin is minutely described by Wrisberg, in his "Observationes Anatomicæ de Quinto Pare Nervorum Encephali." But on this subject, as well as on every thing that respects the anatomical structure of the basis of the brain and the connexion of its parts, it will be sufficient to consult the learned work of Sœmmering, "De Basi Encephali," with its engravings and numerous references.

[1] Magendie has given us an interesting account of a boy, who, after having been completely deaf until the age of nine, by means of an operation, acquired the perfect use of the ears. Among the most remarkable circumstances of the case, we may notice the difficulty which he had in obtaining a knowledge of the position of sounding bodies, and still more of imitating articulate sounds; it was only after many unsuccessful trials that he could accomplish this object, and even after an interval of some months, his powers in this respect were very limited; Journ. t. v. p. 223 et seq. See the art. "Deaf and Dumb," in Brewster's Encyc.

[2] A remarkable example of this kind is mentioned by Darwin as having occurred in the person of Fielding; Zoon. v. ii. p. 487. See the art. "Blind," in Brewster's Encyc., by Prof. Scott, for further information on this subject; also an essay by Venturi, on the knowledge of distance which we obtain from the ear; Magaz. Encyc. t. iii. p. 29 et seq.

reflections, which would entirely alter their original direction. He endeavoured to account for the effect by supposing that the bones of the skull, in the neighbourhood of the ear, are sensible to the vibrations of sound, and that we form our judgment from the portion of the head which immediately receives these vibrations, or which feels them the most powerfully, and still more, by comparing the effect produced upon the two sides of the head, or upon the parts surrounding the two ears[1]. Upon this principle we may explain the well known fact, that those persons who have lost the use of one of the ears, are less able to judge correctly of the position of sounding bodies, analogous to what we observe in those who have lost the sight of one of the eyes.

Audible impressions are of two kinds; those which produce the mere perception of sound, and those which give rise to what are termed musical tones. This difference is supposed to depend upon the vibrations in the latter case bearing a regular relation to each other, while simple sounds possess no regularity of this kind. When a body is uniform in its texture and its figure, the vibrations of its different parts will be isochronous, so that they will all coincide and leave distinct intervals between them, thus constituting musical tones; while, on the contrary, when the body is not uniform, the oscillations will be proportionally irregular, and we shall have mere sound produced, composed of an assemblage of vibrations bearing no relation to each other.

All kinds of elastic bodies, when of the proper form, are capable of producing musical tones. What we commonly employ for this purpose are metallic wires and membranes of various forms, constituting the basis of stringed instruments, and air confined in tubes, producing wind instruments. Sounds, both simple and musical, differ from each other as they are strong or weak; but musical sounds have, besides this, a specific difference, independent of their strength, by which they become what is styled acute or grave, high or low; a difference which is supposed to depend upon the rapidity of the vibrations, those which are the most rapid giving rise to the acute or high tones[2].

The mental emotions which are associated with certain musical tones are very powerful, and often evidently depend upon education, habit, or some accidental circumstance; but there can be no doubt that certain combinations of sound are naturally grateful to the ear, while others, on the contrary, are harsh and unpleasant. The combinations which are the most agreeable are those in which the number of vibrations that occur in a given time bear a certain geometrical relation to each other. These combinations of tones give rise to what we term harmony, while the succession of tones constitutes tune. The science of harmony, or that which treats of the relation which musical tones bear to each other, forms a very important department of mechanical philosophy, while the proper adjustment of the different tones to each other constitutes one of the most refined and elegant of the polite arts. Both of these may be considered as ultimately depending upon the same principle, and capacity in the ear, or in the part of the sensorium connected with it, for perceiving the relation between musical tones. This would seem to be a distinct faculty from the mere sensibility to sound. Perhaps, in some degree at least, it may be regarded rather as an intellectual than as a physical quality, depending more upon certain combinations of ideas, than upon any mechanical state of the organ. We frequently observe persons, whose power of hearing is very acute, and who are yet totally devoid of what is termed a musical ear; while, on the contrary, deaf persons often possess the most delicate perception of musical tones.

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[1] Manchester Mem. v. v. p. 622 et seq.

[2] See the art. "Acoustics," in Brewster's Encyc. by Campbell, for an account of the nature of musical tones, and the difference between stringed and wind instruments. See also the elaborate and learned memoir on the human voice, and its relation to musical instruments by Savart, in Magendie's Journ. t. v. p. 369 et seq.

We are quite ignorant on what this faculty depends, or what it is that constitutes a musical ear. It has been conjectured that the perception of musical sounds may depend upon the vibrations of some particular parts connected with the tympanum, as for example, upon the cochlea, or some of the bony canals, because we find that these parts are the most elaborately formed in certain animals, which have been thought to possess a delicate perception of musical tones, whereas in animals that have acute perceptions of sound generally, but no power of discriminating between different tones, although the organ of hearing is capacious, these parts appear to be less developed. But it is doubtful whether the facts are sufficiently ascertained to support the hypothesis, and we have no account of the comparative state of the ears in different individuals of the same species, which can assist us in the inquiry[1].

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[1] Dr. Wollaston has made us acquainted with a series of curious facts respecting a peculiarity in certain ears, which seem to have no defect in their general capacity of receiving sound, or in the perception of musical sounds in particular, which yet are insensible to very acute sounds. This insensibility commences when the vibrations have arrived at a certain degree of rapidity, beyond which all sounds are inaudible to the ears so constituted. Thus we are informed that certain individuals are incapable of hearing the chirp of the grasshopper, others the cry of the bat, and one case is mentioned where the note of the sparrow was not heard; Phil. Trans. for 1820, p. 306 et seq. With respect to the limit to the perception of these acute sounds, the author remarks: "The chirping of the sparrow will vary somewhat in its pitch, but seems to be about four octaves above E in the middle of the piano-forte. The note of the bat may be stated at a full octave higher than the sparrow, and I believe that some insects may reach as far as one octave more; for these are sounds decidedly higher than that of a small pipe one-fourth of an inch in length, which cannot be far from six octaves above the

middle E. But since this pipe is at the limit of my own hearing, I cannot judge how much the note to which I allude might exceed it in acuteness, as my knowledge of the existence of this sound is derived wholly from some young friends who were present, and heard a chirping, when I was not aware of any sound. I suppose it to have been the cry of some species of gryllus, and I imagine it to differ from the gryllus campestris, because I have often heard the cry of that insect perfectly." p. 312. On this subject, I may again refer to Savart's valuable memoir, in the 5th vol. of Magendie's Journal.

CHAPTER XV.

OF TOUCH, TASTE, AND SMELL.

THE senses of sight and hearing bear a considerable analogy to each other, with respect to the mode in which they receive the impressions of their exciting causes. The agents which affect the retina and the expansion of the portio mollis, are of a totally different nature from the primary causes of the impressions, and are connected with them solely by means of experience and association. But in the other external senses, those of touch, taste and smell, the immediate exciting cause is either the body itself, or a certain portion of it which is directly applied to the sensitive organ. In gaining our conceptions of the form, taste, and odour of bodies, although we are assisted by comparing the different classes of perceptions with each other, yet, if the organ be in a sound state, we are not liable to the same kind of errors as in those of sight and hearing. We accordingly find, that one great use of touch is to correct the impressions that we derive from the other senses, and that when we have it in our power to apply the organ of touch to a body under examination, it is had recourse to, as the mode which enables us to acquire the most satisfactory information respecting it[1].

SECT. 1. *Of Touch.*

In popular language, the sense of touch is supposed to receive every impression which is not derived from the other four senses, sight, hearing, smell, or taste; an inaccuracy which probably arose from the term feeling being frequently used as synonymous with touch. Strictly speaking, however, the word touch should be applied to the sense of resistance alone, a sensation which is sufficiently specific, and may be easily distinguished from all other sensations[2].

The sense of touch, even when restricted to the idea of resistance, is very much more extended in its seat than any of the other senses. Every portion of the external surface, and perhaps even of the internal surfaces, appears to be capable of receiving the impression of resistance; yet there are certain parts which possess a peculiar degree of delicacy in this respect. In man this is the case with the points of the fingers, while in some of the inferior animals the sensibility to touch appears to reside principally in the lips and the tongue. The greater sensibility of these parts is to be attributed, in some measure, to the delicacy of their cutis, to the quantity of blood distributed over their surface, and to their being plentifully supplied with nerves; but we may presume that it depends in part upon the effect of habit, because we find, that certain individuals, who are without hands, have acquired a nearly equal degree of sensibility in the toes, or some other parts of the body [1].

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[1] Blumenbach, Instit. Physiol. § 230, p. 133.

[2] Haller notices the distinction, but only in a general way; El. Phys. xii. 1. 1, 2. Blumenbach, however, is disposed to employ the word in its more extended sense; Inst. Physiol. § 227..9, p. 133. Magendie's distinction of "tact" and "toucher," may be considered as equivalent to the one that I have proposed; El. Phys. t. i. p. 146.

It is frequently asserted, that the touch is the most certain of all the senses, and the one which corrects the errors into which we are led by the others, especially by the sight and the hearing [2]. This is, to a certain extent, true; for the organ of touch, when it acts, is brought into contact with the body producing the impression, whereas, in the case of the eyes and the ears, we only receive the impression of something that is emitted from the body, or of some medium that has been affected by it. Yet, if the perceptions gained by the sense of touch are little subject to error, it must be confessed, on the other hand, that they are very limited, and that our knowledge would be confined within a very narrow range, were we to acquire no ideas but through this sense[1].

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[1] Blumenbach, § 231, 2. p. 134. See Malpighi, in Manget, Bibl. Anat. t. i. p. 26 et seq. Grew supposes that he had discovered a peculiar organization in the lines with which the fingers and hands are marked, which was connected with the peculiar sensibility of these parts; but the observation has not been confirmed; Phil. Trans. for 1684, v. xiv. No. 159. p. 566, 7. fig. 1. On the anatomical structure of the parts of the surface which are endowed with the most delicate sensibility, see Riet, De Organo Tactus, in Haller, Disput. Anat. t. iv. p. 1 et seq.; Ruysch, Theat. Anat. 3. tab. 4. fig. 1. and 7. tab. 2. fig. 5, and Cowper, Anat. Corp. Hum. tab. 4. fig. 1..5, for the nervous papillæ of the lips, after removing the epidermis; Albinus, Anat. Acad. lib. i. tab. 1. fig. 1..11, for the integuments, and lib. 3. tab. 4, for the papillæ of the glans penis and mamma; and Caldani, Icones Anatom. tab. 90. We have an interesting series of experiments by Prof. Weber, of Leipsig, on the sensibility of the skin, by which is meant its power of receiving and conveying to the mind accurate perceptions of the mechanical impressions made upon it. As might be expected, the tip of the tongue and the ends of the fingers possess this power in the highest degree, and comparing these with the skin of the muscular parts, the proportion was found to be not less than 80 to 1. It appears that this sensibility of touch bears no relation to the capacity for feeling pain, and there seems to be no general principle to which it can be referred, except some peculiarity in the nerves of the part, depending probably both on the quantity of nervous matter, and on the mode of its distribution; Ed. Med. Journ. v. xl. p. 83 et seq. There is no sense, the organs of which appear to be so varied in the different classes of animals as that of the touch. In man it is principally seated in the ends of the fingers, in many of the mammalia it appears to reside in the tip of the tongue, in some of the carnivora it is supposed to be seated, in part at least, in the whiskers; in fish, in the peculiar filamentous appendages which are attached to the mouth, while in many of the insect tribes the antennæ would seem to be the appropriate organ. Dr. Graves has given us a valuable analysis of Weber's experiments in Jameson's Journal, v. 21. p. 67 et seq.

[2] This was a favourite opinion of Buffon, and one on which he expatiated with much eloquence; Nat. Hist. v. iii. p. 294..301.

The relation which the touch bears to the other senses, especially to that of sight, is a point that has given rise to much discussion among physiologists and metaphysicians. It has been asked, are our ideas of distance and extension gained by the touch alone, or by the touch associated with the sight? It has likewise been asked, how far we are able to acquire an idea of figure, as distinct from mere extension, by the sight alone, without the aid of the touch[2]? Notwithstanding the number of celebrated men who have investigated these questions, we are perhaps still unable to give more than a conjectural answer to them; we may, however, venture to assert that the idea of distance and of figure which could be gained by the sight alone must be very imperfect, and frequently altogether incorrect.

We have an ingenious speculation of Condillac, which was intended to elucidate this subject. He supposes a being to be formed resembling the human, in its organs and physical powers, but in the first instance without a nervous system. He then conceives it to be endowed with the single sense of touch, and examines what ideas would be conveyed to it by the surrounding objects, through the medium of tangible impressions alone. He afterwards gives it the other senses in succession, and in each case inquires what would be the result of the successive additions, and how the perceptions would be gradually brought into that state in which they are possessed by a perfectly organized being[3].

In blind persons the sense of touch supplies many of the impressions which, under ordinary circumstances, are produced by the sight. They are, however, very materially aided by the sense of hearing, more especially in what regards their communication with their fellow-creatures; this sense, through the intervention of speech, being the one which we employ in the common intercourses of society. But it occasionally happens, that we meet with persons who are deprived of the senses both of sight and of hearing, and yet, as far as we can judge, possess the full power of receiving the perceptions of external objects, were they provided with the necessary instruments for acquiring the impressions of them.

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[1] The form and structure of the human hand enables us to acquire a degree of accuracy in our tangible ideas, much superior to that of any other animal, but this depends merely upon the mechanical convenience of the part. There is a well known case of a female in the country, entirely without either upper or lower extremities, who has supplied the defect of hands by the tongue and lips, combined with the motions of the muscles of the neck.

[2] The celebrated problem which was proposed to Locke by Molyneux refers to this point; whether a blind man, suddenly restored to sight, could distinguish between a globe and a cube which were placed before him, by his sight alone; Molyneux supposes that he could not, and Locke assents to the opinion; Essay, book 2. ch. 9. § 8. Smith agrees with Locke; Optics, § 132; but Jurin offers some very powerful considerations in favour of the opposite opinion; Remarks on Smith, § 161..170. See also the observations of Winteringham, in his Exper. Inq. p. 259.

[3] Traité des Sensations, part. 1.

An extremely interesting case of this description lately occurred in Scotland, where a man was born blind and deaf, yet whose mental powers appear to have been naturally perfect. He was fortunately surrounded by kind and intelligent relatives, so as to enjoy every advantage of which his bodily situation admitted, for obtaining information by means of the other external senses, and we are indebted to Mr. Wardrop and to Prof. Stewart[1], for a minute account of the state of his understanding, and of the portion of knowledge which he was enabled to acquire. His conceptions of external objects, most of what may be termed his general or abstract ideas, were principally derived from the touch, and it is not a little curious to observe, with what perseverance he pursued his investigation of the various objects that were presented to him. He appeared to possess a peculiar delicacy of touch, and still more of smell; and by means of these senses alone he acquired a knowledge of the nature and presence of surrounding bodies, which would previously have been thought impossible. The general result of the observations that were made upon this person warrants the conclusion of Locke and of Berkeley, that our acquired perceptions are originally derived from impressions made on the external senses, and that when we abstract or generalize our ideas, we do it by comparing and combining the knowledge we have derived from this source[2].

SECT. 2. *Senses of Smell and of Taste.*

The senses of smell and of taste are analogous to that of touch in the circumstance of the body itself, which produces the impression, being immediately applied to the organ; and indeed they may, to a certain extent, be regarded as modifications of the latter sense. In the human species they are subservient rather to our gratification and enjoyment than to our existence, but in the inferior animals they seem to be the means by which they receive many of those instinctive ideas that are immediately necessary for the support of the individual and the continuance of the species.

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[1] We have a very ample account of Mr. Wardrop's Memoir in Edin. Med. Journ. v. ix. p. 473 et seq. Prof. Stewart's narrative of the case, accompanied by a variety of details from Dr. Gordon and others, is contained in the Edin. Phil. Trans. v. vii. p. 1 et seq.; the same is inserted in the 3d vol. of his "Elements," p. 401 et seq. See also a paper by Dr. Gordon, Ed. Trans. v. iii. p. 129 et seq. and remarks in Ed. Rev. v. xx. p. 462 et seq.

[2] Dr. Hibbert has given us an interesting account of another case of an adult who was born blind and deaf; but, in addition to these privations, there was reason to suppose that he was naturally very defective in his intellectual powers, while no attempts had been made to obviate or remove this deficiency; Edin. Phil. Journ. v. i. p. 171 et seq.

The immediate organ of smell appears to be the mucous membrane, named Schneiderian, from the anatomist who first accurately described it[1], which lines the internal parts of the nostrils, and more particularly the turbinated bones. The same membrane, although differing a little in its structure, is continued over all the parts which communicate with the nostrils, especially the various cavities or sinuses that are in the contiguous bones. We may presume that the sense of smell is exercised, to a certain extent, by the whole of this membrane, although some parts of it possess a more delicate sensibility.

The Schneiderian membrane is supplied very plentifully with blood vessels and with nerves; the latter, as is always the case with the organs of sense, being derived from at least two distinct sources, from the first pair, termed the olfactory, and from certain branches of the fifth pair[2]. The olfactory are supposed to be the nerves which are the proper seat of the sense of smell[3], while the branches of the 5th pair serve for the general purposes of the nervous influence[4]. These branches of the 5th pair form a part of the system of nerves, which has been ingeniously developed by Sir C. Bell, under the title of respiratory, and consequently, we may presume, that it is upon these that the irritation is produced which excites sneezing. There is a peculiarity in the termination of the olfactory nerve, that it does not, like the optic and the auditory nerves, terminate in a filamentous texture, but is reduced to a pulpy substance, which is, as it were, incorporated with the mucous membrane for which it is destined.

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[1] See his treatise, De Osse Cribriformi et Sensu ac Organo Odoratus; also, De Catarrhis, lib. 1. sect. 2. ch. 1. p. 149 et seq.; Haller, El. Phys. xiv. 1. 13. We have views of the anatomical relations of the organ of smell in Haller, Icones Anat. fas. 4. tab. 2. Tabulæ Narium internarum; in Santorini, tab. 4. exhibiting the interior of the nose, mouth, and pharynx; in Scarpa, Anat. Annot. lib. 2; in Sœmmering, Icon. Organ. Hum. Olfactus; inserted in Cloquet, Man. pl. 136, 7; in Caldani, Icon. Anat. pl. 101, 2; and in Cloquet, Anat. pl. 121..3.

[2] Haller, El. Phys. xiv. 1. 18, 19. and xiv. 3, 4. We have an account of the structure and disposition of the first pair of nerves by Hunter, Anim. Œcon. p. 263..5; by Scarpa, Anat. Annot. pl. 1, 2. to the second book; by Sœmmering, De Basi Encephali, § 21..30. and Icon. Org. Olfactus, tab. 2. fig. 3, 4, and tab. 3. fig. 1; by Vicq-d'Azyr, Planches, No. 16..20, 27; and by Metzger, Nervorum primi Paris Historia.

[3] Many of the earlier anatomists did not admit this body to be a nerve, but supposed it to be an excretory organ appropriated to the brain; this hypothesis is defended by Diemerbroek, Anat. lib. 3. c. 8. p. 603; his work was published in 1672. Vieussens, who published his "Neurographia" in 1716, supposes it to be the proper nerve of smell; lib. 3. c. 2. p. 163, 4.

[4] Magendie has, however, attempted to demonstrate by experiment, that this appropriation of the office of these nerves is incorrect; he conceives that the sense of smell does not depend upon the olfactory nerves, as they have been usually denominated, but upon the branches of the fifth pair, that are distributed over the pituitary membrane of the nostril. As these branches, at the same time, seem to impart to this membrane its general sensitive power, it is not evident, upon this view of the subject what is the use of the first pair of nerves; El. Physiol. t. i. p. 132..4; Journ. t. iv. p. 170 et seq. p. 302 et seq. and t. v. p. 21 et seq. See also Desmoulins, Anat. Sys. Nerv.; and Lond. Med. Journ. v. lii. p. 82. We have, on the other hand, a number of judicious observations upon Magendie's doctrine by Eschricht, the object of which is to prove, that the first and fifth pair of nerves bear the same relation to the organ of smell, that the optic and the auditory nerves bear to the branches of the fifth, as respectively distributed to the eye and

It has been remarked that the organ of smell is very imperfectly developed at birth, and it would appear, that the sensations connected with it are proportionally feeble and indistinct. It is also observed, that in the inferior animals, those in which the organ is of the greatest size and the most elaborate structure, have the sense of smell the most acute, and we are informed, that the same may be observed in the different varieties of the human species, especially in certain tribes among the Africans and the aboriginal Americans[1].

The sense of taste is seated in the tongue and fauces, and is probably extended even to the gullet, but the most delicate sensibility appears to reside in the tongue, and more especially in the extreme part of it. The tongue, like other acutely sensitive organs, is plentifully provided with nervous filaments, and with blood-vessels, and it has a peculiar papillary structure, which we presume is adapted for receiving the impressions of sapid bodies[2]. Like the other organs of sense, its nerves are

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the ear; Magendie's Journ. v. vi. p. 339 et seq. Dumeril has endeavoured to prove, that fish have no proper organ of smell, but that the part which is usually supposed to be for the purpose of receiving odours, constitutes their organ of taste. The author rests his hypothesis partly upon the assumption, that odours, being essentially of a volatile or gaseous nature, cannot exist in fluids, and partly upon the anatomical structure of the mouth in fishes, and of the nerves which are sent to it. The correctness of the first position may, I think, be reasonably doubted, and with respect to the distribution of the nerves of the part, it may be fairly objected, that their connexions and relations to each other, are too intricate and numerous to warrant us in drawing conclusions of so much importance as those which Dumeril attempts to establish. We have a translation of his paper in Nicholson's Journ. v. xxix. p. 344 et seq. taken from Mag. Encyc. Sept. 1807.

[1] Blumenbach, in his first Decas, remarks upon the ninth skull, that of an American Indian, " Olfactus officina amplissima," p. 24. For many valuable remarks on the sense of smell, see the art. " Odorat," by Dr. M. Edwards, Dict. Class. d'Hist. Nat.; it contains an interesting account of the comparative physiology of this sense. See also Adelon, Physiol. t. i. p. 347; and the 1st chapter of Dr. Harwood's Compar. Anat. We have a remarkable illustration of the degree of acuteness which any one of the senses may acquire by the frequent exercise of them in the case of James Mitchell, which was referred to above, who, on various occasions, substituted the sense of smell for those of sight and hearing, of which he was deprived.

[2] Sœmmering, Icon. Organ. Gust. Hum.; for the papillary structure of the tongue, see Morgagni, Advers. Anatom. No. 1. tab. 1. and Ruysch, Thes. Anatom. No. 1. tab. 4. fig. 6. p. 35, 6. Op. t. ii.; see also Cloquet, pl. 120, taken from Gerdy.

derived from different sources, according to the purposes which they are intended to serve. Those that are destined to receive the impressions of taste, are derived from the 5th pair, while the nerves that serve for the motion of the part, or for the general purposes of the nervous influence, proceed from the 8th and 9th pairs[1].

The senses of smell and of taste are in many respects very intimately connected with each other. They are both of them excited in the same manner by the application of odorous and of sapid particles respectively to the nerves which are appropriated to receive these impressions. In a variety of instances the same substances excite the impressions both of taste and of smell, and although these impressions are generally supposed to be sufficiently specific and distinct from each other, yet a close attention to their nature, and to the manner in which they are produced, leads us to conclude, that in our ordinary conceptions, we frequently confound them with each other. This appears to be more particularly the case with the taste; many of the impressions which are referred to the tongue, and are supposed to result from the application of sapid particles to it, being really produced by odorous effluvia affecting the nerves of the nose[1]. Some physiologists have carried the idea so far as to deny altogether the separate existence of the impressions of taste, conceiving that the tongue and palate are only sensible to resistance, and that there is nothing specific in the nature of their action. This opinion has been supported by experiments, which, it must be acknowledged, reduce the operation of the nerves of taste into much narrower limits than had been formerly assigned to them. Still, however, I think there can be little doubt that the nerves of the tongue are capable of receiving impressions, which cannot be referred either to those of mere touch, or to any other of the primary sources of our perceptions, and that, whereas, in a great number of instances, we have ideas of smell that are unaccompanied by those of taste, so we have certain ideas of taste that are unconnected with those of smell[2].

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[1] Monro, on the Nervous System, tab. 26; Blumenbach, Instit. Physiol. § 238. p. 137; Bell's Anat. vol. iii. p. 161, 2; Meckel, de Quinto Pare Nerv. Cereb. § 100, 1. This affords an exception to the remark in p. 721, respecting the office of the fifth pair. We have an elaborate description of the ninth pair of nerves by Boehmer, in which he gives a full account of its distribution, its connexion with the other nerves, and the various opinions that had been entertained respecting its use; Ludwig, Scrip. Neur. t. i. p. 279 et seq. cum tab. See also Caldani, Icon. Anat. pl. 103, 4. where many of the figures are original. For the various opinions that have been entertained respecting the nerves of taste, I may refer to Adelon, Physiol. t. i. p. 206 et seq. Mr. Owen observes, that the sense of taste in birds is very imperfect, and that no branch of the 5th is sent to the tongue; it is principally a prehensile organ or is used in deglutition; Cyc. of Anat. v. i. p. 311. The experiments of Mr. Audubon seem to prove that birds of prey are chiefly guided by their smell. See the remarks of Blainville on the organ of taste, in the 3d chapter of his work de l'Organization des Animaux. Prof. Panizza has lately made the specific office of the different nerves that are sent to the tongue the subject of a series of experiments, of which an account is given in the Edin. Med. Journ. for Jan. last, p. 78..87. They were performed principally on dogs and consisted in dividing separately the hypoglossal, the lingual branch of the 5th, and the glosso-pharyngeal nerves, and noticing the different effects that were produced. The result is stated to have been, that by the division of the first, the tongue lost the power of voluntary motion, of the 2d, of general sensibility, and by the division of the last, of the specific sense of taste. In the following number of the some journal, p. 426 et seq., we have a communication from Mr. Broughton, in which he informs us, that his experiments, as far as he had pursued them, agreed in their results with those of Panizza, and he generally assents to the Professor's conclusions; he observes, p. 431, "that the problem of the medium of taste is now solved." The glosso-pharyngeal, in its functions, is supposed to be analogous to the optic nerve. Notwithstanding, however, the apparent accuracy of the experiments of Prof. Panizza, and the sanction which they have received from Mr. Broughton, they have been controverted, and as I conceive on good grounds, by Mr. Mayo. He instituted a similar series of experiments, and by noticing certain circumstances, which were not attended to by the Professor, as well as from various anatomical and pathological considerations, he concludes, in conformity with the opinion which he had formerly expressed on this subject, Comment. p. 2. p. 10..2, that the lingual branch of the 5th is the proper nerve of taste, but that it also possesses a degree of general sensibility, that the 9th, or hypoglossal, is the nerve of voluntary motion, while the glosso-pharyngeal is in part a nerve of voluntary motion and in part of general sensibility, but not of taste. The writer of the review of Rudolphi's physiology, in the Amer. Journ. of Med. Sc. v. vii. p. 174, offers an opinion, which appears plausible, that although the lingual branch of the 5th is the more special nerve of taste, yet that the glosso-pharyngeal also contributes to this sense, because they both enter the papillæ, the glosso-pharyngeal being more particularly destined to the posterior, and the 5th to the anterior papillæ.

The senses of smell and of taste can scarcely be said to give rise to any of our acquired perceptions, but they present us with many remarkable examples of the effects of habit and association. We may presume that certain odours, and more especially certain flavours, are naturally more agreeable than others, but we find these original tastes to be so much modified by custom and by the various usages of society, that our acquired tastes generally become much more powerful and more difficult to eradicate, than those that are natural to us.

The effect that is produced upon the organs of smell and of taste by the frequent exercise of them is worthy of remark, the first being conspicuous among uncivilized people, and the latter

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We have some judicious observations on the relation which subsists between these senses in Caldani, Instit. Physiol. cap. 17. and 18. p. 159 et seq.

[2] Chevreul classes substances according as they affect both the smell and the taste, the smell only, the taste only, or produce the mere sensation of touch in the tongue; Magendie's Journ. t. iv. p. 127 et seq. and Mém. du Museum, t. x. p. 439 et seq. Our conceptions of taste are very indistinct, and the terms which we employ to discriminate them are vague and general. Grew made an ingenious attempt to define, with more accuracy, the various flavours of vegetables; Anat. of Plants, § 29. p. 13, 4; but the subject has been since scarcely attended to. Bellini's treatise, De Organo Gustus, contains an account of the doctrines of the mechanical physiologists respecting the mode in which sapid bodies affect the tongue.

in the most highly refined states of society. The fact is the more worthy of our notice, as in this case there is nothing in the mechanism of the organs, which can render us more dexterous in the use of them, so that this greater acuteness of the sense must be entirely acquired by more minutely attending to our perceptions. By this means we may either actually increase the force of the perceptions, or without increasing their force, we may be rendered more sensible to its operation.

SECT. 3. *Sensations of Heat and Cold, &c.*

I have already remarked that, besides the organs of what are usually termed the five external senses, there are other parts of the body capable of receiving impressions, which are of a specific nature, and which give rise to perceptions that are specifically distinct from those which have been described above. Some of the most remarkable of these are the sensations of heat and cold, those which attend muscular motion, those of hunger and thirst, and those of the sexual organs.

The sensations of heat and cold[1] are referred, in a great measure, to the surface of the body, and would seem naturally to be felt in nearly an equal degree by every part of it. A considerable difference is, however, produced in certain portions of the skin, from their being habitually more or less exposed to changes of temperature, and there may probably be an original difference in consequence of the number of nerves that are sent to a part, the texture of its integuments, or the quantity of blood which is transmitted to it. Whatever hypothesis we may adopt respecting the nature of heat, or its relation to cold, we find, as a matter of fact, that any considerable elevation or depression of temperature causes the sensation in question, and that, in a certain degree, the force of the sensation depends upon the quantity of heat which has been added to, or removed from the body. But we have sufficient proof that the sensations of heat and cold are not in exact proportion to the degrees of heat and cold that are applied, but that they rather depend upon the difference between the previous temperature of the body and that to which it is afterwards subjected.

There are, however, many states of the constitution in which the sensations of heat and cold do not correspond, either with the actual temperature, or the alteration which it experiences. In many morbid conditions of the body the sensations of heat and cold afford no indication of the real temperature, while the actual temperature is occasionally affected without affording us a corresponding change in our sensations. Mental impressions of various kinds have the effect of almost instantaneously increasing or diminishing both the actual temperature and the sensation of it, while we observe that the effects do not bear any exact proportion to each other. In the sensations of heat and cold, as in all the other classes of our sensations, we find that the influence of habit is very powerful, but it would appear that the associations formed with impressions of temperature are not very numerous or very important.

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[1] Magendie regards these as a species of what he calls "tact;" El. Phys. t. i. p. 150.

The seat of these sensations is the same with that of the touch, and like this, may be referred to the nerves that are distributed over the cutis[1]. Yet, it appears probable that the two sets of impressions are received by different nerves, because we find that the sensibility to resistance and to temperature are not in proportion to each other, either as existing in different individuals, or in the different parts of the same individual. We have occasional opportunities of observing this difference in certain morbid conditions of the body still more remarkably than in the healthy state. A case of this kind is related by Darwin[2], and they are not of very unfrequent occurrence, where the general sensibility of the surface is much impaired, while it still retains the impression of temperature, and the reverse, so as to show a want of correspondence between these two powers, which, it may be concluded, could only take place in consequence of their being exercised by different organs. It must, however, be acknowledged, on the other hand, that we are not able to observe any thing in the structure or disposition of the nerves that are dispersed over the surface, which would lead us to infer that their different parts were destined for the exercise of different functions.

A little reflection upon the nature of our own feelings will teach us that the sensations which attend the motion of the limbs are of a specific kind, and are completely different from the sensations of touch strictly so called[3]. These are ultimately to be referred to the contraction of the muscles, this act, as it appears, producing certain impressions which are conveyed to the sensorium commune, and excite corresponding perceptions. These perceptions we learn to associate with peculiar contractions, and in this way we acquire a knowledge of the motions to which these contractions give rise. The acquired perceptions thus obtained we employ in most of the ordinary actions of life; in all the motions both of the limbs and of the trunk, we learn to proportion the effort to the degree of effect which we wish to produce, and thus we gain the habit of performing all the necessary actions, without being conscious of the mental process which is necessary for this purpose. The skill which certain individuals acquire in the mechanical part of music, as well as the great dexterity of rope-dancers, tumblers, and jugglers, depends in a great measure, upon their accurate perceptions of the contractions of the muscles [1], a faculty which in this, as in all other analogous cases, is to be referred, in some degree, to an original delicacy in the nerves of the part, and in some degree to custom and education.

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[1] Sir C. Bell remarks, that the muscles are comparatively insensible to the impressions of temperature; Phil. Trans. for 1826, p. 175.

[2] Zoonomia, v. i. p. 122. See also the account of Dr. Vieusseux's case, as drawn up by himself with much minuteness; although many of the symptoms are difficult to explain, they clearly point out the difference between the sensations of touch and of temperature; Med. Chir. Tr. v. ii. p. 221..3.

[3] Sir C. Bell's researches on the office of the different parts of the nervous system admirably illustrate the doctrine which is laid down in the text, by proving that there is a set of nerves appropriated to the sensitive faculty of the muscles, distinct from those which give them the power of motion; Phil. Trans. for 1826, p. 163 et seq.

We may presume that it is the perceptions which attend the contractions of the muscles which the blind, in many cases, substitute for visible perceptions, and even in those who can see, it is probable that these contractile perceptions, as they may be termed, are connected with visible perceptions, and aid us in judging of the form of bodies, and of many of their mechanical properties. The ideas of tangible extension are principally gained by moving the hand over the surface of the body, in which act the muscles connected with the elbow and shoulder are called into operation. And, upon the same principle, in ascertaining the thickness of a body, we employ the muscles that connect the thumb with the fingers, and thus in each case we receive sensations which are different from those of mere resistance, such as would be produced by the simple pressure of a hard body upon a portion of the cutis.

The sensations of hunger and thirst, and of the sexes, are styled, appetites, and consist in uneasy feelings, which seem to be produced by a peculiar condition of certain secreting organs, which uneasiness is removed by a change in the condition of these organs [2]. I have had occasion to make some remarks upon the immediate cause of hunger and thirst in the chapter on digestion; they probably depend upon the state of the mucous membrane which lines the stomach and fauces, which is removed by the reception of food and drink. With respect to its relation to the other sensations, every one who attends with accuracy to his own feelings, must be conscious that hunger no more resembles pressure upon the body, than sight resembles a mechanical force applied to the eye-ball.

The associations connected with the peculiar sensations of the stomach are of considerable importance in the relations of life. In all nations, as well the most barbarous as the most civilized, the usual intercourses of society are accompanied by the taking of food, and it may be conceived that the enjoyment derived from this source forms a permanent connexion with the benevolent feelings and promotes their operation. None of the senses exhibit in a more remarkable degree the effect of habit; and there can be little doubt that the great majority of individuals are influenced much more by this principle, than by the actual calls of hunger, in the reception of their food, both as to the times of taking it and the nature of the food employed. The accidental associations of the stomach, as well as those of the palate, are often very remarkable, and these are so connected together, that we often experience the sensation of nausea from bodies, which possess no property that can excite vomiting, except that it has a taste similar to some other body which has an emetic effect. We are likewise well acquainted with numerous instances, where vomiting is produced by mere mental affections, unaccompanied by any sensible impression.

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[1] Sir C. Bell, in his remarks on what he terms "the nervous circle which connects the voluntary muscles with the brain," Phil. Trans. for 1826, p. 163 et seq., points out the necessity of perceptive nerves in the voluntary muscles.

[2] We may consider the appetites as perceptions, necessarily connected with the internal organization of the body, and impelling to certain actions, independent of external circumstances; see remarks in Harris's Philos. Arrang. p. 417.

The sexual feelings seem to be immediately produced by the presence of the seminal secretion in the vesiculæ seminales, or some of the contiguous parts, and are removed by its discharge. The sensations which depend upon the appetites are some of the most violent to which the animal frame is subject, and exercise a very powerful influence over the actions of the individual. They give rise to many of our strongest and most durable associations, and are the immediate origin of the most important connexions of social life. They exist with very different degrees of force in different individuals, partly, as it would seem, from a difference in the original constitution, and partly, from the influence of custom, and external circumstances.

SECT. 4. *General Remarks on the Perceptions of Impressions.*

When we take a general view of the different classes of the perceptions of impressions, and compare them with each other, notwithstanding their diversity, we perceive many points of resemblance and analogy. As I have already remarked, they have each their specific cause, although the distinct nature of the cause is not, in all cases, equally apparent. The emanations from luminous bodies, the undulations of the air, the effluvia of odorous substances, and the particles of sapid food, are sufficiently specific, and have each of them an organ for their specific operation. This, however, is not so obviously the case with the sense of touch, either as to its cause, or the organ by which it is exercised; the cause being merely the sense of resistance, by whatever body produced, while the nerves which exercise this sense are diffused over the whole surface, so as to lead to the conclusion, that the skin may be the proper organ of touch.

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The sensations of temperature have an obvious external cause of a sufficiently specific nature, but we are ignorant of their specific organ, and it remains a question that we are unable to decide, whether there are certain nerves especially appropriated to the impressions of temperature. The sensations of hunger and thirst, and of the sexes, are to be regarded in a different light from those of temperature; we have here an appropriate organ, but it may be doubted whether we have a specific agent, unless we consider the secretions of the parts as such. In the sensations which attend muscular contraction, the will may be considered the agent, while the whole of the muscular system and the nerves of voluntary motion may be regarded as the appropriate instruments.

We have now been considering the nature of sensation, from whatever cause excited, as an effect produced in some part of the body by a certain agent, this effect being transmitted by the nerves of the part to the brain, where it constitutes a perception. According to this view of the subject, sensation would appear to exist in the nerves, and perception in the brain, or at least these are the agents by which the faculties are respectively exercised[1]. But although this may appear the most consistent view of the subject, still it leaves some points unexplained; one of these respects the manner in which we acquire our perceptions of simple pleasure and pain, and refer them to their proper seat.

These sensations must be regarded as distinct from any that have yet fallen under our consideration, and they would appear to be excited by a different process. In every part of the body which is provided with nerves, we possess a degree of feeling which seems to be independent of external impression; and all such parts, from the operation of various circumstances, both internal and external, become the seat of pain. It would appear that the immediate cause of pain may be resolved into whatever tends to derange the structure or action of the part, every kind of mechanical injury, excessive stimulation, or a morbid condition of any of its constituents.

It may be questioned, whether the physical feelings of pleasure are to be considered, like those of pain, as capable of being received by all the sensitive parts of the body, or whether they are not rather of a more specific nature, and confined to particular organs and structures. In many cases our feelings of pleasure are intimately connected with mental emotions; the mere removal of pain is not unfrequently regarded as a positive enjoyment, and it may be remarked, that where we have distinct perceptions of physical pleasure, they may be generally referred to certain organs connected with the appetites or the external senses.

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[1] This is substantially the doctrine of Parry, Pathology, § 578, although it appears to differ from it, in consequence of the distinction which I have thought it necessary to make between sensation and perception.

The perceptions of pain, like those that are derived from the impressions of external objects, are seated in the sensorium commune. This is proved, both by those cases in which the feelings of pain are interrupted by dividing or compressing the nerve, and by those in which the irritation of a nerve produces a conception of pain in a part which no longer exists. We, however, always refer the pain to the part of the body upon which the injury has been inflicted. With respect to pains in the external parts of the body, we have no difficulty in explaining the means by which we refer them to their proper seat, but it is not so easy to account for the way in which it is accomplished with regard to the internal parts, where the seat of the injury is concealed from our view. Do we in these cases judge of the seat of the pain by association and experience, or is there any thing in the structure of the organs which enables us, in the first instance, to form our judgment?

To a certain extent, the knowledge which we possess may be referred to the effect of habit and association. When we receive an impression on any part of the body, from some obvious external cause, which produces a certain sensation in the part, if we again feel the same sensation, we refer it to the same seat. This may apply to all the external parts of the body; and, with respect to the organs of sense, we may conceive that we shall be guided by the specific nature of the agent or of the effect. But this throws no light upon the means by which we are enabled to distinguish the seat of local impressions upon internal organs, especially when these arise from constitutional causes. For example, when disease occurs in the stomach, we have a corresponding sensation in the part, the effect being the direct result of local action, unconnected with any mental operation. In these cases, I am disposed to believe that the ganglia are concerned, and that we are to search for their use, in part at least, as constituting secondary centres of perception to which the action of the nerves is transmitted, and where the painful feeling is actually experienced[1].

Although the express object of physiology is to give an account of the system in its natural and healthy state, yet we are enabled to derive occasional assistance in our researches, from a knowledge of the diseased actions of the body, which constitutes the science of pathology, as we hence obtain a clearer conception of the modes in which the various parts of the animal frame are connected with each other. On this account it may be proper to notice an important feature in the animal œconomy, one which materially affects its operations and regulates its motions, which may be characterized by the term of self-adjustment. Exposed as the body is, at all times, to a variety of external agents, differing from each other both in their direct and their indirect effects, it was necessary that there should be some kind of corresponding change in the machine, to prevent the irregularities that might otherwise arise in its action. Now we shall find that the different vital functions are so adapted to each other, that their respective defects or excesses are compensated by the extraordinary action of some other function, which extraordinary action is the necessary result of the previous irregularity. It was from observing a number of examples of this kind that a pathological hypothesis was formed, which has long been a favourite doctrine of the schools of medicine, according to which all these trains of actions are referred to the operation of a specific principle, which has been named the vis medicatrix naturæ. But we may venture to affirm, that there is no foundation for this mode of reasoning, as these trains of actions can be referred to no one physical principle, and only agree in their final cause. They resemble each other only in exhibiting examples of the admirable order which pervades all parts of the universe, and which we observe as well in the inanimate, as in the animated parts of creation.

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[1] The different circumstances mentioned by Hartley, prop. 32, although they are all of them more or less employed in ascertaining the seat of internal pains, do not appear to me sufficient to account for the effect.

There is a peculiar operation, which is confined to the living body, which tends to preserve the machine in its proper order, and to regulate its motion, which has been styled re-action[1]. This more nearly approaches to what may be regarded as a specific principle, and may perhaps be considered as a mode of self-adjustment, which operates in all cases upon the same substances, and by the intervention of the same functions. If the action of a vital part be, by any cause, diminished, provided the defect be within certain limits only, the diminution of action becomes the immediate cause of an increase of power in the part, by which it is enabled to overcome the obstacle and restore the balance of the system. This capacity of re-action appears to reside both in the contractile and the sensitive parts, and is one of the most efficient means which is employed by the physician for restoring the functions to their state of healthy action, when this has, by any means, become deranged.

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[1] For a variety of valuable observations on this affection of the living system, I shall refer to the Pathology of the late Dr. Parry, a work containing many profound and sagacious remarks on the actions of the animal œconomy and their connexion with each other; § 581 et seq. These sections contain many curious illustrations of the self-adjusting operations. Some important observations on the power of re-action will be found in an essay of Dr. M. Hall's on the Effects of the Loss of Blood, in which the author offers a salutary caution to the inexperienced or unobserving practitioner against the improvident use of the lancet; Med. Chir. Trans. v. xiii. p. 121 et seq.

CHAPTER XVI.

OF THE CONNEXION OF THE PHYSICAL AND THE INTELLECTUAL FACULTIES.

I HAVE now taken a view of the first division of the nervous functions, the physico-sensitive; we must next proceed to consider those that I have termed simply sensitive, such as depend upon the action of the different parts of the nervous system, or of the different nervous functions, on each other. The purely intellectual functions do not properly fall within the province of the physiologist; yet they are frequently so much connected with the sensitive functions, that it will be proper to make some remarks upon the nature of the relation which they bear to each other, and of the influence which the mental powers exercise over those of the body[1].

Although I endeavoured in the last chapter to restrict the meaning of the word "touch" to what appears to be its correct sense, yet I took occasion to remark, that the exciting causes of all the external senses act by what may be regarded as a species of touch. The rays of light strike the retina of the eye; the undulations of the air, which constitute sound, communicate their motions to the interior of the ear; the sense of smell is produced by particles emitted from the odorous body, and carried by the air to the nose; while taste is immediately caused by the contact of the sapid body. The ultimate cause of perception is, however, unknown; nothing but experience could teach us that rays of light entering the eye would excite ideas of vision, or that undulations of air would impress the ear with ideas of sound, and we are unable to say why the reverse operations might not have taken place; yet we are sufficiently convinced of the fact by uniform experience. It is upon our ignorance of the connexion which exists between the cause of sensation and the effect produced, that Berkeley founded his celebrated doctrine of the non-existence of matter. The object of his hypothesis is to show, that the ordinary conception of material particles, which are endowed with a variety of properties, so as to give rise to the phenomena which constitute what we term the material world, is without proof; that the intervention of these particles to the production of perception is not necessary, and that all of which we have any actual knowledge is the existence of certain perceptions, which are the immediate operation of the Deity upon the sentient principle[1]. The full comprehension of this subject will probably always elude the grasp of the human faculties; but it does not fall within our province to discuss its merits. The object of physiology does not consist in refined speculations upon the essence of matter, but in observing the changes which are produced upon the living body, and the connexion which these changes bear to each other[2].

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[1] Gregory designates a knowledge of "the laws of union between the mind and the body, and the mutual influence which they have upon each other," as one of the necessary parts of the education of a physician, and it is evidently no less essential to the study of physiology; Duties of a Phys. p. 93. In connexion with the various topics that are treated of in this chapter, I will beg to refer my readers to the truly philosophical work of Dr. Abercrombie on the Intellectual Powers.

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[1] Berkeley's Principles of Human Knowledge, in which he argues against the commonly received opinion respecting the existence of matter, exhibits the same kind of acute and profound spirit of research which forms so distinguished a feature of the "Theory of Vision." But the subject, unlike the former, is not capable of an appeal to observation and experiment. He has, however, the merit of showing that the opinion which he combats is founded rather upon authority than upon any demonstration of its truth, and that if we admit it, we do so more from its being a convenient method of expressing our conceptions, than from a conviction of its correctness. We accordingly find, that all the attempts that have been made to refute Berkeley's hypothesis have consisted rather in an appeal to popular feeling than in strict philosophical deduction. Even Reid, after arguing with considerable acuteness and efficacy against the ideal hypothesis, deserts his vantage ground, and calls in the aid of certain principles, the existence of which is at least as questionable as that of any part of the system which he combats. It may be remarked, that the fundamental position upon which Berkeley's theory rests, is clearly stated in the writings of Malebranche, although it does not appear in what degree Berkeley derived his doctrine from this source. There is, however, a degree of similarity, as well in the character as in the writings of these philosophers, which renders it not improbable that Berkeley must have been acquainted with the "Search after Truth." The following observations of my intelligent friend, Dr. Roget, may be regarded as essentially coinciding with the doctrine of Berkeley. ". . . . in rigid strictness, we have no certain knowledge of the existence of any thing, save that of the sensations and ideas which are actually passing in our minds, and of which we are necessarily conscious." We infer the existence of external objects from "trains of impressions made upon our senses, of which impressions alone our knowledge can, in metaphysical strictness, be termed certain;" Bridgewater Treatise, p. 25, 6.

[2] It has always, I confess, appeared to me, that the dread with which it has been so much the habit of most of the modern metaphysicians to view the speculations of Berkeley, as leading to sceptical opinions with regard to the important topics of religion and morals, is entirely without foundation. I should as soon expect that a student of physiology would doubt whether he possessed the power of voluntary motion, because he was not acquainted with the mode in which the will acts upon the muscular fibre, as that a person of common understanding would have his faith and conduct affected by a perusal of Berkeley's works. A great portion therefore of the zeal which was manifested by Reid and his associates, in overthrowing the Berkeleian hypothesis, I regard as altogether unnecessary. At the same time, I think, that the principle which Reid has so clearly laid down, that the mind perceives the impressions of external objects themselves, and not the mere ideas of these impressions, is the obvious and direct expression of the fact.

Since the publication of Locke's Essay on the Human Understanding, it has been generally admitted, that our ideas are primarily derived from impressions made upon the senses [1]. This great philosopher [2] arranged the objects of thought in two great divisions, which he termed ideas of sensation and ideas of reflection. The first comprehending the knowledge which we immediately derive from the impressions of external objects; the second, the ideas which are produced by the operation of the mind upon the materials which it had already acquired, from the impressions made upon the senses. According to the nomenclature which has been employed in this work, the first may be termed perceptive ideas, the second intellectual ideas [3].

There is no question in the whole circle of the sciences, which has been more the subject of disputation, than what respects the connexion between the nervous system and the intellectual faculties. The doctrine which is the most commonly received is, that the mental powers, although connected with the brain, are ultimately to be referred to something independent of matter, while, on the contrary, certain philosophers of great acuteness have maintained that the mind, or that part of our frame which thinks and reasons, is necessarily connected with matter, can never exist but in conjunction with it, and that thought is no more than a property of a peculiar kind of material existence.

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[1] Prof. Stewart observes, that "impressions made on our senses by external objects, furnish the occasions on which the mind, by the laws of its constitution, is led to perceive the qualities of the external world, and to exert all the different modifications of thought of which it is capable." Elements, sect. 4. v. i. p. 99.

[2] I cannot mention the great name of Locke, without expressing my admiration of his writings and my veneration for his character. On this subject I shall indulge my readers with a quotation from the Quarterly Review, in which the merits of this philosopher are justly estimated. "There is scarcely one event of our lives, to which we look back with more lively recollection than to the period when we first read the essay upon the Human Understanding. It still remains in our memory, like an era in the history of our thoughts, from which we seem to date a sort of revolution in the very constitution of our knowledge. For it is not with a view to opinions that the writings of Locke are to be studied; but rather for the sake of witnessing the operation of his mind. There runs through his essay such a vein of precise and admirable reflection; he places his thoughts, right or wrong, in so clear a light; distinguishes and discards all trifling and merely verbal disputes; makes us understand ourselves so unequivocally, in the words which we employ, and in the subjects upon which we are meditating; that we know not any work that could be named in which the exercise of thinking may be so safely taken. This is never so strongly felt as when we come to his writings, fresh from the pages of some modern metaphysician. It is like changing the smoky atmosphere of a city for some pure and mountain air; the mind feels as if it were inhaling health from the very thoughts which it breathes; so much singleness and directness and integrity is there about all his opinions; such a contempt for paradox, such superiority to all the little tricks by which the common-place thoughts of common-place minds are trimmed out in the present day; and decked, if we may so express ourselves, in the mere cast off clothes of real learning and physiology." v. xxvi. p. 487. See also Enfield's History of Philosophy, vol. ii. p. 538, 9. Locke effected for metaphysical science what Haller did for physiology; he shewed us the limit of our knowledge, and pointed out the subjects which were the best suited for future inquiry.

[3] These terms nearly coincide with Hartley's ideas of sensation and intellectual ideas; On Man, Introd. v. i. p. ii.

It has unfortunately happened that this subject, which is one of great interest and curiosity, has seldom been viewed with that philosophical spirit, which should always direct our investigations, and by which alone we can expect to arrive at truth. It is admitted that certain errors may be so interwoven with our accustomed associations, on topics connected with morals and religion, as to render it doubtful, on some occasions, how far we ought to attempt their removal; but if this concession be made, on the one hand, it is incumbent upon us, on the other, not to inflame the prejudices which may exist on these topics, but to use our endeavours to correct all undue excitement, and thus to bring the mind into that tranquil state, which may enable it to receive truth without the fear of injury. In this spirit of candour and conciliation, I propose to make a few remarks upon this celebrated question.

It is argued by the materialist, that different kinds of matter possess different and specific kinds of properties; some bodies are hard, others elastic, some are endowed with what we term life, others are destitute of it. Living substances again have their distinguishing properties; the muscles are contractile, and the brain is sensitive. These properties of the muscles and of the brain are supposed to be necessarily attached to the respective substances, and to be incapable of existing without them; we can have no contraction without the muscular fibre, and no sensation without nervous matter. But besides sensation, a certain part of the nervous system, the brain, possesses another set of properties, peculiar to itself, which have been termed mental, and which collectively are supposed to constitute mind. Mind, therefore, is a faculty, or set of faculties, belonging to the brain, in the same way that contraction is the property of the muscle. It is argued, that we can have no conception of the existence of mind, except as attached to the brain, that it is derived from external impressions acting upon the brain, through the intervention of the organs of sense; that it is more or less perfect, according to the perfection of the brain and its appendages; that it is co-existent with this organ and partakes of its diseases[1].

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[1] As this question has been very clearly stated by Mr. Belsham, I shall insert the positions which he has advanced in favour of materialism, in what respects the physiological considerations. " When there is no organization, as far as our observation extends, there is no perception. Wherever such an organic structure as the brain exists, perception exists. Where this organization is imperfect, perception is imperfect. Where the organization is sound, vigorous, and healthy, perception is proportionably vigorous and clear. Where the organization is impaired, perception is enfeebled and obscured. And when the organization ceases, perception appears to cease.

To this it may be replied, that, although different bodies have different and specific properties, yet that these properties have all a relation to each other, so that their operation may be referred to certain general principles. When a muscle is stimulated to contraction, although we do not see how the stimulus acts upon the fibre, yet we can trace the effect of its contraction, and can observe the relation which it bears to the cause. We cannot explain how light produces vision, but we can point out the mechanical laws by which it operates, and we can experimentally prove that the impression on the retina is the primary cause of the sensation which is transmitted by the optic nerves to the sensorium commune. But no analogy of this kind can be detected with respect to mind. Even admitting the first step in the process to be beyond our comprehension, we might expect, as in the case of the muscle, and of the eye, to be able to follow up the succession of changes, and to shew their physical connexion with each other and with the brain. Nothing, however, of this kind can be accomplished[1]. We observe, indeed, a certain correspondence between the development and integrity of the brain, and the perfection of the intellect; but this is explained upon the principle, that whatever be the primary cause of mind, the brain is the organ by which it is manifested, and that, consequently, the proper condition of the organ is as essential, as that of the faculty by which it is directed.

The controversy, therefore, as far as it is not merely verbal, appears to turn principally upon the two following considerations: first, do the phenomena of mind bear such a resemblance to those which are usually ascribed to matter, as to justify us in placing them in the same class, and attributing them to the same organ? And, secondly, is the relation between the condition of the brain and the state of the intellect, such as to indicate that a necessary connexion exists between them.

Our reply to the first of these questions may be founded upon the same kind of reasoning which we employ in the subdivision of what are ordinarly termed the physical properties of matter. There are certain phenomena, which, from their analogy or resemblance, we class together under the denomination of me-

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Elements, p. 333. I feel reluctant to involve the great truths of theology in our physiological discussions, but, I think I may, without impropriety, repeat the remark of Dr. Prichard; "The whole universe displays the most striking proofs of the existence and operation of intellect or mind, in a state separate from organization, and under conditions which preclude all reference to organization." On the Nervous System, p. 52, 3. In reference to this discussion I may remark, that the first writer who made a clear and intelligible distinction between mind and matter, seems to have been Descartes; this distinction also forms a prominent feature in the writings of Boerhaave; see particularly his Instit. § 27.

[1] On this subject the remarks of Mr. William Belsham appear to me to be very much in point, I would almost say conclusive; Essays, No. 13. v. i. p. 312 et seq. See also Dr. Prichard's essay on the vital princ., sect. 5 and 6, where the question is discussed with the author's characteristic perspicuity and candour.

chanical, referring them all to the operation of an assumed general principle, which we style gravitation. We have, in like manner, another set of phenomena, which in consequence, as we suppose, of their differing essentially from the former, while they resemble each other, we refer to another assumed principle, chemical affinity. We conceive these to be different, because we cannot refer their operation to the same general laws, and because we do not perceive a change in the one to be accompanied by a corresponding change in the other. Upon the same grounds, therefore, that we conceive ourselves justified in supposing gravitation to be a property different from chemical affinity, I should maintain that mental are essentially dissimilar from physical phenomena, and that we must consequently reply to the proposed question in the negative[1].

It may be admitted, indeed, that the point in discussion is, in some measure, a question of degree, and one which as it cannot be subjected to the test of experiment, must always remain a matter of mere opinion. It may be further urged against the immaterialist, that his decision is founded upon our ignorance; that (to pursue the analogy of the subdivision of the physical powers) in the same way that we have discovered galvanism and magnetism to be modes of electricity, so future discoveries may assimilate mind to matter, demonstrate their necessary connexion with each other, and shew their points of analogy and resemblance. I will not presume to prescribe limits to our discoveries, either in physical or in metaphysical philosophy, but it may be fairly argued that, until such discoveries are made, or until we have some indication of their probability, we are impeding the progress of knowledge, by assuming a possible occurrence as the basis of an hypothesis, and that the cause of truth and knowledge is more effectually served, by arranging phenomena according to their actually ascertained differences, than by attempting to generalize possible or even imaginary resemblances.

The materialists have, however, seldom gone so far as to assert, that they could point out any real resemblance between the properties that are referred to matter and mind respectively, under the titles of physical and intellectual. But they allege, that the division rests entirely upon the definition which is applied to the former. The immaterialists, it is said, assume a series of properties, which they style physical, and which they restrict to what they denominate matter; but it is maintained that this division is altogether arbitrary, that we are entirely unacquainted with the actual properties of matter, and that, for any thing which can be proved to the contrary, thought is as much entitled to this appellation as hardness or extension. I admit the imperfection of our knowledge, and the narrow limits by which the powers of our comprehension are bounded; but it must be observed that, if we take this view of the subject, we resolve the whole into a mere verbal dispute, respecting the definition of terms; and it will then remain for us to inquire, which of these statements is the most in accordance with our usual conceptions on the subject, and which is the best calculated to afford us accurate ideas respecting the point under discussion[1].

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[1] It may be asserted, that all the physical changes to which bodies are subject, constituting what we term the properties of matter, may be ultimately resolved into certain modifications of attraction and repulsion; and, I conceive, it may with equal confidence be asserted, that the phenomena of attraction and repulsion are in no degree applicable to the operations of the mind. We should be less liable to erroneous conceptions on this subject if we were to follow the suggestion of Locke, book ii. ch. xiii. § 17..9. and, discarding all hypothetical language, were content to speak of the properties merely of matter and of mind, without considering them as attached to any substance or substratum, of which we are entirely ignorant. We have some excellent remarks "on the nature and extent of our knowledge of mind," by Dr. Abercrombie, p. 23..35. See also Dr. Alison's Physiol. p. 153.

The second question which was proposed proceeds more upon physiological considerations, and is one in which we are able to appeal more directly to the evidence of facts than in the former case. Now these facts, when duly considered, will, I conceive, lead us to a similar conclusion. When we inquire whether the relation between the condition of the brain and the state of the intellect is such as to indicate that a necessary connexion exists between them, we apply to the anatomist, and obtain from his investigations the only data which can enable us to form our decision.

[1] The most powerful argument of the modern materialists, is derived from our ignorance of the actual nature of matter, and the consequent impossibility of giving a correct definition of it. This is clearly expressed by Priestley, in his Disquisitions, sect. 1, 2; and in his Correspondence with Price; see particularly, On the Nature of Matter, p. 243..256. It is very forcibly urged by Cooper; Tracts, p. 266..286; and must be admitted to be of considerable weight. But, were we to act strictly upon this principle, we should abstain from all controversy upon the subject, and at once confess that it was one concerning which all discussion was vain and useless. What I contend for is, that, regarding this question in the same manner with other philosophical questions, and applying our terms with the same degree of accuracy, and with the same restrictions as in the other analogous cases, we cannot consider the properties of mind and of matter as belonging to the same class, or as referable to the same agent. Porterfield remarks, that "sense, perception, and thinking, cannot possibly be a mode of motion or figure, nor of any other property or power of matter;" On the Eye, v. ii. p. 215; and, unless we change, not only our technical definition of matter, but our conception of its nature, I conceive we must assent to the position. As connected with this topic, I shall recommend to my readers the perusal of the seventh section of Dr. Barclay's Inquiry, a work replete with information respecting the opinions that have been formed by others upon some of the most abstruse points in physiology, and enriched with many original remarks, indicative of a well furnished and capacious mind. In a volume comprising so many intricate discussions, it is impossible that any one who exercises the invaluable and inalienable right of private judgment should in all cases assent to the opinions of the writer; yet I feel myself called upon to acknowledge, not merely the high gratification, but the great advantage which I have derived from its careful and attentive perusal.

It would be in vain for me to attempt to give even an abstract of the great number of observations which have been made on this subject; but the result of the whole appears to me to shew, that the greatest disproportion exists between the derangements of the brain and those of the mental powers. After the most complete state of insanity, it is often difficult to detect the smallest alteration in the structure of the brain; whereas, on the contrary, the brain has not unfrequently been found very considerably disorganized, when no defect had been previously observed in the intellect [1].

It may indeed be said, that in both these cases we derive our conclusions from insufficient data; that there may, in the first instance, be some slight, although essential, change in the physical state of the brain, and that, in the second, notwithstanding its apparent derangement, still some certain portion may remain unchanged, which is the immediate organ of intellect. But this objection itself favours the opinion, that the relation which the mind bears to the brain is totally different from that which the other functions bear to their organs, and it is from this want of resemblance that I conceive myself warranted in drawing the inference, that mind is not a property of the brain, in the same way that contractility is a property of the muscle, or sensibility of the nerve. Beyond this point I do not presume to extend my inquiry; the nature of the connexion between matter and mind, or the mode in which they act upon each other, is at present completely unknown, nor do I think that we are in possession of any method of investigation by which it is probable that any additional light will be thrown upon the inquiry [2].

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[1] In proof of this position, it will be unnecessary to do more than quote the following passage from Pinel: "Il faut convenir cependant que dans d'autres cerveaux d'alienés on ne trouve aucune de ces lésions physiques, aucume alteration dans la structure organique de ces parties, et, ce qui est encore plus décisif, c'est qu'on les remarque quelquefois dans d'autres cas differens, et à la suite de certaines maladies entièrement étrangeres à l'alienation mentale, comme l'épilepsie, l'apoplexie, les convulsions, les fievres ataxiques." Traité sur l'Alienation Mentale, p. 453.—Dr. Burrows, in his late valuable work, has given a full account of the observations of modern anatomists on the state of the brain in insanity; Commentaries, p. 58 et seq. Esquirol, in his account of the institution at Charenton, says that his numerous examinations of the brain, "n'apprenderait rien sur le siége et la cause immédiate du délire." He remarks that the researches made on the subjects of Charenton and Salpétrière may be said to be "steriles pour la détermination des conditions materielles du délire;" Ann. d'Hygiène, t. iii. p. 159, 0. He informs us generally, that "the lesions of the brain are neither in relation to the disorders of the mind, nor the diseases communicated to it;" Prichard on Insanity, p. 313. We have an observation to the same effect by Dr. Seymour, Med. Chir. Tr. v. xix. p. 167. For a very ample account of the pathology of the brain, I may refer my readers to Foville's art. "Aliénation Mentale," in Dict. Méd. Prat. t. i. p. 531 et seq.; to this article is appended a copious list of references; also to the remarks of Georget, art. "Folie," § 4. Dict. de Méd. t. ix. p. 257 et seq.; and to Dr. Abercrombie, p. 325..

[2] It were well if on this, as on most other points in which the conduct of the understanding is concerned, we were to follow the sage counsel of Locke,

I have stated that impressions made upon the external senses, and carried to the brain, produce perceptions, and that these constitute the origin of all our knowledge of the properties and qualities of bodies. We are ignorant of the nature of the process by which this train of actions is produced, but it appears certain, that some permanent change is left in the brain, because when a perception has once existed with sufficient strength, a state nearly resembling it may be produced without the repetition of the exciting cause. The state thus produced constitutes an idea[1].

Although every one, who reflects opon his own feelings, must

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and not attach too much importance to questions that are beyond our power ever to decide upon, or which, after long discussion, are finally resolved into mere verbal controversies. See the Introduction to his Essay, especially, § 2, 7. Before I dismiss the subject, I may remark, that the doctrine of materialism has been proposed under two forms or species, which differ very essentially from each other. The one which I regard as the least incorrect, is that which was supported by Priestley, and which rests principally upon the alleged impossibility of defining matter, or of drawing a distinction between the supposed properties of matter and spirit. The other species of materialism is that in which the phenomena of mind are conceived to depend upon some peculiar substance, which is thought to be of a more refined nature than that which enters into the composition of the body generally; such as the materia vitæ, electricity, or the imaginary ethereal fluid, which has borne so conspicuous a part in both our physical and the metaphysical hypotheses. By a very singular inconsistency, we find that some of those who have been the most warm opposers of what we may term the nominal materialism, have been the advocates of this more gross and palpable form of the doctrine.

[1] This is strictly speaking, a perceptive idea. I think it would be convenient to restrict the term idea to an object of thought as proceeding, either directly or indirectly, from external impressions, and to apply the term conception to that state which is induced by the presence of the body which causes the impression. I am, indeed, aware, that in so doing, I am acting in opposition to the great authority of Professor Stewart, who defines conception to be "that power of mind, which enables it to form a notion of an absent object of perception;" Elem. ch. iii. v. i. p. 133; but this difference almost unavoidably follows from the mode in which I have thought it necessary to use the term perception. Hume considers "ideas" to be synonymous with "thoughts;" Essays, v. ii. p. 31. Helvetius, whose language on this subject is generally correct, still farther restricts the term idea to what I have styled intellectual, corresponding to Locke's ideas of reflection; Sur l'Esprit, t. i. p. 68. I think that a degree of ambiguity has been produced in the writings of some of the modern metaphysicians, by the manner in which they have employed the words simple and compound ideas; the former being appropriated to those that originate in perception, the latter in intellectual operations; Hartley, Introd. p. ii. But I conceive that the circumstance of their being perceptive or intellectual has no necessary relation to their condition as being simple or compound. Many perceptive ideas are compounded, i. e. we receive perceptions which are themselves composed of more simple perceptions, and which are impressed upon the mind without any decomposition or analysis. I do not mean by this remark to enter upon the discussion, how far our ideas are, strictly speaking, capable of being compounded, generalized, or abstracted; I only maintain, that the same kind of combination takes place in the one case as in the other. Locke did not make the distinction to which I have referred above; he has complex ideas of sensation, and simple ideas of reflection; Essay, b. 2. ch. 2..12.

be aware of the difference between an original perception and its idea[1], yet it is not easy to determine precisely in what this difference consists. Hume, who makes this the express subject of inquiry, only observes, that the idea is less vivid and distinct than the primary perception[2], an observation which does not afford any adequate solution of the difficulty. But the connexion between perception and ideas, however produced, is one of which we cannot doubt, and it is no less certain, that in some way or other, they are connected through the intervention of the nervous system.

[1] That state of the mind, in which ideas are mistaken for perceptions, may be regarded as the most unequivocal indication of insanity, as distinct from fatuity, on the one hand, or mere obstinate caprice, on the other. As far as we are to be guided in our conclusions by pathological and anatomical considerations, I should maintain, that in insanity the disease of the mind is frequently independent of, or antecedent to, that of the brain. In delirium we may conceive that the primary disease is in the brain, while in fatuity the brain would appear to be imperfect and incapable of performing its functions. There is also another state, in which the brain is morbidly affected, so as to convey false perceptions to the mind, but where the mind itself is sound, and where the individual is consequently aware of the fallacy of his perceptions. A case of this kind is detailed with great minuteness by Nicolai of Berlin, who was himself the subject of it; Nicholson's Journ. v. vi. p. 161 et seq., and v. xv. p. 288 et seq.; see also Mayo's Physiol. p. 213 et seq. Many accounts of spectres, and other supposed miraculous sights and sounds, are to be referred to corresponding changes actually produced upon the brain, although considerably modified by the imagination and credulity of the narrators. This point is very ingeniously elucidated by Dr. Alderson, in his Essay on Apparitions; Ferriar, who has subsequently written on the same subject, has not sufficiently distinguished between three sets of causes, all of which may contribute to the result; the peculiar condition of the brain, the mental indisposition under which the individual laboured, and certain natural phenomena, which, without any disease, either of the brain or of the understanding, were not understood, and were therefore referred to some supernatural agency. Cases of this description, where there are false perceptions, accompanied by a conviction of their fallacy, must, I apprehend, furnish an insurmountable objection to Reid's fundamental position with regard to the mode by which we acquire our knowledge of the external world. On this subject see Parry's Pathology, § 775..782, where we meet with many valuable observations, but I think that the author has incorrectly classed this affection as a variety of insanity, to which it is no more entitled than a false perception proceeding from a morbid state of the nerve of the finger, or any other part of the body. We have a still later work on the Philosophy of Apparitions, by Dr. Hibbert; it contains much curious information and many valuable remarks upon the different morbid conditions both of the body and of the mind, which may be supposed to give rise to these appearances, but I think that in some of his metaphysical speculations he is too refined, and that his views may be characterized rather as ingenious, than as correctly deduced from the acknowledged laws of the animal œconomy. A doctrine not very different from the above is broached by Hobbes in his Treatise on Human Nature, p. 102; a treatise which contains much acuteness of remark, delivered in a quaint and antiquated style.

[2] Essays, v. ii. p. 30, 1.

APPENDIX TO CHAPTER XVI.

The conclusion which Dr. Hibbert draws from his inquiry is, "that apparitions are nothing more than morbid symptoms, which are indicative of intense excitement of the renovated feelings of the mind." p. 375. I am disposed to dissent from that definition on two grounds; first, I think that these phenomena are not always the result of intense excitement; and, secondly, that they are not simply renovated feelings, i. e. a repetition of previous perceptions. With respect to the first point, I should prefer considering them as morbid symptoms, depending upon some irregularity of the natural functions, and which, according to circumstances, may consist either in an excess or a defect of the ordinary actions of the system. And with respect to the second, there are, I conceive, in most cases of this description, new perceptions produced, which had never previously existed. Consequently the part of the nervous system, whether the brain or the nerve, which is affected, is brought into a state which it had never before experienced.

I am induced to form this conclusion partly from the circumstance of having been myself the subject of this disease. Without going into a minute detail of the case, I shall merely state, that I was labouring under a fever, attended with symptoms of general debility, especially of the nervous system, and with a severe pain of the head, which was confined to a small spot situated above the right temple. After having passed a sleepless night, and being reduced to a state of considerable exhaustion, I first perceived figures presenting themselves before me, which I immediately recognized as similar to those described by Nicolai, and upon which, as I was free from delirium, and as they were visible for about three days and nights with little intermission, I was able to make my observations. There were two circumstances which appeared to me very remarkable; first that the spectral appearances always followed the motion of the eyes, and secondly, that the objects which were the best defined, and remained the longest visible, were such as I had no recollection of ever having previously seen. For about twenty-four hours I had constantly before me a human figure, the features and dress of which were as distinctly visible as that of any real existence, and of which, after an interval of many years, I still retain the most lively impression, yet neither at the time nor since, have I been able to discover any person whom I had previously seen, that resembled it.

During one part of this disease, after the disappearance of this stationary phantom, I had a very singular and amusing imagery presented to me. It appeared as if a number of objects, principally human faces or figures, on a small scale, were placed before me, and gradually removed, like a succession of medallions. They were all of the same size, and appeared to be all situated at the same distance from the face. After one had been seen for a few minutes, it become fainter, and then another, which was more vivid, seemed to be laid upon it, or substituted in its place, which, in its turn, was superseded by a new appearance. During all this succession of scenery, I do not recollect that, in a single instance, I saw any object with which I had been previously acquainted, nor, as far as I am aware, were the representations of any of those objects, with which my mind was the most occupied at other times, presented to me; they appeared to be invariably new creations, or at least new combinations, of which I could not trace the original materials.

The circumstance which I at the time considered to be so extraordinary, that the motion of the spectres followed that of the eye, has been also observed by Dr. Brewster, and must therefore be regarded as not so anomalous a fact as I, at the time, considered it to be[1]. Dr. Hibbert supposes this motion of the spectres to prove that the retina itself is the seat of these morbid impressions, and this is the conclusion which naturally presents itself to the mind on the first view of the subject [1]. But perhaps upon further reflection, we may find reason to doubt of the correctness of this opinion. In the first place we have no independent evidence of the eye itself being affected in these cases, while there is every reason to suppose that the brain is the primary seat of the disease. And, in the next place, if we inquire why the mental spectres appear to occupy any definite portion of space, why they seem to be on the right side or on the left side, why they appear at the distance of five, ten, or twenty feet, I apprehend that the answer must be, that our judgment is directed by associations previously formed with states of the brain, which, to a certain extent, resemble the present morbid condition. I have, in a former chapter, stated the circumstances which enable us to judge of the visible position and magnitude of objects, and we may suppose that our ideas of visible motion are derived from associations of an analogous kind. I think it would tend to illustrate this subject, if the patient were directed to observe whether the spectres appear to follow the eye, when the balls are fixed, while the whole of the head is turned round, and likewise what occurs when a degree of vertigo is induced. These observations it did not occur to me to make at the time, nor do I find that they have been made by others. We have a very interesting case detailed in Brewster's Journ. v. ii. p. 21 et seq., where the illusions took place with respect both to the eye and the ear; this double effect renders it the more probable, that the morbid affection was seated, not in the organs of sense, but in the brain. We have a farther account of the same case in v. iii. p. 319 et seq., and in v. iv. p. 261 et seq.; see also Dr. Abercrombie, p. 349..367.

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[1] Journ. of Science, v. ii. p. 8, 9.

[1] Phil. of Apparitions, p. 249..1.

CHAPTER XVII.

OF ASSOCIATION, HABIT, IMITATION, SYMPATHY, INSTINCT, AND IMAGINATION.

Of those classes of vital operations, which I have designated as intermediate between the mental and the physical powers, and as originating, or consisting in the joint operation of both parts of our frame, I shall select the following, as more particularly deserving our consideration, in consequence of the extensive influence which they exert over the animal œconomy; association, habit, imitation, sympathy, instinct, imagination, and volition; upon each of these I shall proceed to offer a few remarks in succession[1].

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[1] It has been a favourite object with some metaphysicians, and among others with Reid, to establish what they conceive to be a complete analogy between the mind and the body, by ascribing to the former a variety of distinct faculties or functions. But this attempt is, I am inclined to believe, at least premature. With respect to the body, we observe that different trains of actions are performed by different organs, and we thence style them different faculties. But we have no independent proof of this being the case with respect to the mind; and, as to the nature of the actions themselves, although it is true that memory and judgment, for example, are different from each other, yet it may be remarked, that as far as we can form any opinion on such subjects, they seem rather to be varieties of the same kind of power, than powers of a totally different nature. See Locke's Essay, b. 2. ch. xxi. § 20. Stewart follows the plan of Reid, and indeed seems to attach more importance to it than Reid himself. Hartley speaks of the faculties of the mind, but it does not appear that he employs the term in the strict sense in which it is used by Reid and Stewart; On Man, v. i. p. 3.

The faculties enumerated by Hartley are memory, imagination, understanding, affection, and will. Of these the memory and understanding are less the objects of physiological consideration. Reid enumerates among the intellectual faculties, the powers which are immediately derived from our external senses, memory, conception, the power of analyzing and compounding, judgment, reason, taste, moral perception, and consciousness; On the Intellectual Powers, p. 76. Some of these, however, upon Reid's own principles, I conceive, are not to be considered as distinct faculties, but as complex feelings, produced by the joint operation of more simple processes, connected by association. Prof. Stewart, in his Elements, considers in succession, as distinct powers or faculties of the mind, perception, attention, conception, abstraction, association, memory, imagination, and reasoning. Cooper maintains that "all the phenomena of thought may be comprised under perception, recollection, judgment, and volition." Tracts, p. 273. On all the topics that are discussed in this chapter I shall refer to the corresponding parts of Dr. Abercrombie's work, a work which cannot be too diligently studied. See also the art. "Facultés Intellectuelles," by Adelon, Dict. de Méd. t. viii. p. 469 et seq.

SECT. 1. *Association.*

When two or more impressions of any kind have been made upon the nervous system, and repeated together for a sufficient number of times, they become associated; so that if one of them only be produced, it will call up the idea of the others[1]. This operation of the animal œconomy is too obvious to have been overlooked by the most casual observer, and frequent allusion is made to it, as well by the philosophers as by the poets and orators of antiquity. It was correctly described by Locke, who appears to have been the first writer that had a clear conception of its importance in the regulation of our thoughts and actions[2]. Since his time it has formed a prominent feature in all our metaphysical systems, and Hartley made it the basis of his theory, forming, as it were, the connecting link between all the vital operations, both physical and intellectual[3]. Adam Smith employed his usual aptness of illustration in describing its influence upon our ideas of beauty, with respect both to objects of taste, and to our sense of moral propriety[4], and it has been made use of by Darwin to explain many of the most complicated functions of the animal œconomy, as well in its state of health as of disease; but although we shall find its operation to be very extensive, I conceive that both Hartley and Darwin have considerably exaggerated its influence.

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[1] Hartley's general theorem is as follows: "If any sensation A, idea B, or muscular motion C, be associated for a sufficient number of times with any other sensation D, idea E, or muscular motion F, it will, at last, excite *d*, the simple idea belonging to the sensation D, the very idea E, or the very muscular motion F." On Man, prop. 20. v. i. p. 102.

[2] Locke, after remarking that "some of our ideas have a natural connexion and correspondence one with another," goes on to state, that "there is another connexion of ideas wholly owing to chance or custom: ideas, that in themselves are not all of kin, come to be so united in some men's minds, that it is very hard to separate them; they always keep in company, and the one no sooner at any time comes into the understanding, but its associate appears with it; and if they are more than two which are thus united, the whole gang, always inseparable, show themselves together;" Essay, b. 2. ch. 33. § 5. v. i. p. 420. In the remaining part of the chapter he points out the influence which the association of ideas possesses over many of our principles of action and modes of thinking. The germ of Locke's doctrine may, indeed, be found in Hobbes; he says, "when a man thinketh on any thing whatever, his next thought after is not altogether so casual as it seems to be;" and, referring our ideas to "motion within us," he supposes that the first motion has some effect in bringing on the second; Treatise on Human Nature, p. 104. Berkeley also describes the fact with his usual clearness and brevity of expression; he says, "that one idea may suggest another to the mind, it will suffice that they have been observed to go together, without any demonstration of the necessity of their co-existence, or without so much as knowing what it is that makes them so to co-exist;" New Theory of Vision, p. 16.

[3] Hartley informs us in his preface, that Gay had, a few years before, published a treatise, in which he "asserted the possibility of deducing all our intellectual pleasures and pains from association." This must have been about the year 1730.

[4] Theory of Moral Sentiments, part 5. v. ii. p. 1 et seq.

Association manifests itself in various ways. Perceptions may be associated with perceptive ideas and with intellectual ideas, and the ideas of each species may be associated with each other, or with ideas of the other species; perceptions may be associated with mechanical actions, and, conversely, mechanical actions with perceptions; mechanical actions may be associated with each other and also with ideas[1]. Examples of all these varieties will readily suggest themselves to the mind, but those with which the physiologist is the most immediately concerned are the associations which take place with muscular contractions.

Darwin lays it down as a law of the animal œconomy, that all animal motions which have occurred at the same time, or in immediate succession, become so connected, that when one of them is re-produced the other has a tendency to accompany or succeed it. Many of those trains of action, which are the most commonly employed in the ordinary concerns of life, are connected together by association; and although, in the first instance, there is, as far as we can perceive, no necessary connexion between the individual actions, and they might even have been associated by mere accident, yet if the conjunction be sufficiently repeated, an association is formed, which can never afterwards be broken. The force of association is so powerful, and its effects are so universal, that it is often difficult to decide, whether any particular actions are connected together by association, or by some other principle of the constitution.

This difficulty may sometimes be resolved, by inquiring into the cause of these complex actions, and observing how they were originally acquired. A number of muscles in different parts of the body are employed in progressive motions of various kinds, for example, in walking. We alternately contract the different muscles of the lower extremities; by an effort of the back and loins, we throw the weight of the trunk, alternately, from side to side, and we generally move the arms at the same time, to assist in balancing the body, and preserving its perpendicularity. All this series of complicated motions is so connected together, that it would require a powerful exertion of the will to perform them in a different order, and they proceed with almost as much regularity as the motion of the heart, or any other over which the will has no control. Yet the act of walking is one which can only be acquired by long practice; and we have reason to suppose, that if a person, born without arms or legs, could be afterwards furnished with them when advanced in life, he would be as incapable of walking as of playing upon an instrument of music. There are, on the contrary, other actions strictly voluntary, and which also require the co-operation of many muscles, but which seem to have some necessary connexion with each other. Of this description is swallowing. The muscles of the mouth, lips, cheeks, tongue, throat, and neck, are all concerned; and they are made to succeed each other in such an order, as to produce one of the most beautiful and complicated mechanical actions of which the human body is capable; yet we observe that the infant, immediately after birth, swallows the mother's milk with as much facility as it ever afterwards acquires. This train of actions, therefore, cannot be connected by association.

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[1] The different classes of associations are given in some detail by Mr. Belsham in his Elements; ch. 3. sect. 1, 2. p. 22..35; he follows the system of Hartley with very little deviation. Hume conceives that all our associations may be traced to the three principles of resemblance, contiguity in time and place, and cause and effect; Essays, v. ii. p. 73; to these, I presume, we ought to add analogy. Darwin justly remarks, that the general direction of our inquiries and the nature of the information which we store up in the mind, depend very much upon the kind of association which we the most frequently employ; Zoon. v. i. p. 49..3. Priestley's Hartley, Introd. Essays, No. 2. contains "a general view of the doctrine of the association of ideas," which may be perused with advantage.

It hence becomes a curious subject of inquiry, whether we can point out any circumstance which can enable us to discover on what the difference in these two cases essentially depends. What we naturally look to, as the probable means of solving this difficulty, is the nervous system; and we are led to inquire, whether there be any thing in the disposition of the nerves, the source whence they are derived, or the connexion which they have with each other, which can throw any light upon the subject. I think we may venture to assert, that our present knowledge will not afford us any satisfactory solution of the difficulty; but so much has been accomplished in this department of physiology by Sir C. Bell, that we may expect to obtain considerable assistance from this source in our future investigations. In the mean time, we seem to be warranted in concluding, that where the association is formed between muscular parts, all of which are entirely under the control of the will, as, for example, between the muscles of locomotion, we may refer the effect to the principle of association; but that in proportion as the trains of actions are less under the control of the will, they seem to be referable to the effect of association, combined with that of mere nervous connexion.

Sect. 2. *Habit.*

A principle in the animal œconomy, which is nearly allied to association, and which produces equally powerful effects, is habit[1]. It may be defined, a peculiar state of the mind or body, induced by the frequent repetition of the same act. Habit is proverbially styled a second nature, and there are numerous instances which prove the truth of the remark, for there is scarcely any impression however disagreeable, or any mode of life however repugnant to our feelings, to which by habit we do not become reconciled [1]. The operation of habit has been supposed, by some metaphysicians, to be confined to the voluntary actions, or, at least, to such as were originally so, although they may have subsequently become involuntary. But this limitation appears to be incorrect; the perceptions, the intellectual operations, and even the physical functions, are all of them considerably influenced, and, in some instances, even completely reversed by the effects of habit.

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[1] Reid divides what he terms the active powers of man into the three classes of mechanical, animal, and rational, and makes instinct and habit to compose the first class; Essays, 3. ch. 1. p. 97 et seq.; but I acknowledge that I do not perceive the propriety either of the denomination or of the restriction. He defines habit, "a facility of doing a thing, acquired by having done it frequently." Ubi supra, p. 117. It not unfrequently happens that actions, which are referred to habit, because we are not acquainted with any better mode of accounting for them, are found to be actually referable to some specific cause. Of this description appears to be the preference which is gene-

But, although the operation of habit is to be observed perhaps equally in the mental and in the physical part of our frame, yet the course of our inquiries will naturally lead us to regard it more as it respects the bodily constitution. And whether we contemplate it as affecting the functions, both intellectual and sensitive, or the operation of external agents upon the system, we shall find its influence to be almost universal. In the accommodation to circumstances, which is the result of habitual action, we may perceive the operation of what I have termed the principle of self-adjustment; but, in this case, the effect being brought about slowly or almost imperceptibly, the action is less observable, and the successive steps of the operation are often not to be detected. Some of the most remarkable instances of the changes induced by habit are in the nutritive functions, where the digestive organs must have experienced a complete alteration in their mode of acting upon bodies, so as to render those substances digestible, and even salutary, which were originally inert and indigestible. In this case we are led to conclude, that the secretions of the alimentary canal must have experienced such an alteration, as to adapt them to the new substances upon which they are destined to act, while a no less remarkable alteration must have taken place in the nervous system connected with these parts.

rally given to the right arm over the left. Comte has lately published an ingenious essay on the subject, in which he endeavours to prove, that this peculiarity depends on the anatomical structure of the fœtal vessels, and on the consequent position which the fœtus naturally assumes while in the uterus.

[1] Custom and habit are frequently considered as synonymous terms, but, strictly speaking, we must regard the latter as the effect of the former. "Custom is the frequent repetition of the same act; habit is the effect of such repetition." Taylor's Synonyms, p. 52. Cullen points out this distinction; but, when he enters upon a detail of the effects of custom on the animal functions, he frequently violates his own definition. The remarks which he makes are, however, very judicious, and show the powerful effect of this principle upon some parts of the system which might seem to be the least connected with its vital powers. He describes its effects: "1. On the simple solids; 2. On the organs of sense; 3. On the moving power; 4. On the whole nervous power; 5. On the system of blood vessels." Mat. Med. p. 21..31.

The effects of habit on the sensitive functions, where the nervous system is more directly concerned, are no less remarkable. There appears to be no assignable limit to the alteration which may be produced, both as to the degree and even the very nature of the effect. We become insensible to the most powerful agents, and we acquire artificial perceptions, which are frequently the most opposite to those that seem the natural result of impressions made upon the external senses.

The effects of habit are peculiarly observable in those operations which recur only after certain intervals, such as taking food or going to rest. When the usual period arrives, we experience the sensation of hunger, or become oppressed with drowsiness, not because the stomach is entirely empty, or the powers completely exhausted; for if by any accident the meal be deferred, or the inclination to sleep be powerfully resisted, the hunger and drowsiness leave us, and some hours may elapse before we again perceive the effects[1]. It is extremely difficult to assign any cause for these periodical accessions of habitual feelings; but it may be observed, that there is a tendency in the human constitution to go through a certain train of actions in the space of the diurnal revolution of twenty-four hours. This is observable in the state of the pulse, and in many of the secretions, and it may be presumed that the influence of habit is confined to this limit, for it is not probable that, by any volition, we could so far change our modes of life, as to go through the usual routine in a shorter time, or extend it to a much longer, for example, in eighteen hours on the one hand, or to thirty on the other[2].

One of the most remarkable effects of habit is to blunt or diminish sensations of all kinds, so that not only do disagreeable impressions cease to be so, but even pain, if not too violent, becomes comparatively indifferent. On the contrary, many circumstances, originally indifferent, acquire by habit a kind of connexion with the animal œconomy, which makes them almost essential to the continuance of its functions. The effect of repetition upon the intellectual operations is very different from that upon the nervous functions, for while these are diminished by it, the former are rendered more acute[1].

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[1] The temporary cessation and subsequent renewal of these feelings may, to a certain extent, be ascribed to re-action, as far as respects the stomach; but the period of their recurrence, and the regularity of their return depends upon habit.

[2] Although I think it can scarcely be doubted, that there is a natural tendency in the animal œconomy to undergo a certain succession of actions in the diurnal period, it is extremely difficult to determine, how far it should be assigned to this cause, and how far to habit. The alternation of day and night obviously directs most of the occupations of life, and the influence of this alternation is impressed upon us, in various ways from our earliest infancy. The Esquimaux tribe, that was discovered by Sir John Ross, in the N.E. part of Baffin's Bay, who, for nearly eight months of the year, are not subject to these alternations, and who are, at the same time, unconnected with the inhabitants of other countries, seemed to afford an excellent opportunity of throwing some light upon this point, by ascertaining how far their customs are founded upon the observance of the diurnal period. The information that was obtained is unfortunately imperfect, but as far as it goes, it favours the idea, that they have no regular periods for taking food or rest; see Capt. Sabine's narrative in Quart. Journ. v. vii. p. 80.

SECT. 3. *Imitation.*

The next principle which I shall notice is imitation. Perhaps, strictly speaking, imitation ought to be regarded as a complex action, rather than as a distinct principle, yet it seems to depend upon a peculiar state, which is not very easily referable to any more general effect, and which, from whatever cause it originates, produces very important operations. There is a tendency to, or capacity for, imitation naturally existing in the constitution, for one of the first symptoms of intellect that we perceive in children is their attempt to imitate the actions of those around them. This has usually been regarded as an ultimate fact, a circumstance, the reality of which we cannot doubt, but the causes of which we are unable to explain[2].

We may speculate so far as to assume, that it is more easy to imitate an action which is impressed upon our senses, than to invent a new one, and that when an action has been once performed, the repetition of it is more easy than the original performance, and would seem even to be attended with a certain degree of pleasurable sensation. But although the physical cause of imitation is obscure, its final cause is obvious and important. By imitation we learn the use of speech, or the power of uttering articulate sounds, and when aided by association, of comprehending them when uttered by others.

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[1] Bichat. Sur la Vie &c. art. 5. p. 29 et seq. His remarks on the effect of habit upon the sensitive, or as he terms them, the animal functions, are generally correct; but I cannot agree in his observation that the organic functions are " constamment soustraits à l'empire de l'habitude." He continues: " La circulation, la respiration, l'exhalation, la nutrition, les secretions ne sont jamais modifiées par elle." p. 35. I conceive that a sufficiently long course of habitual action will considerably modify, perhaps every one of these, certainly the two last. The author, indeed, in the next paragraph, in a great measure, retracts his assertions. On the subject of habits and acquired peculiarities see Adelon, Physiol. t. iv. p. 512 et seq. I may also refer to some observations of Dr. Christison's on the effect produced by the habitual use of large quantities of opium, as illustrating the changes produced on the different functions; Ed. Med. Journ. v. xxxvii. p. 123 et seq.

[2] Reid observes, " Another thing in the nature of man, which I take to be partly, though not wholly instinctive, is his proneness to imitation;" On the Active Powers, p. 111. A theory, if we may so term it, of imitation, is formed by Darwin, and is elaborated with his usual dexterity; Zoon. sect. 22. § 3; but, I think, I may venture to assert, that there is no one position on which the theory is built, for which he has adduced any substantial proof, and that the whole rests upon a series of analogies that are indefinite and inapplicable.

Hence we acquire the rudiments of all our future education, and profit by the knowledge of those who have preceded us.

I have already had occasion to offer some remarks upon the voice and speech, as produced by the contraction of certain muscles attached to the glottis, and to the tongue and lips respectively[1]. The problem which now remains for us to solve is, in what manner, or by what medium of communication, we are enabled to become acquainted with the actions of these muscles, and thus to imitate them, when some of the parts are entirely concealed from our view, and the rest we know, rather from anatomical examination, and from minutely investigating the operation, for the purpose of experiment, than from our ordinarily noticing their action. We acquire our ideas of the tone of the voice entirely by the ear; and that, in the case of speech, we derive our knowledge principally from the same sense, is proved by comparing the state of the blind with that of the deaf, in respect to their capacity for uttering articulate sounds. In the former case no deficiency is perceived; often, indeed, they possess a remarkable accuracy in this faculty, in consequence of the attention being almost exclusively confined to audible impressions, whereas, on the contrary, in the deaf, it is only after a long and tedious process, that they are able to acquire a very imperfect power of articulation.

The fact then would appear to be, that certain contractions of the muscles of the glottis, and of the parts connected with the mouth, enable us to produce vocal and articulate sounds, but that the changes immediately connected with these contractions are either concealed from our view, or not observed by us, so that the only intimation which we obtain of their existence is conveyed to us by the ear. Without knowing how the change is actually effected, we, by an act of volition, produce the same change in the larynx or mouth, and thus produce the same sound. It has been conjectured, that we learn by repeated trials what peculiar sensation in the muscles of the organs is excited by their contraction, and the consequent emission of certain sounds, and that when we wish to re-produce the same sound, we begin by re-producing the same sensation through the intervention of the muscles[2]. But this supposition only removes a part of the difficulty, even supposing this experimental process to have been gone through, of which, I conceive, we have no proof, and are certainly altogether unconscious.

SECT. 4. *Sympathy.*

One of the most distinguishing peculiarities of the animal frame, unlike any thing that we behold in inanimate nature, is the connexion subsisting between different parts, which we term sympathy[1]. There is scarcely any action which we perform, or any part that is moved or affected, but the motion or affection influences other parts, besides those primarily acted upon. In some cases this evidently depends upon mere contiguity, in others we can trace a direct vascular or nervous communication, and it may be frequently referred to association. But there are instances where none of these causes seem to be applicable, where the parts are distant from each other, where there has been no repetition of the actions, so that they cannot have acquired an association with each other, and where there seems to be no direct communication through the medium either of the vessels or of the nerves.

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[1] P. 417.

[2] Hartley on Man, ch. 1. sect. 3. prop. 21. v. i. p. 107..9.

But, although in such cases, we perceive nothing in the physical disposition of the parts which can explain this sympathetic connexion, I am disposed to think, that it must be referred to the operation of the nervous system. One important use of this system is to unite all the several parts and functions of the animal machine into one connected whole, each portion of which may, to a certain extent, feel the impressions that are made upon every other portion.

Although, however, we may suppose that the sympathetic actions are thus connected with each other, by what may be termed an indirect operation of the nervous influence, it will still remain for us to inquire in what way this connexion is effected. And here two important questions present themselves; is the influence, in such cases, conveyed by a certain set of nerves only, for example, by those belonging to what has been termed the sympathetic system, or by the non-symmetrical nerves of Sir C. Bell? and, secondly, is the intervention of the sensorium necessary? To the first of these questions we may, I believe, without hesitation, answer in the negative, because we have many cases of obvious sympathetic action, in parts where these nerves do not exist, and yet, as we can conceive of no other medium of connexion except by nerves, we must refer it to the operation of those of another description. With respect to the other point, whether the intervention of the sensorium commune be necessary, I may remark, that this subject was fully discussed by the physiologists of the last century, and more especially by Whytt. His phraseology is often, as I conceive, incorrect, in consequence of its resemblance to the Stahlian hypothesis, but the facts which he adduces are of such a nature as, I think, to prove that the co-operation of the brain is essential in those actions which we refer to the operation of sympathy[1].

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[1] Darwin, in treating of the effects of sympathy, as is too frequently the case, has involved the subject in a series of metaphysical subtleties; Zoon. sect. 31. § 1. v. i. p. 441 et seq. Parry defines sympathy as follows: "When, from a cause immediately acting on one part, so as to produce sensation or motion, either or both of these effects is produced on another part, that second effect is called sympathy. Thus, in inflammation of the liver, a pain is sometimes felt on the top of the right shoulder." Pathology, § 607. p. 259. He points out four species of sympathies; 1. Sensation producing sensation; 2. Sensation producing motion; 3. Motion producing motion; 4. Motion producing sensation. We must be careful not to confound the effect of sympathy with those of association; but by ascertaining the mode in which they were originally produced, it will be, in most cases, not difficult to discriminate between them.

Among the affections which are ordinarily termed sympathetic, I may mention the general uniformity in the motion of the two eyes; the secretion of milk by the mamma, consequent upon parturition: the convulsive contraction of the diaphragm which produces sneezing, as caused by the irritation of the nerves belonging to the mucous membrane of the nostrils; pain of the head occasioned by a certain condition of the stomach; imperfect vision from a morbid state of the intestinal canal; vomiting from the irritation of a biliary calculus in the duct of the liver; and a variety of other affections, the occurrence of which would never have been predicted or suspected, but which are well ascertained matters of fact. It must be admitted, that in these cases, we do not perceive any peculiar or especial nervous connexion, which might seem necessary to account for the phenomena, but we are so well acquainted with the nature of nervous action as to justify the conjecture that it is the immediate agent in these operations.

I have been speaking of sympathy as affecting different parts of the same body, but its operation is more wonderful as affecting different individuals. Besides the mental impressions of a sympathetic kind, which, like other complex intellectual processes, depend upon a number of associations, originating from various causes, there is a kind of physical sympathy, by which, from observing pain or suffering in another, the body becomes actually affected in a similar manner. This subject has been ingeniously illustrated by Adam Smith, and forms the basis of his beautiful, although perhaps fanciful theory of morals. He observes, that the source of our compassion for the sufferings of others arises from the faculty which we possess, not only of imagining ourselves in the situation of the sufferer, but of actually being affected, to a certain extent, with the same painful sensation. "When," says this writer, "we see a stroke aimed and just ready to fall upon the leg or arm of another person, we naturally shrink and draw back our own leg or our own arm, and when it does fall, we feel it in some measure, and we are hurt by it as well as the sufferer."[1]

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[1] See particularly his treatise, On Vital and Involuntary Motions, sect. 11. entitled, "On the share which the mind has in producing the vital and other voluntary motions of animals." Works, p. 140 et seq. For the opinion of Cullen on the subject, see his life by Thomson, p. 305..9. After having stated in the text what, I believe, may be regarded as the ascertained fact, we may, in the notes, indulge in a little speculation on the subject. I have divided the nerves into three classes, the simply sensitive, the perceptive, and the motive; the first of these, as far as we know, transmit their influence in both directions, and without any regard to a central point of union. In the second, the influence passes from the extremities to the centre, and in the third from the centre to the extremities. Now, in the production of sympathy, a perception is transmitted by a nerve of the second class to the brain, and a consequent change is propagated from this organ by a nerve of the third order; if motion be the ultimate result, we may suppose the operation to produce no farther effect, but if it be a perception which ensues, we may conceive a motion, possibly in the capillary arteries, to have been produced; this re-acts upon a nerve of the second order, which conveys the perception to the brain.

To this cause may be attributed the tendency to fainting, which many individuals experience at the sight of blood, or from being present at a severe surgical operation. A still more remarkable example of this transferred sympathy occurs in some kinds of convulsive diseases, where the sight of the patient will excite a similar disease in the spectator. Were it not digressing from the proper subject of physiology, I might here mention the effects of different kinds of fanaticism, very remarkable instances of which are upon record, where violent motions having taken place in certain individuals, have been propagated, apparently in an irresistible manner, among all their followers. Examples of this kind are not uncommon even in our own age and country, and whatever we may think of the principles of those who encourage them, or of the state of mind by which they are produced, we can have no doubt of their reality as the result of sympathetic impression[2].

It is, I conceive, very difficult to explain these phenomena, or to refer them to any more general principle. It may be said that they depend upon a species of imitation, but the imitation, if it be so considered, is essentially different from the ordinary kind, as being involuntary. Upon the whole, I am disposed to regard this class of actions as specific, and not explicable by any of the powers which are generally admitted, as regulating the operations of the living body[3].

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[1] Theory of Moral Sentiments, v. i. p. 4. See the same idea expanded in Stewart's Elements, ch. 2. sect. 1. v. iii. p. 153 et seq.; On Sympathetic Imitation. The remarks of this writer are always interesting and elegant, but, it must be acknowledged, that they are frequently diffuse, and deficient in that correct precision, which is so desirable in metaphysical discussions.

[2] A very remarkable train of facts, which may be referred to this source, as examples of sympathetic impressions propagated through a number of individuals, and affecting both the mental and the corporeal functions, is related in the Edin. Med. Journ. v. iii. p. 434 et seq. See also an account of Epidemic Convulsions, in the Isle of Anglesea, related by Dr. Haygarth, in his Essay on the Imagination, § 2. p. 47 et seq.

[3] Dr. Alison has lately published an elaborate essay on Sympathy, in the second volume of the Edin. Med. Chir. Trans. p. 165 et seq. His definition of physiological sympathy, although perhaps somewhat longer than necessary, I conceive to be correct and appropriate. The term sympathy, he remarks, "is correctly applied to all combinations or successions of the vital phenomena presented by different parts of the body, which we observe so generally, that we judge them not to be accidental, which are independent of the will, and not owing to any necessary dependence on one another, which we can refer to other ascertained laws of the animal œconomy, of the living actions of the

SECT. 5. *Instinct.*

A principle of great importance in the animal œconomy, both as regards the individual, and the relation which subsists between different individuals, is instinct. It may be defined[1] a capacity for performing by means of the voluntary organs, certain actions which conduce to some useful purpose, but of which purpose the animal is itself ignorant[2]. It is well illus-

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parts thus simultaneously or successively affected." p. 166. He divides the effects of this principle into those that produce sensations merely, and those that produce actions, but it is to the latter of these that his observations are exclusively directed, although, I may remark, that most of his statements will apply equally to both of these cases. The cause of these actions, or the nature of the connexion which subsists between the primary and the consequential change, is fully considered by the author. He gives many reasons for the opinion, which was maintained by some of the most eminent physiologists of the last century, that we can discover no direct anatomical connexion, sufficient to explain the phenomena; while he endeavours to prove that there is, in all cases, an indirect connexion, through the intervention of the brain, or according to Whytt, by means of a mental sensation. The exposition of this doctrine is given at considerable length, and it is shown, that some of the late experiments of Dr. Philip serve very materially to establish its correctness. The following quotations contain a summary of the reasoning employed in the first part of this paper. "It is quite obvious, that the instances now given of irritations of different and distant parts, producing the same sympathetic action when they excite the same sensation,—and still more the instances of different irritations of the same parts, producing totally different effects of this kind, when they excite different sensations, are nearly incompatible with the supposition of sympathies depending on certain definite nervous connexions," p. 185: and again; "I have now stated the arguments whith appear to me the most convincing, in regard to the two principles formerly laid down; *first*, that the sympathetic actions we have considered, in the natural state, are to be ascribed to the influence of certain mental sensations; and, *secondly*, that the effect of these sensations, in producing them, cannot be explained on the anatomical principle of connexions among the nerves of the sympathizing parts." p. 189.

[1] A more concise definition has been proposed; spontaneous impulse to certain actions not accompanied by intelligence. Reid defines it, "a natural blind impulse to certain actions, without having any end in view, without deliberation, and very often without any conception of what we do." On the Active Powers, Ess. 3. ch. 2. p. 103. I may remark, that the chapter on instinct, although, like every other part of Reid's works, deserving of an attentive perusal, is upon the whole vague and indeterminate. Cabanis describes instinct to consist in determinations made by the animal, independent of its volition; Rapports, t. i. p. 86. He afterwards defines it more precisely, "Le produit des excitations dont les stimulus s'appliquent à l'interieur." p. 137. We have a further and more detailed account of it in t. ii. p. 388 et seq.; but in this, as in other parts of his work, he confounds the action of the organic functions with the effects of instinct. For the definition of instinct and the illustration of its nature and effects I may further refer to Buzareingues, Phil. Physiol. p. 79; to Broussais, Physiol. appl. à la Pathol. ch. 7. t. i. p. 111..141; to the elaborate article "Instinct," by Virey, in Dict. des Sc. Méd. t. xxv. p. 367 et seq.; to the outlines of Dr. Alison, Sect. 14; and to Mr. Mayo's, p. 189..194.

[2] Magendie applies the term instinct more generally to "des penchants, des inclinations, des besoins, au moyen desquels ils sont continuellement excités et même forces à remplir les intentions de la nature." El. Phys. t. i.

trated by the example, so frequently cited, of the mode in which birds proceed in building their nests. If a bird be taken from its parent, soon after being hatched, and be confined in a cage, so as to have no communication with other birds, before it lays its eggs, it will prepare its nest with as much skill as if it had been brought up with individuals of the same species, and had practised the building of nests for a number of successive seasons[1].

The motive which directs the bird, and the skill which it displays, cannot have been derived either from tradition, imitation, or reason, nor can the effect depend upon any direct impression made upon the nerves or muscles. We must here suppose that a particular state of the brain exists, similar to what, in human beings, is gradually induced by reason or instruction, which prompts to a train of actions, as far as we know, connected together only as they tend to one ultimate object. When an animal for the first time receives those feelings which induce it to prepare for its young, it can have no conception of the events which are to follow; it can form no idea of the nature of its offspring, of its wants, nor, in short, of any thing connected with it. We must therefore suppose, that a part of its œconomy consists in having certain impressions made upon the brain at certain periods, corresponding to the time of laying the eggs, which lead to the same effect, as if these impressions had resulted from causes which induce analogous actions in the human species.

The above is one of the most complicated cases of instinct; there are some where the effect appears to be the result of a direct impression upon a nerve or an organ of sense, and when the impression is followed by a certain action, similar to what, in a human being, we should attribute to association. To this may be referred the natural dislike or antipathy which animals experience to certain articles of food, which are not suited to their digestive organs. It would appear that, in these cases, they are principally guided by the smell, for there are remark-

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p. 207. According to this definition, all the natural appetites are included under the class of instincts, for some of which at least, although the capacity be implanted in the constitution, the exercise of them depends upon the knowledge which we acquire by education or the intercourses of society. What he afterwards describes under the title of social instincts are many of them complex feelings, arising principally from various modes of association. I remarked above, that Cabanis does not sufficiently distinguish between automatic and instinctive actions; Reid has likewise fallen into this error, ubi supra, p. 103 et seq., and it seems to be the case even with Parry; Pathol. § 620. .2. But it is sufficiently easy to make the distinction, if we bear in mind, that the latter are performed without the direct application of a stimulus, and through the intervention of the voluntary organs, although not, strictly speaking, by an effort of volition. M. Virey unequivocally refers the action of the vital functions, even of the absorbents, to instinct; art. "Instinct," in Dict. Scien. Méd. t. xxv. p. 377.

[1] The case of the newly born lamb, which was adduced by Galen, as an illustration of the power of instinct, is equally remarkable; see Young's Lectures, v. i. p. 449, 0. and Parry's Pathol. 620, 1.

able instances, where this sense seems to direct them, even in opposition to the most palpable evidence of some of the other senses. There is a species of Stapelia, which has exactly the odour of putrid flesh, and it is observed that the carrion fly lays its eggs on the flower, no doubt under the instinctive impression that it thus provides a suitable lodgment for its young[1].

There is a series of anatomical facts, connected with this subject, which seems to demonstrate that instinct is, in its essential nature, a different principle from reason. By comparing the faculties of different classes of animals, we find that these two powers generally exist in a kind of inverse ratio to each other; the more perfectly organized animals possessing a larger share of reason, and those that are less so being more directed by instinct. Now by observing the nervous system of these animals respectively, we find that there is a gradation in the comparative size of the brain and nerves, which corresponds to the state of their faculties. In Man, where reason exists in the greatest degree, and where instinct holds a subordinate place, the brain is the largest, in comparison to the rest of the nervous system. In quadrupeds and birds the size of the brain decreases, while that of the spinal marrow and nerves increases; this comparative scale goes on through the amphibia and fish, until we arrive at some of the insect tribes, which, although they possess a variety of organs and many elaborate functions, yet have very small and imperfect brains[2]. And we observe that the faculties of reason and instinct bear a respective ratio to the comparative size of the brain and nerves. In quadrupeds we have very decisive proof of the operation of instinct although still with an evident portion of reason; in cold-blooded animals instinct very much predominates, and to this faculty we shall probably, upon mature reflection, refer many of the varied operations of the insect tribes, their variety and perfection depending rather upon the variety and perfection of their organs of sense and motion, than upon the nature of the principle which directs the actions.

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[1] The remarks of Cuvier appear to me so appropriate, that I shall present them to my readers at some length. "Il existe dans un grand nombre d'animaux une faculté differente de l'intelligence; c'est celle qu'on nomme *instinct*. Elle leur fait produire de certaines actions nécessaires à la conservation de l'espèce, mais souvent tout à fait étrangeres aux besoins apparens des individus, souvent aussi tres compliquées, et qui, pour être attribuées à l'intelligence, supposeraient une prévoyance et des connaissances infiniment supérieures à celles qu'on peut admettre dans les espèces qui les exécutent. Ces actions, produites par l'instinct, ne sont point non plus l'effet de l'imitation, car les individus qui les pratiquent ne les ont souvent jamais vu faire à d'autres; elles ne sont point en proportion avec l'intelligence ordinaire, mais deviennent plus singulières, plus savantes, plus disinteressées, à mesure que les animaux appartiennent à des classes moins élévées, et, dans tout le reste, plus stupides. Elles sont si bien la proprieté de l'espèce, que tous les individus les exercent de la même manière sans y rien perfectionner;" Regne Animal, t. iv. p. 53.

[2] Buzareingues asserts, as a general fact, that the relative size of the corpora quadrigemina to the other parts of the brain is in the inverse ratio of the intellect and the direct ratio of the instinct; Phil. Physiol. p. 83; but it does not appear that he founds his statement on his own observations; it is, however, to a certain extent, confirmed by those of Serres. Mr. Mayo has shown, by direct experiment, that in one case at least, the same nerve is subservient both to the instinctive and the voluntary motions of a muscle, and from this circumstance he concludes, that instinctive motions are necessarily voluntary. I believe Mr. Mayo to be correct as far as the organs are concerned, but I regard instinct and volition as essentially different faculties; at the same time I may remark, that this question, like so many others of a similar nature, is perhaps rather one of words than of ideas. The remarks

The operation of instinct, as observable in the inferior animals, is so remarkable, that the existence of this principle in them has seldom been doubted. It has, however, been called in question by Darwin; he argues that the effects of instinct should be always uniform, and proceed precisely in the same track, as it is a kind of blind impulse, impressed upon the animals, which is exactly the reverse of reason. But he remarks, that in the actions usually called instinctive, as the building of a nest, we discover symptoms of reason; we see the bird adapting itself to circumstances, both in the position and choice of its materials. If it cannot procure the substance which similar birds employ, it endeavours to get something like it, and if it cannot build the nest exactly in the proper situation, it searches out for one resembling it[1]. To this argument it may be replied, first, that animals possess a certain share of reason, and it does not follow that this is to be extinguished by instinct; it is more probable that they will co-operate to the same end, and will each supply the deficiencies of the other. And, in the second place, there is no ground for supposing, that instinct consists in this blind impulse to certain specific actions; it seems rather to depend upon a state of mind impressed on the animal, which may lead it to accomplish the action in the best manner that is within its power. But there is one circumstance, which I regard as an unanswerable proof of the existence of instinct; that there are many animals, whose whole duration is only for a short space of time, at a certain period of the year; they can therefore never see their parents

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of Broussais may be referred to; Physiol. Ch. 7; but I conceive that he is not sufficiently discriminative in his definition of those actions which are to be considered as instinctive.

[1] Zoon. sect. 16. v. i. p. 135 et seq. He remarks, that "all those actions of men or animals, that are attended with consciousness, and seem neither to have been directed by their appetites, taught by their experience, or deduced from observation or tradition, have been referred to the power of instinct;" p. 136. This definition is defective from its not, on the one hand, excluding the operation of a direct external stimulus, and on the other, from its not including the final cause of the action. Brown's remarks on Darwin's objections to our ordinary conceptions of the operation of instinct are very judicious and satisfactory; Remarks on the Zoonomia, sect. 9. p. 263 et seq. He defines it, "predisposition to certain actions, when certain sensations exist;" p. 265.

and of course derive no benefit from the experience of their predecessors. Yet in their habits they exactly resemble them, and have gone on so, precisely in the same track, for hundreds of generations. This applies to several of the insect tribes, whose habits are often extremely curious, and exhibit much of what might be called ingenuity and contrivance, did not its uniformity prove it to be instinctive.

It has been a subject of contention among metaphysicians whether man possesses any thing which can properly be called instinct, or whether those actions which, at first view, appear to be of this description, are not more properly to be referred to other sources. Reid, and most of the Scotch writers, have supposed that we possess a great variety of principles that are innate, or at least originate from the nature of our constitution, independent of any external circumstances. The disciples of Locke, on the contrary, have very much diminished the number of these original qualities, and have endeavoured to account for the effects, by the operation of various agents, directed by association, sympathy, imitation, and some other of the principles which have been described above. That these innate principles have been multiplied to an unnecessary, or even a ridiculous excess, seems to be now generally allowed, and also that some cases, which at first view appear the most complicated, have been resolved into other faculties, seems equally probable; yet I am disposed to think, that there are certain actions, which are the most conveniently explained by admitting the existence of instinct. And this appears to be agreeable to the analogy of nature. The actions of brutes are directed by a large share of instinct, mixed with only a small portion of reason; those of man by a greater proportion of reason, but not without some admixture of instinct[1].

SECT. 6. *Imagination.*

The imagination is a faculty of a purely intellectual nature[2], yet its effects upon the body are so remarkable, that it will be proper to take some notice of them in this place. When the mind is stored with ideas, either obtained from the perception of external objects, or from the operation of its own powers, it possesses the faculty of combining these ideas in various forms, and of disposing them in new trains, different from those in which they were originally received[3]. This constitutes the imagination, which thus becomes the source of a new set of feelings, often more powerful than those immediately derived from the direct impressions of external objects[1]. It belongs to the moralist and the poet to trace the effects of the imagination upon the passions and the feelings, but its influence upon the corporeal functions strictly falls under the cognizance of the physiologist. Many facts clearly prove that the imagination can affect, not only the nervous system, which might be supposed the more immediate subject of its operation, but that it can act upon the circulation, the respiration, the digestion, and, in short, that it is one of the most important agents in the animal œconomy. In medical practice, its effects are the subject of daily observation, and present at one time the most powerful obstacle, and at another the most active assistant, to the exertions of the physician. The history of medicine abounds with examples of its influence, and the greatest sagacity is requisite to distinguish between the physical effect of remedies and their power over the imagination. Instances are daily occurring of remedies being announced, under some secret or mysterious form, which accomplish the most remarkable cures, attested by unexceptionable evidence. The composition of these remedies is generally, after some time, made known, but it may be asserted, that there is scarcely a single instance on record, in which the same beneficial effects have resulted after the discovery. An account of the influence of the imagination, as connected with medicine, would afford a melancholy detail of the weaknesses and follies of human nature. The powers of witchcraft were universally acknowledged, the most ridiculous and disgusting compounds were sanctioned by colleges and universities, not much more than a century ago, and in our own times we have seen the general assent which has been given to animal magnetism, and the metallic tractors.

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[1] Prof. Stewart correctly observes, that in infancy "existence is preserved by instincts, which afterwards disappear when they are no longer necessary;" Elements, sect. 8. p. 270.

[2] According to Prof. Stewart, the imagination is not a distinct power of the mind, like attention, conception, or abstraction; Elem. p. 478. The remark may be metaphysically correct, but the effects of the imagination are sufficiently distinct to warrant our considering them in a separate section.

[3] Darwin employs the term imagination in a somewhat different sense; he

A remarkable series of facts on this last subject was published a few years ago by Dr. Haygarth. While the delusion was at its height, he determined to ascertain how far the effects ascribed to this instrument could be accounted for by the powers of the imagination. He accordingly provided himself with bits of wood, formed like the tractors, and with much assumed pomp

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does not think it necessary that the ideas should be combined in an order different from that in which they were originally received; he appears to resolve it nearly into recollection or memory; Zoon. v. i. p. 43, 130. Indeed, very much the same idea was entertained by Hobbes; he defines imagination "conception remaining, and by little and little decaying." Treatise on Human Nature, p. 4; he speaks of imagination and conception as very nearly synonymous terms.

[1] Montaigne's Essay on the Force of the Imagination, b. 1. ch. 20, affords a curious and amusing specimen of the combination of sagacity and credulity. The account given by Newton, of the power of the imagination in producing ocular spectra, as occurring in his own person, is a sufficient proof of the influence of the mental over the physical functions; Brewster's Journ. v. iv. p. 75 et seq.

and solemnity, he applied them to a number of patients, whose minds were prepared for something extraordinary. He not only used them in nervous diseases, where the cure is often equivocal, and may be ascribed merely to fancy or caprice, but he employed them in cases apparently of the most opposite nature. The effects were astonishing: obstinate pains of the limbs were suddenly cured, joints that had been long immoveable were restored to motion, and, in short, except the renewal of lost parts, or the change of mechanical structure, nothing seemed beyond their power to accomplish[1]. Had we the imagination at all times under our control, we might dispense with a large part of the materia medica. Undoubtedly, upon this principle, we must explain many of the pretended miracles of ignorant ages and nations. The facts are true, but the inference from them is false. In some cases the vulgar were imposed upon by designing impostors, but not unfrequently we may conclude, that both parties were equally the dupes of their credulity.

If we admit the justice of these remarks, it will induce us to advance a step farther in our investigation, and to ask, whether the imagination may not actually produce a state of the system, which shall constitute a specific disease. It has been observed by medical writers, that a disease has been unusually prevalent at a particular period, when there has appeared no external cause to which this increased prevalence could be reasonably assigned, while at the same time, from certain circumstances, the disease in question has been more than ordinarily the object of attention. That this might be the case with diseases of the nervous system, is sufficiently intelligible, as in these cases the functions of the parts are often much affected, without their experiencing any change in their organization. But it has not been confined to these affections; diseases have been produced in parts that are only remotely connected with the nerves, and where the change must ultimately consist in an altered action of the arterial capillaries, and perhaps of the absorbents. Although in such cases it is necessary to obtain very direct evidence of the fact, and to search in all directions for more obvious causes, yet it does not seem impossible that such a change may be effected upon the corporeal organs, through the medium of the mental emotions.

[1] Of the Imagination, as a cause and a cure of disorders, &c.

CHAPTER XVIII.

OF VOLITION[1], AND THE PASSIONS.

SECT. 1. *Nature of Volition.*

I HAVE frequently had occasion to remark upon the connexion between muscular contraction and the will; on this circumstance is founded the division of muscular motions into the two great classes of voluntary and involuntary. Voluntary motion may be regarded as one of the most important effects produced by the re-action of the nervous system upon the muscles, and as being that power which more immediately connects us with the external world.

Volition, or the act of the mind which constitutes the will[2], is excited by a variety of causes, partly depending upon direct perceptions of pleasure and pain, and partly upon associated feelings; but in all cases volition, where it leads to an active exertion, is preceded by a motive. The mere act of volition, like all the other mental faculties, is directly connected with the brain, while the exercise of volition requires the co-operation of the brain, nerves, and muscles. Whatever volition is formed in the mind, it cannot be carried into effect unless the nerve and the muscle be in a sound state. The manner in which this singular process is accomplished is very much concealed from our view. All that we certainly know is, that the mind forms a volition; this is accompanied with a consciousness of power, and immediately the effect is produced. For example, we will to move the arm in a certain direction, and, provided the nerves that connect the arm with the sensorium commune and the muscles of the part be in a sound state, the arm is immediately moved.

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[1] If we may be allowed, in any case, to consider the mind as possessed of distinct faculties, volition would appear to be the power which is the most essentially different from the other mental operations. These seem all to depend upon a certain combination or relation of ideas to each other, influenced, more or less, by the intervention of perceptions from the impressions of external objects. But in the exercise of volition the process proceeds in the inverse direction; it originates in the mind, is transferred to the brain and nerves, and from these to the muscles or organs of sense. I have referred above to the opinion of Locke respecting the futility, or even impropriety, of attempting to ascribe the mental operations to distinct faculties. He, however, remarks upon the nature of volition, as essentially different from that of thought, and seems disposed to resolve all the mental operations into modifications of these two powers; Essay, b. 2. ch. vi. v. i. p. 104, 5. The same principle forms the basis of Reid's division into the intellectual and active powers; Intell. Powers, p. 67. If we were to indulge in any further speculations on the subject, we might propose the division of the mental operations into three classes, to be referred to perception, volition, and intellect, which, for the convenience of language, might be denominated distinct faculties. Perception consists in the power of receiving impressions from external objects, an operation which must proceed from the extremities of the nervous system towards its centre; volition is the re-action of the mind upon external objects, where the operation is transmitted in a contrary direction; while in intellect, our ideas only are concerned, without the intervention of external objects, and where probably the brain acts as the sole intermedium, without the co-operation of the nerves. I conceive it would not be difficult to arrange all the mental operations as species of these three genera; but it may be questioned, whether such a technical arrangement would throw any light upon the subject, or in any respect advance our knowledge, either of the nature of the mind or of its connexion with the body.

[2] Locke defines volition to be "an act of the mind directing its thought to the production of any action, and thereby exerting its power to produce it;" Essay, b. 2. ch. xxi. § 28. According to Hartley, "The will is that state of mind, which is immediately previous to, and causes, those express acts of memory, fancy, and bodily motion, which are termed voluntary;" On Man, Introd. p. iii. According to Reid, volition is "the determination of the mind

It becomes an interesting object of inquiry, what takes place in this process, what are the intermediate links in the chain of actions between the feeling in the mind and the motion of the limb. The act of volition induces a certain state of the brain; this is in some way propagated through the nerves, these again act upon the muscles in some peculiar manner, and, lastly, the muscular fibres are shortened, and thus move the joint in the required direction. That the will originates in the brain, in the same sense that our other intellectual feelings arise there, we can have no reasonable doubt, although, as forming a part of the mind, we may conclude that something besides the mere modifications of matter is concerned in the operation. There are abundant facts that prove the nerves to be the media through which the will acts, and indeed, according to the view of the subject which has been taken in the former part of this work, we are led to conclude, that the chief use of the nerves which are distributed to the muscles is to place these muscles under the control of the will. But although we may feel no doubt of the reality of these three changes; first, that of the brain; secondly, that of the nerve; and lastly, that of the muscles as induced by the nerves, we are totally ignorant of the nature of the first two, and equally so of the manner in which the three are connected together[1].

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to do or not to do something which we conceive to be in our power;" On the Active Powers, p. 60. Volition, as appears by these definitions, applies both to the physical and to the intellectual functions, but the object of this treatise will lead me to consider it principally as connected with the former class. See the remarks of Dr. Alison, Physiol. p. 170. When treating of the nervous system, I had occasion to notice the opinions of some of the French physiologists on the seat of volition, especially those of Flourens and Bouillard.

[1] Dr. Roget remarks, that in every voluntary action, which ensues on the application of an external agent to an organ of sense, twelve successive processes intervene between the cause and the effect; Bridgewater Treat. v. ii. p. 535.

The only attempt at explanation which deserves to be noticed is that of Hartley, and this perhaps more from the general respectability of the author, than from the merit of the hypothesis itself. He refers all the mechanical changes in the nervous system to vibrations among its particles, and in this way accounts for the permanency of the impression produced by external objects, as well as by the operation of the intellectual powers themselves[1]. Upon this hypothesis, we may remark, that it assumes the position, that the changes in the nervous system are effected by the intervention of motion; but I think it may be asserted, that not one of the characteristics of motion can be recognized in any of these operations, and that not a single circumstance can be adduced, which affords any decisive evidence of its existence.

The only direct argument that has been brought forward, either by Hartley himself, or by any of his followers, is the acknowledged fact, that when an impression has been made upon the nervous system by an external agent, the effect remains for a certain length of time after the cause is withdrawn; that it is then gradually diminished, and that a permanent change is finally produced[2]. This has been conceived to bear an analogy to the vibratory or oscillatory motions between the particles of bodies; but the analogy is at least of very doubtful application, and is not supported by the phenomena. For if we are in any degree to reason upon mechanical principles, we should conceive that a substance capable of such extreme delicacy in its vibratory action, as the medullary matter of the brain and nerves, must be eminently elastic; and that, consequently, when the action ceased, its particles would be restored to their former relative position. And even admitting the hypothesis in its fullest extent, with all its array of propositions and corollaries, I do not perceive that it throws the smallest light upon the nature of the connexion between the different parts of the operation that we have been contemplating. It neither shows how the consciousness of power can affect the brain, how this idea can be conveyed to the muscular fibre, nor how it can cause the muscle to contract.

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[1] On Man; prop. 4. v. i. p. 11, 2. It is not a little curious to observe the confident strain in which even so acute a metaphysician as Cooper speaks of the hypothesis of Hartley. After having resolved all the phenomena of thought into perception, recollection, judgment, and volition, he goes on to say: "Of these the three latter are demonstrably modes of motion. Hartley has proved it; and I again repeat, what I have observed on a former occasion, that it is inexcusable in the present day to attempt the discussion of the phenomena termed mental, without adopting or confuting his system;" Tracts, p. 273.

[2] Hartley on Man, prop. 3..5. v. i. p. 9..34; the whole of his first chapter should, however, be read, in order to obtain a complete view of his hypothesis. See also Priestley's Hartley, Essay 1; and Belsham's Elem. ch. iii. sect. 4. p. 38..44.

This account of the process of voluntary motion must render it evident, that what we will to perform is merely the ultimate effect, because we are unconscious or even ignorant of the train of causes. The exact objects of volition may be classed in two divisions, under the title of immediate and remote; the first consisting principally of the formation of certain vocal and articulate sounds, or certain motions of the joints, as producing voice, speech, and locomotion; the second, of those actions which we conceive to be within our power, but where we think only of the end to be obtained, without attending to the mechanical means. These two may be illustrated by what takes place in acquiring an art or accomplishment. In learning a language, for example, we begin by imitating the pronunciation of the words, and use a direct effort to put the organs of speech in the proper form. By degrees, however, we become familiar with this part of the operation, and think only of the words that are to be employed, or even the meaning that is to be conveyed by them. In learning music, we begin by imitating particular motions of the fingers, but at length the fingers are disregarded, and we only consider what sounds will follow from certain notes, without thinking of the mechanical way in which the notes are produced. Both these kinds of motions, however, may be said to be voluntary, because they are both brought about through the medium of the will, although in the latter case the motion is not the direct object of volition.

I have stated that a consciousness of power enters into our feelings of volition, and we must inquire in what this immediately consists, or in what way our sense of power is exercised[1]. The power which attends our volitions is absolutely directed to the contraction of certain muscles, but these are not the objects of our will, because we are frequently unconscious of the contractions. When we wish to effect a particular motion of a muscle, we induce a certain state of feeling, which we know by experience has been previously associated with the same muscular action. This feeling we appear to be able to repeat at pleasure, and to it succeeds the desired motion; our idea therefore of power consists in the recollection of the feelings which accompany our motions. But here, as on so many former occasions, although we are able to trace back certain successive steps in the order of actions, we see no connexion between them, and are quite at a loss to determine why they are united by the relation of cause and effect.

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[1] Reid has offered some strictures upon the account of power which is given by Locke; On the Active Powers, ch. ii. p. 22 et seq. Many of the remarks I conceive to be just, but I apprehend that the more important error into which this acute philosopher has fallen, consists in his considering power to be represented by a simple idea. The objections of Hume against the existence of the idea of power depend, I think, principally upon his supposing that it must be a simple idea; Inquiry, sect. 7. Essays, v. ii. p. 77 et seq.

With respect to the nature of the power of volition, or the capacity which the mind possesses of producing at pleasure certain changes both physical and intellectual, we are altogether unable to refer it to any more general principle. It may be regarded as the most completely mysterious operation to which our frame is incident, and as one which, in all its parts, is the most remote from any of the effects which we ordinarily ascribe to matter. And even were we to admit the material hypothesis, and to take it in connexion with the Hartleian doctrine of vibration, still we gain no actual information upon the subject. We merely assume a series of positions, of none of which we have any direct evidence, and which possess no more than a verbal connexion with each other, without any actual analogy or resemblance. Upon the same principle, therefore, that I have acted on former occasions, I object to all such attempts at explanation, as being not merely futile, but decidedly objectionable.

The second class of muscular motions are the involuntary, or those which are produced by something acting upon the muscle, independently of the will. These have been styled by Hartley automatic [1], but the term is not appropriate, for we suppose the existence of an external cause as much in these, as in voluntary motions; the essential distinction between them consists in the relation which they bear to volition. Among the involuntary motions may be classed the contraction of the heart and perhaps of the diaphragm [2]: almost all the muscular fibres, that are spread over expanded membranes, act independently of the will, as well as those that are attached to vessels of all descriptions, such as the capillary arteries and the absorbents. All muscles are subject to involuntary motions in certain diseased states of the body, and there are some which partake of the two modes of action; but, for the most part, each kind of motion belongs exclusively to its appropriate muscle.

Before volition can be exerted, it is necessary that a motive exist in the mind, and hence it follows that, strictly speaking, there can be no voluntary motions in new-born infants. One of the first actions that is performed after birth is swallowing, but in this case there can be no exercise of volition, as there is no conception of the nature of the action, and in short no mental feeling of any kind in existence.

This I consider as a clear case of the operation of instinct; where a series of actions is performed, so as to accomplish an important object in the animal œconomy, which is attended with consciousness, and is effected by the muscles that are under the control of the will, but where the individual is ignorant of the end in view, and employs no mental process for its production. But in the adult swallowing is a voluntary act, although generally of the remote species; however, it appears that motions, which are involuntary in the first instance, become afterwards voluntary. Hartley ascribes this change to association. He supposes that when an involuntary action has been frequently performed, we connect or associate together the idea of the motion with the sensation which precedes it, and that, having it in our power to re-produce our sensations at pleasure, we learn to re-produce the motions connected with these feelings, whenever we conceive them to be necessary for our enjoyment or existence [1]. In this way it is that Hartley supposes we acquire the use of speech. The infant is led to utter a variety of sounds in consequence of direct impressions made upon the organs. These sounds become associated, from various causes, with other perceptions; and, according to the usual operation of associated feelings, the sounds call up the ideas that are connected with them.

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[1] He says that he calls these motions "automatic," from their resemblance to machines, whose principle of motion is in themselves; Introd. p. iii. But this, it may be remarked, is merely a technical resemblance, there being no real similarity between the principle of action in the two cases. Parry does not appear to have distinguished between voluntary and involuntary motions with his usual accuracy, nor to have correctly marked their relation to each other; Pathol. § 608..0.

[2] This remark applies only to the ordinary actions of this organ.

All muscular motions are, therefore, in the first instance, involuntary; some of them continue so during life, while there are others over which we gradually acquire a voluntary power. It may be asked, what is the cause of this difference? Is there any circumstance in the structure or organization of the muscles of involuntary motion different from that of the muscles which become subject to the will? We might previously suppose that some difference does exist, because we find, with a few exceptions, that the corresponding muscles in different individuals agree in the relation which they bear to the will. In a sound state of the body the muscles subservient to speech and locomotion are completely voluntary, the muscles that belong to the circulation are involuntary, while there are others that are of an intermediate nature. Now this difference appears to depend in great measure, if not altogether, upon the source whence the muscles derive their nerves. The nerves which place a muscle under the control of the will are derived immediately from some part of the brain or spinal cord, and we may generally observe a proportion between the degree of voluntary motion and the quantity of nerves with which a part is furnished. The other office of the nerves, that by which all the parts of the system are connected together into one whole, and endowed with mere sensation, seems to depend more upon the nerves that proceed from the ganglia, and it is probably to the ganglia that the perceptions of the internal organs are always, in the first instance, referred.

We may then conclude, that there is something in the original structure of the part which places it under the control of the will. It appears, however, that the voluntary motions are at first involuntary; by degrees the will acquires its power over them, and that they become again involuntary, at least the connexion is of that kind which I have named remotely voluntary, and which exists without our consciousness: Hartley calls these motions secondarily automatic [1]. With respect to those muscles over which the will never acquires any power, we have reason to suppose that, under ordinary circumstances, they receive the impression of their appropriate stimuli upon the fibre itself, without the intervention of the nerve. When this is clearly ascertained to be the case, it constitutes an obvious point of discrimination between the voluntary and the involuntary muscles, and affords an evident reason why the latter must, at all times, continue to be involuntary.

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[1] On Man, v. i. p. 107..9.

SECT. 2. *Account of the Passions.*

Among the powers which serve as the connecting media between the physical and the intellectual parts of our frame are the various passions or affections. The passions are generally regarded as exclusively belonging to the department of morals or metaphysics; yet it will appear, upon examination, that they are nearly related to our corporeal organization; that they, in a considerable degree, depend upon it, and have a material influence over it.

The impressions received by the senses and conveyed to the brain, the common centre of all our perceptions, uniting there with the ideas that had been previously acquired by the understanding, may be regarded as the origin of our passions. It would appear, therefore, that in all cases they may be ultimately referred to the wish to obtain some good, or to avoid some evil, either real or supposed, and may consequently be regarded as modes or modifications of volition [2]. The organs of sense will be the proper inlets of the passions; but the external senses themselves are only affected by them in a remote or secondary manner, while we shall find that certain of the physical functions are placed more immediately under their influence[1]. A perception received by the eye or the ear, combined with some previous idea of danger, excites the passion of fear. But the effects of fear are especially manifested upon the heart and arteries; the pulse becomes irregular, throbbing violently or being nearly suspended, according to the degree of the emotion or the mental feeling immediately connected with it[2]. The extent to which this action may proceed is absolutely indefinite; we have numerous examples, in which the effects produced upon the circulation by mental excitement have remained during life, and to such an excess has this excitement been occasionally carried, as to have caused instant dissolution.

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[1] On Man, v. i. p. 108, 9. He considers the act of swallowing to be an example of this transition of automatic into voluntary and of voluntary into secondarily automatic motions; p. 117. Prof. Stewart endeavours to prove, that motions which are once voluntary always remain so, and that our not being conscious of the act of volition is owing merely to our not attending to them; Elem. ch. ii. v. i. p. 112 et seq. It appears to me to be rather a question of words than an actual difference in the conception of the facts; but perhaps the term remotely voluntary, which I have adopted, may express the fact, without involving any controversy respecting theory.

[2] Locke, without entering into minute details, merely considers the passions generally, "as modes of pleasure and pain;" Essay, book 2. ch. 21. v. i. p. 215..0. Hartley still farther illustrates this principle, and also shows in how great a degree they are influenced by association; On Man, prop. 89. v. i. p. 368..373. Hume's "Dissertation on the Passions" contains many ingenious observations on the mode in which they are called into operation, the connexion which they have with each other, and the relation which they bear to our ideas, both perceptive and intellectual; Essays, v. ii. p. 184..221.

It is not the circulation alone which is affected by the passions, nor do they act merely by increasing or diminishing the vital energy of the whole system. Particular organs seem to feel the effect of particular mental emotions; fear and joy act upon the heart, surprise appears more especially to affect the respiration, and grief the digestive organs. We shall find a clear indication of this connexion in our common forms of speech, which must have been derived from observation and generally recognized, before they could have become incorporated with our language. The paleness of fear, the breathlessness of surprise, and the bowels of compassion, are phrases sanctioned by the custom of different ages and nations[3].

It was probably from dwelling upon considerations of this description, that Bichat was led to form what appears so

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A considerable portion of Reid's treatise "On the Active Powers," is devoted to the subject of the passions; see particularly, Essay 3, ch. 6. p. 180 et seq.; it displays the usual excellencies and defects of his writings: the style is clear, and the illustrations generally appropriate; but it is diffuse, and he manifests a zeal and pertinacity for his peculiar doctrines, which not unfrequently degenerate into uncharitableness and prejudice. Cogan's "Philosophical Treatise" contains many useful remarks on the distinction between the different passions, although, I think, in some cases, rather too technical and refined.

[1] Grove defines a passion to be "any emotion of the soul (mind) which affects the body, and is affected by it." Works, v. iv. p. 228. Cogan devotes a section to the "medical influence of the passions," in which he details their effects upon the physical functions generally; Treatise, part 2. c. iii. sect. 1. p. 278 et seq.

[2] In Sir C. Bell's elegant treatise "On the Anatomy and Philosophy of Expression," we meet with many valuable observations on what may be termed the physiology of the passions, or the mode in which certain organs of the body serve to express certain mental emotions. It would appear that the nerves which he styles respiratory are the primary agents, and that they transmit the impressions made upon the nervous system to the muscles of the face and the neighbouring parts, by means of which the ultimate effect is produced; Essays, No. 1..6. passim.

[3] Parry mentions many instances where certain mental emotions produce a peculiar effect upon certain secretions; Pathol. § 666.

singular a conclusion, that the organic functions are the primary seat or origin of the passions[1]. Nothing, however, appears to me more clear, than that the ordinary conception on this subject is the correct one; that the passions are, in the first instance, mental operations, and of course connected, like other operations of this kind, with the nervous system. They originate, according to circumstances, either from impressions made by external objects, from certain internal feelings, or from ideas; all of these distinct sources being more or less combined and connected together; but the passions, when formed, have their seat in the nervous system, and through this it is that they exert their influence over the various parts of our frame.

This view of the subject will lead us to the conclusion, that the passions are, to a certain extent, innate, or that different individuals, placed under the same circumstances, will exhibit different passions, depending upon a difference in their physical constitution. This difference may be referred either to a difference in the organization of the individual, by which certain organs are disposed to receive particular impressions in preference to others, or merely to a greater or less delicacy of the nervous system generally, by which the same impressions are more or less acutely perceived. According to the degree in which the passions depend upon physical causes, in the same proportion must they be regarded as under the influence of the corporeal organization; but, I conceive, that there is none of them in which this combination of the two sets of causes cannot be traced.

The opposite doctrine has, indeed, been defended by some ingenious metaphysicians, and particularly by Helvetius. He contends, that every individual is originally formed with an equal capacity for receiving the impression of external objects or of internal sensations; but that, from the effect of education, or of various incidental circumstances, we acquire the power of attending more or less minutely to our perceptions; and that, from the same cause, they make a greater or less impression upon the mind, and consequently become associated, in various ways, and with various degrees of force, with our mental powers[2]. Hence, according to this hypothesis, the varieties which we observe in the passions of individuals may be referred, in a great measure, to the accuracy with which they attend to and recollect their sensations; this circumstance being itself, in the first instance, the result of accident or of some extraneous cause, apparently slight, and afterwards entirely overlooked.

But I apprehend that this opinion is inconsistent, no less with actual fact than with correct hypothesis. Those who have been much in the habit of observing children, can scarcely have failed to notice a difference in their passions and dispositions, showing itself at the earliest period in which they give any indication of perception or intellect, where there has been the greatest similarity in the modes of life and in the acquired habits. No one can doubt that there is an original difference in the form of the body, in the strength of the limbs, in their capacity for action, and in the perfection of the organs of sense; yet these are all subject to be much modified and affected by external causes. In the same way, it is reasonable to suppose, that there is a provision in our frame for an original difference in the mental powers, which is either fostered or counteracted by the force of education and the general habits of life. What we term disposition or character, may be regarded as a compound of the passions and the understanding. The latter we conceive to be composed of the ideas which the mind originally acquires from external objects; and if we suppose the former to depend, in a considerable degree, upon original constitution, we shall be at no loss to account for the actual condition of human nature, exhibiting strong marks of a native bias, yet influenced in various modes by accidental impressions.

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[1] Sur la Vie et la Mort, par. 1. art. 6. § 1..3. p. 36..50.

[2] This principle forms the leading subject of his treatise "De l'Homme:" the mode in which he reasons may be learned by perusing the first eight chapters of his first section; Œuvres, t. iii. p. 24, et seq.

Both metaphysicians and physiologists have been in the habit of arranging the passions into two great divisions, under the denomination of exciting and depressing, according as they are supposed to operate in stimulating or depressing the vital powers[1]. Of the organic functions, the one which is the most affected is the circulation, and perhaps it is on this alone that we can conceive their action to be primarily exerted. No one can doubt that anger increases the action of the heart, or that in fear the blood is not transferred with the usual force through the different parts of the sanguiferous system. For the most part, however, it would appear more probable that the exciting and depressing effects of the passions are produced through the intervention of the nervous than of the sanguiferous system; and that, according to the nature of the action upon this part of our frame, we are to look for the effects which respectively produce the two classes of mental emotions.

But, although we may conclude that the passions act in the first instance upon the nervous system, and secondarily upon the circulation; yet there are various circumstances which lead us to conclude, that the difference in their action is something more than that of degree, and that, as I remarked above, particular organs are specifically affected by particular passions. While, therefore, we perceive, on the one hand, that organization materially influences the passions, so it would appear, on the contrary, that the passions affect the functions of the organs, and it is not unreasonable to conclude, that they may ultimately affect the structure of the organs themselves. If a violent emotion of grief, or the indulgence of a fit of anger produces a temporary derangement of the stomach, we may suppose that the long continued exercise of these feelings, or their frequent recurrence, may so alter the actions of the parts, as permanently to injure their functions, and finally to affect their structure. Here again we find the testimony of common observation in favour of our speculation. We may conclude, that proverbial aphorisms are, for the most part, founded in truth, and we shall perceive it confirmed in the connexion which subsists between a cheerful disposition and a tendency to fatness. In the same manner, expressions which are generally regarded as entirely metaphorical, will often be found to originate in a simple matter of fact. A sour disposition is probably at some times the effect, and at other times the cause, of an acid state of the stomach; and a man, who is said to possess a warm heart, will often indicate a higher degree of animal temperature, than one who is characterized by coldness of disposition and moderation of feeling. In all such cases it is difficult to discriminate between the effects of external circumstances and of internal structure and organization; but the facts appear to be most easily and satisfactorily explained, by supposing that they depend upon the joint operation of both these causes.

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[1] This division of the passions into exciting and depressing does not correspond to the two great causes to which the origin of the passions has been referred,—the desire of procuring pleasure and of avoiding pain; this is more connected with moral considerations, while the former principally regards their physiological effects.

CHAPTER XIX.

OF CRANIOSCOPY AND PHYSIOGNOMY.

The view which I have taken of the connexion that subsists between the physical structure of the nervous system and the mental faculties, naturally brings me to a subject which has of late attracted a considerable degree of attention among anatomists and physiologists—the dependence of the character and disposition upon the peculiar shape and organization of the brain. Certain facts, which seemed to favour this opinion, had been long noticed; persons of observation were in the habit of associating the idea of superior intellect with a capacious and prominent forehead, while the contrary form was equally conceived to indicate a deficiency of the mental powers. The inspection of the skulls of the insane, and still more of idiots, seemed to prove, that the perversion or deficiency of their faculties was connected with a peculiar form of the head[1], and it was thought that a kind of analogy might be traced through the lower animals, which favoured the same conclusion. When the sculptors of antiquity formed the statues of their gods or heroes, to which they were desirous of imparting the character of high intelligence, they endeavoured to accomplish this by giving a peculiar form to the head; and many expressions, employed in the languages of various ages and nations, show that an opinion of this kind has been commonly adopted. But it was embraced in this general way rather as a speculation, countenanced by a few casual observations, than as the correct deduction of a number of well ascertained facts, which were capable of acquiring a philosophical character, and of forming a distinct department of philosophical science.

Sect. 1. *Nature and Object of Cranioscopy.*

The subject was first placed in this point of view by Drs. Gall and Spurzheim, who, in consequence of their accurate dissection of the brain, and their mode of separating its different parts from each other, were led to conjecture, that these parts were appropriated to distinct mental faculties. Dr. Gall had previously devoted himself to an examination of the natural indications of character which are exhibited by individuals, and had convinced himself, that the varieties which we observe in this respect are to be regarded as, in a great measure, innate. Proceeding upon this principle, and assuming that the brain is the organ through the intervention of which the mental faculties are exercised, he conceived it to be not improbable, that a physical difference in the form and structure of the brain might be detected, corresponding to these differences in the native character and dispositions. Partly, as it would appear, from his idea of the anatomical structure of the brain, in what regards the relation of its different parts to each other, and partly from a pre-conceived hypothesis, he fixed upon the external convolutions of the cerebrum and cerebellum, as the respective seats of the individual faculties; and proceeding upon the supposition, that the size of an organ must be a measure of the capacity which it possesses of exercising its appropriate functions, he deduced the principle, which lies at the foundation of the new doctrine, that the character and disposition are necessarily connected with the respective size of the convolutions of the brain. It is farther assumed, that the size of the convolutions may be ascertained by an examination of the form of the cranium, the peculiar shape of which, as it differs in different individuals of the same species, is conceived to be, in a great measure, determined by that of the brain; as we find, in other organs of the body, that the hard parts are frequently moulded by the growth of the softer parts that are contiguous to them[1]. Hence we derive the practice of cranioscopy[2], or the art by which we endeavour to discover the nature and extent of the mental faculties, by ascertaining the form of the skull.

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[1] Lavater gives us the outline of the features of a number of idiots, which, it will be admitted, are very characteristic of the defective state of their mental faculties; Essays, by Holcroft, v. ii. p. 280. See also the plates in Pinel, Sur l'Aliénation Mentale; and in Gall and Spurzheim's Anatomy, No. 19, 20.

The arguments which have been urged in favour of the science of cranioscopy are partly anatomical and partly physiological. In the first place it is said, that the brain exhibits a very elaborate structure, and a very complicated organization, and it is therefore reasonable to conclude, that its different parts must be subservient to the exercise of different functions. Secondly, both metaphysicians and physiologists have been in the habit of referring all the impressions which we receive through the intervention of the nerves to some central part of the brain, but the great diversity of opinion which exists respecting the part which ought to be regarded as this common centre, affords us at least a strong presumption of its non-existence, while, on the contrary, if we suppose that there actually is such a central spot, we are at a loss to assign any use to the remainder of the brain. Thirdly, we are in possession of a number of observations upon the partial loss of the mental faculties, in consequence of disease or injury of the brain; and although we are not able to trace out the connexion between the situation of the injury received and the defect of the mental powers, yet it favours the opinion that these faculties are distributed over the different parts of which the brain is constituted. Fourthly, the analogy of the nerves that are connected with the external organs of sense is adduced by the cranioscopists in favour of their doctrine. Each of these nerves, in conveying their respective impressions, must exercise a different office, and in the same way, the different convolutions of the brain are supposed to be the organs of the respective mental functions. Fifthly, it is argued that the state of the brain, in regard to its perfection and full development, corresponds to the state of the mental faculties at the different periods of life, and also to their degree of perfection among the inferior animals, so as to indicate a necessary connexion between these circumstances. Sixthly, the brains of different individuals actually differ in the proportionate form and size of their parts, and it is therefore reasonable to presume, that this may be the cause of the difference which is admitted to exist in the faculties of different individuals. Seventhly, the exercise of the mental powers, like those of the physical functions, is attended with fatigue; but it is found by experience that the fatigue only extends to that particular power which has been exercised; it may, therefore, be presumed that its action is confined to a certain portion of the brain only. Eighthly, proceeding upon the principle, that the dispositions and mental faculties are, to a certain extent, innate; and, observing that they exist in different individuals in different proportions, it follows that they must be attached to different organs.

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[1] Desmoulins, however, remarks, that the internal contour of the brain is frequently not parallel to the external surface; Anat. des. Syst. Nerv. p. 599.

[2] I may remark, that this subject, which was originally brought before the public under the appellation of cranioscopy, has been lately styled phrenology; but as the first of these terms appears to me the most appropriate and descriptive, I shall continue to employ it.

The above appears to me to exhibit a fair statement of the nature of the arguments which have been employed, to prove the antecedent probability of the doctrine of cranioscopy. But its advocates are aware, that its merits must principally rest upon the degree in which it is found to correspond with well ascertained facts and correct observation, and with the power which it actually affords us of acquiring a knowledge of the character and disposition of individuals by an examination of the skull. It is therefore by an appeal to experience, that the supporters of cranioscopy, and Dr. Spurzheim in particular, attempt to establish their opinion,and they have accordingly brought forward a number of facts of this description, which are supposed to form a sufficiently firm basis for their system. They consist of the results which were obtained by examining the heads of the various individuals of all ages, ranks and conditions, minutely noticing the deviations from the average form, especially with regard to the size and situation of the eminences or protuberances which they exhibited. The examination has also been extended to the inferior animals, and the same principles have been applied to their skulls, both as to what respects their general form, and the proportionate size of their individual parts, whether indicating a generic or an individual difference.

In estimating the value of these arguments, I shall arrange them in two divisions, as they relate to general considerations of probability, or as they depend more upon particular facts. And with respect to the first point, I think it will be admitted that there is none of them which possesses more than an indirect application to the question under discussion. Admitting that the perfect organization of the brain is a necessary intermedium for the exercise of the mental powers, we may conclude, that every part of this organ must have a necessary connexion with the exercise of these powers, as every part of the eye and the ear has a reference to the production of vision and of sound. In consequence of our knowledge of the physical laws of light and of the undulations of the air, we are enabled to trace out the mode in which the several parts of the eye and of the ear co-operate to produce the ultimate effect. Had we the same knowledge of the mode in which the mind operates upon the brain, we should probably have it in our power to detect the same kind of co-operation of all its parts and structures to the production of perception and thought. But on this point we are in total ignorance, and therefore, although we may go so far as to assert, that a perfect brain, in a certain sense, is essential to a perfect mind, we are unable to say in what way it is so.

The only anatomical argument which is of so tangible a nature as to allow of any thing approaching to direct deduction, is derived from a consideration of the degree in which an injury of the brain produces a corresponding injury of the mental powers. Upon this point I have already stated my opinion, and I have only to add, that while the connexion is not of that nature which indicates the relation of cause and effect, so I should be still less disposed to allow, that the facts which we possess are of that distinct and direct nature, which can enable us to connect particular injuries of the brain with corresponding injuries of particular faculties.

The position, that the size of an organ is an indication of the degree of its power or capacity, a position which may be regarded as almost the fundamental principle on which the whole doctrine rests, is in direct contradiction to fact. To revert to the case of the eye; it may be asserted that the perfection of this organ, either when considered with respect to the different species of animals, or to the different individuals of the same species, does not bear the least relation to its size, but depends entirely upon the nature of its organization, and, except in those cases where the exercise of an organ is connected with mechanical force, as in muscular contraction, bulk has no relation to the perfection of a part[1].

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[1] Desmoulins, who admits that the doctrine of Gall and Spurzheim is plau-

The analogy that has been so much insisted upon, of the power which the several organs of the body possess of exercising their appropriate functions, will, I apprehend, be found upon examination to be inapplicable to the case of the brain. We perceive that the eye is especially adapted for receiving the impressions of sight, and we can explain the mode in which it acts upon the rays of light. We know, on the other hand, that the ear is not adapted for receiving the impressions of vision, nor of being affected by the rays of light, and we hence conclude, that the ear exercises a different faculty from the eye. But as far as the argument would apply to the brain, we must consider it as a single organ, although composed of various parts, and the different mental powers as modes or species of the same faculty. And proceeding upon the same principles in this case, as with respect to the organs of sense, we should consider the brain, taken in the whole extent as the organ of mind, confessing our ignorance of the particular use of its minute parts, or of the manner in which its powers are affected or modified.

And even were it proved, as a general principle, that distinct parts of the brain were appropriated to distinct mental functions, we may still be permitted to doubt, whether the cranioscopists have been fortunate in their division and appropriation of the functions which are supposed to possess these distinct localities. If we consider the subject theoretically, we might presume, that there would be a separate organ corresponding to each of the external senses, as the impressions are themselves distinct in their nature, and might be supposed to require some different modification of the nervous matter for their perception. And again, with respect to the intellectual powers, there are some which appear so distinct from the others, that we might apply to them the same mode of reasoning, and suppose it probable that they might possess their appropriate organs. The faculty of memory might be supposed to require a different modification of the nervous power from that of the imagination; and this again from that of abstraction or volition. But we do not observe any classification or division of this kind in the faculties that are enumerated by Dr. Spurzheim or his disciples. Some of them are complex feelings, resulting from the union of primary perceptions with ideas; others appear to be a combination of ideas only; some may be regarded as the obvious result of association; and others again as the effect of association operating through the intervention of education, or of the accidental circumstances in which the individual has been placed[1].

sible, thinks that the arguments brought forward by them are inconclusive, because they are derived from the size and external form of the cranium.

[1] In Dr. Spurzheim's "Anatomy of the Brain," we have the situation of the different organs delineated in pl. 5, 6, 7; the number enumerated is thirty-five, and their denominations are as follows:—Amativeness, philoprogeni-

And, with respect to what may be regarded as the practical application of the art or science of cranioscopy, it may be objected, that the convolutions of the cerebrum are not what one should expect to be the seat of the ultimate operations of the organ. They are not the part in which we behold that elaborate and complicated structure, the existence of which has been supposed to form so powerful an argument in favour of the doctrine, while this view of the subject still leaves unexplained the uses of the more minutely organized parts, that are situated in the interior of the brain. And, farther, were we to admit the position, that the convolutions of the brain are the seat of the mental faculties, it would be necessary to establish two points, before we could employ them as indications of these respective powers; first, that the convolutions of all brains occupy corresponding situations with respect to the cranium, or are exactly opposed to the same portion of its internal surface[1]; and, secondly, that the cranium is, in all its parts, of a uniform thickness, so as to afford us, by its external surface, the means of acquiring an accurate knowledge of the convolutions that are subjoined to it.

But, although I conceive that the above considerations are not without their weight, and that, upon an impartial review of the subject, they are such as may at least counteract the hypothetical arguments that have been advanced in favour of cranioscopy, I am disposed to agree, with what appears to be the principle of the most intelligent advocates of the doctrine, that the question can only be decided by an appeal to facts. These facts are of two kinds, although exactly coinciding in their object. We must obtain skulls that are marked by some peculiarity of form and shape, and must then endeavour to learn what was the natural character of the subject; or we may take the cases of those who have shown some decided peculiarity of disposition and character, and may examine the figure of their skulls. A sufficient number of these observations, carefully made and impartially recorded, cannot fail to decide the question, whether there be any ground for the doctrine of the appropriation of the different parts of the brain to distinct faculties, and, more particularly, whether we have it in our power to ascertain their seat by an external examination of the cranium.

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tiveness, inhabitiveness, adhesiveness, combativeness, destructiveness, secretiveness, acquisitiveness, constructiveness, self-esteem, love of approbation, cautiousness, benevolence, veneration, firmness, consciousness, life, marvellousness, ideality, mirthfulness, imitation, individuality, configuration, size, weight and resistance, colouring, locality, calculation, order, eventuality, time, melody, language, comparison, causality. This work contains the last account of Dr. Spurzheim's peculiar views respecting the structure of the brain, the relation of its different parts to each other, and the mode in which they are the most advantageously detached and exposed to view. It is accompanied by a number of expressive engravings.

[1] We are informed by Dr. Craigie, in his valuable manual of "Pathological Anatomy," that this is certainly not the case; p. 306, 7.

On this point I must give it as the conviction of my mind, that the facts hitherto adduced are altogether inadequate to the end proposed, that they are frequently of doubtful authority and of incorrect application, and that nothing but the love of novelty, and the eagerness with which the mind embraces whatever promises to open a new avenue to the acquisition of knowledge, could have led men of talents and information to place any confidence in them[1].

In offering thus freely my objections to the doctrine of cranioscopy, I have thought it proper to abstain from certain topics, which have been generally urged against it, since I consider them to be, in a great measure, the offspring of bigotry and illiberality. If, on the one hand, its advocates have been hasty and credulous, it must be admitted, on the other hand, that its opponents have too frequently been harsh and uncandid. But its principles are too widely disseminated, and have taken too deep root in the public mind, to be repressed by mere authority or counteracted by ridicule; they must be put to the test of experiment, and by this standard alone will their merits be ultimately appreciated[2].

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[1] I conceive that the general result of the investigation which the subject has experienced by various physiologists and anatomists of the highest authority is decidedly unfavourable to the doctrine of Cranioscopy. In confirmation of this opinion I may refer to the observations of Dr. Prichard, and the various authorities which he adduces, in the article "Temperament", in the Cyclopædia of Medicine, p. 168 et seq.; and still more to the "supplementary note", in his treatise on Insanity, a work equally remarkable for the extent of the information which it conveys, and for the candid spirit which it manifests. See also Dr. Alison's remarks, Physiol. p. 308..0; and Dr. Milligan's, in the notes to his translation of Magendie, p. 555 et seq. The observations of Sir W. Hamilton, prefixed to Dr. Monro's late work on the brain, are, on various points, adverse to the doctrine of cranioscopy.

[2] I have subjoined a list of some of the principal works that have appeared in illustration of the doctrine, commencing with those that were published by Drs. Gall and Spurzheim themselves. Gall, Cranologie; Gall et Spurzheim, Recherches sur le Système Nerveux; Gall et Spurzheim, Anatomie et Physiologie du Système Nerveux; the latter accompanied by a series of beautiful engravings; the Physiognomical System of Drs. Gall and Spurzheim; Spurzheim's Examination of the Objections made to his Doctrine; Spurzheim, Essai Philosophique sur la Nature Morale et Intellectuelle de l'Homme; Spurzheim's Anatomy of the Brain, by Willis. One of the earliest accounts of the doctrine is in the Edin. Med. Journ. v. ii. p. 354 et seq.; this article contains a critique on various treatises by Bischoffe, Walter, and Hufeland; Forster's Sketch of Gall and Spurzheim's System; Combe's Essays on Phrenology; this work, of which several successive editions have appeared, may be regarded as the most elaborate and spirited defence of the system; M'Kenzie's Illustrations of Phrenology. Although a number of strictures have, at various times, been published on the doctrine of cranioscopy, they have appeared in the form of detached essays or articles in the periodical journals; and it must be acknowledged, that they have been more characterized by the brilliancy, or perhaps flippancy, of their wit, than by the soundness of their arguments: it would seem, indeed, that the writers did not regard it as a subject for serious consideration. I must, however, except from this censure the article "Cranioscopy," in the Suppl. to the Edin. Encyc. v. iii. p. 419 et seq, by Dr. Roget, which is truly characteristic of the cultivated and candid mind of its author. I may also refer to the art. "Cranio-

SECT. 2. *Nature and Object of Physiognomy.*

Nearly allied to the science of cranioscopy is that of physiognomy, but differing from it in this respect, that the former professes to judge of the character by the shape of the head, while the latter principally makes use of the form of the features and the general aspect of the countenance. Physiognomy is a science of very early date, and was strongly insisted on by many of the ancients, but among the moderns it was little cultivated by men of talents, until the publication of the work of Lavater. It must be admitted, as a matter of fact, that there are few persons of any reflection, or of any knowledge of human nature, who do not almost involuntarily exercise their judgment on the physiognomy of every new face that is presented to them. Without regard to any physiological speculation or controverted opinions, we, as it were, instinctively attach the idea of a certain disposition to a certain countenance, and regard one set of features as an index of wit and another of stupidity. Nor does this idea want the support of plausible hypothesis. The great instruments of expressing the human passions and feelings are the muscles of the face, and when any passion is strongly marked and frequently repeated, the muscles acquire a tendency to maintain this position even when the corresponding feeling ceases to exist. And farther, by the frequent and powerful contraction of certain muscles, the shape of the neighbouring parts may be affected, the tendons may be permanently extended or contracted, and even the bones of the face may be somewhat altered in their form. The science of physiognomy affords indeed much scope for fancy, and it must be acknowledged, that the peculiar genius of Lavater was not the best adapted to reduce it to the strict rules of induction. His character was marked by enthusiasm rather than by judgment, and although he was very assiduous in the collection of facts, he was deficient in the power of arranging them, and drawing from them any general principles [1]. He informs us, that in the prosecution of his inquiry, he was influenced by a kind of mystical feeling, which he is unable to describe, and in the formation of his system he constantly appeals to a species of instinctive impression, rather than to any principles of correct reasoning [2]. The basis of his hypothesis, if it may be so called, rests

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metry" and "Cranioscopy", in Brewster's Encyc. by Dr. Leach. Dr. Gordon's work on the structure of the brain, although written in a strain of unnecessary and injudicious acrimony, contains many acute remarks and valid objections.

[1] The biographical memoir of Lavater, which is prefixed to Holcroft's translation of his Essays, and which is principally taken from the account of his son-in-law, Gessner, shows him to have been enthusiastic, credulous, zealous, and sincere.

[2] See particularly § 2, entitled "A Word concerning the Author;" also § 5, "Of the Truth of Physiognomy;" also v. ii. § 3, p. 14 et seq. in Holcroft's Trans.

upon a fanciful division of the face into three regions, the upper part being that of the intellectual life, the middle of the moral, and the lower part of the animal life; these are supposed to be analogous to the head, chest, and abdomen, and are respectively the seats of three corresponding classes of faculties. He frequently appeals to the common experience of mankind in proof of the truth of his doctrine, and he maintains that no one who does not possess what he terms "physiognomical sensation" can become an adept in his art. "Whoever," he says, "does not discover in Haller the energetic contemplative look and most refined taste, the deep reasoner in Locke, and the witty satirist in Voltaire, even at the first glance, can never become a physiognomist."[1]

The positions which he labours to prove, as the foundation of the science, are, that "there is a certain correspondence of internal power and sensation with external form and figure," that every part of the face is to be regarded as the organ of a peculiar and appropriate sensation or passion, that it is by studying the lineaments of the countenance, and the changes which they experience, as depending upon the passions and mental emotions, that we are to obtain a knowledge of the character and disposition of the individual, and that, for this purpose, we must compare the shape and relation of the different parts of the countenance with the particular traits of his character. The basis of the forehead he seems to regard as the part of the head which gives the most correct indication of the intellectual powers, but he conceives that the general form of the lower part of the face, as well as the outline of the skull[2], may assist us in our examination. Although, as we have seen, he rests his doctrine so much upon an appeal to general experience and to popular feeling, yet he enters into a minute detail respecting the form of all the different parts of the face, the forehead, the eyes, the eyebrows, the nose, mouth, lips, teeth, and chin; and endeavours to point out the relation which they ought to bear to each other and to the whole countenance. Considering Lavater's work as a great collection of features and countenances, it may be styled a valuable repository of facts[3]; but every one must perceive that his inferences are frequently not sanctioned by the premises, and that his judgment is often warped by prejudice.

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[1] Essays, by Holcroft, v. i. p. 118.

[2] V. ii. p. 205..241. He devotes a considerable degree of attention to the form of the skull, and indicates the mode in which we are enabled to judge by its means of the nature of the character. His remarks are accompanied by a number of outline drawings, but, as is commonly the case, without giving any specific rules for their application. His observations are directed to the general form of the bones of the head and face, and we find nothing in them which relates to the protuberances of the different parts of the skull, which forms the basis of Dr. Gall's system.

[3] Independently of any literary merit, Hunter's translation of Lavater's Essays, embellished by Holloway's engravings, constitutes a beautiful specimen of English art.

The object, whether real or imaginary, of the sciences of cranioscopy and physiognomy is to distinguish between the mental faculties or dispositions of different individuals. Whatever may be our opinion respecting the origin of these differences, whether innate or acquired, and whatever may be our means of ascertaining them, no one can doubt of their existence, even at a very early period of life. What may be called the mechanism of the human mind (an expression which is employed without intending to convey any theory respecting the ultimate cause of the intellectual phenomena) ought to form a very principal object of attention with the moralist and the public instructor, and more particularly with those engaged in the education of youth.

CHAPTER XX.

OF VARIETIES AND TEMPERAMENTS.

THE physical part of the human frame exhibits no less decisive marks of original differences in its organization than the mental. When these differences consist in obvious external characters, which attach to whole nations, or to large communities, they are called varieties; when they belong to a certain number of individuals, and are more connected with internal constitution, they are styled temperaments; and when the peculiarities exist in one individual only, idiosyncracies.

SECT. 1. *Of the Varieties of the Human Species.*

To determine the number of varieties into which the human race ought to be arranged, and to point out the precise features by which each of them is characterised, may be conceived to belong more properly to the province of natural history; but the cause of these varieties is a subject which strictly falls under the cognizance of the physiologist, and upon which I shall therefore proceed to offer a few observations.

And, in the first place, it will be proper to point out some of the more remarkable circumstances by which man is distinguished from all other animals, as we shall by this means be better able to appreciate the nature and extent of the differences between the different tribes of the human race. These circumstances are arranged by Blumenbach under the five heads of external form, internal organization, functions, mental qualities, and diseases. Among the more prominent of these are the erect posture; the peculiar form and construction of the anterior and posterior extremities, as connected with their respective uses of prehension and locomotion; the more elaborate structure of the hands, and especially the size and position of the thumb[1]; the more general action of the digestive organs, so as to constitute man what has been termed omnivorous; his power of accommodating himself to all climates; his slow growth, long infancy, and late puberty; certain sexual peculiarities; the faculties of reason and invention; and lastly, the power of uttering and understanding articulate sounds [1].

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[1] Some writers, as for example, Darwin, have gone so far as to attribute a great part of the superiority of man to the position of the thumb with respect to the fingers, which enables him to grasp and handle objects with more dexterity and minuteness; Zoon. v. i. sect. 16. p. 143, 4; the remark is not wholly unfounded, but it is pushed to an extravagant length. See the remarks of Sir C. Bell, in his Bridgewater Treatise, p. 107, 8. See also Helvetius, De l' Esprit, t. i. p. 60..2, on the various circumstances in the structure and physical functions of animals (even those that the most nearly resemble man) which contribute to prevent their progressive improvement.

These various circumstances, and others of less moment, which I have omitted to enumerate, are amply sufficient to establish the position, that man is so far removed from all other animals, both in his form and his functions, as to entitle him to be regarded as a distinct species. But when we take a survey of the whole human race, although we find that they agree in their general form and structure, and exhibit a general similarity in their functions, we observe, on the other hand, that there are many points in which they differ very materially among themselves, and that these differences are transmitted from one generation to another, so as to prove that they are not the effect either of external circumstances or of mere fortuitous causes.

Naturalists have differed with respect to the number of varieties into which the human race is to be divided; but the division of Blumenbach is the one which is the most commonly adopted, and which appears to be founded upon the most correct observation. He fixes the number of varieties at five; the Caucasian, so named from its supposed origin in the western part of Asia, the Mongolian, the Æthiopian, the aboriginal American, and the Malay. The Caucasian he regards as the standard or type of the rest; this together with the Mongolian, and the Æthiopian, forming the three most distinct varieties, while the American may be regarded as intermediate between the Caucasian and the Mongolian, and the Malay between the Caucasian and the Æthiopian [2].

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[1] Blumenbach de Gen. Hum. var. nat. sect. 1. Mr. Lawrence has very amply and satisfactorily pointed out the characteristics, both anatomical and physiological, which distinguish man from all other animals; he has ably exposed the exaggerations and errors into which some authors of considerable celebrity have fallen, when they have attempted to approximate certain varieties of the human race to the inferior animals; Lect. sect. 1. p. 134..242. The principal circumstances mentioned by Mr. Lawrence are smoothness of the skin, and absence of natural means of defence; erect stature, with various anatomical points necessarily connected with it; possession of two hands and their perfect structure; great proportion of the cranium to the face; structure and relation of the jaws; structure and position of the teeth; development of the cerebral hemispheres; proportion of the brain to the nerves; greater number and superiority of the mental faculties; speech; capability of inhabiting all climates, and subsisting on all kinds of food; slow growth, long infancy, late puberty, and certain sexual peculiarities. Camper has pointed out, with much clearness, the leading circumstances in which the anatomical structure of man differs from that of other animals; "Deux Discours sur l'Analogie qu'il y a entre la Structure du Corps Humain et celle des Quadrupedes," &c. in Œuvres, t. iii. p. 527 et seq. with the accompanying plates. See also the remarks of Rudolphi, Elem. of Physiol. by How, B. 1. Ch. 1.

[2] De Gen. Hum. var. nat. sect. 3. See also Lawrence's Lectures, p. 326, 7, and plates Nos. 1..5, taken from Blumenbach, and his 10th chapter,

Their present distribution over the face of the globe would appear to coincide nearly with what it has been ever since we were in possession of any adequate description of the different parts of its surface. The Caucasian inhabits the whole of Europe, except the northern parts of Sweden, Norway, and Russia, the west of Asia as far as the Oby, the Caspian Sea, and the Ganges, together with the north, and even a portion of the interior of Africa. The Mongols are spread over the central and eastern parts of Asia, with the exception of the Peninsula of Malacca; they also stretch along the whole of the arctic regions, from Russia and Lapland, to Greenland, and the northern parts of America, as far as Behring's Straits. The Æthiopic variety inhabits the greatest part of the central, and the whole of the southern parts of Africa, and is also found dispersed over some of the Oriental Islands. The Malays, however, constitute the greatest part of the inhabitants of these islands, as well as of Malacca and the islands of the Pacific Ocean; while the whole of America, except the northern extremity, is the native seat of what are termed the American Indians.

An interesting question here presents itself, whether these dif-

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where will be found a collection of very valuable facts, and a copious list of references; also Cloquet, Anat. de l'Homme, t. i. pl. 29, 0. Dr. Prichard, in his interesting and valuable "Researches into the physical History of Man," minutely examines the different circumstances which are pointed out by Blumenbach, as forming the characteristics of his five varieties, and in consequence of the number of exceptions which are to be met with, he is disposed to reduce them to three. From the form of the upper part of the head, especially from its breadth, as viewed posteriorly and vertically, he designates them by the terms mesobregmate, stenobregmate, and platybregmate: these nearly coincide with the three principal varieties of Blumenbach, the Caucasian, the Mongolian, and the Æthiopic; vol. i. p. 173, 4. Other physiologists, especially among the French, who are generally disposed to multiply divisions in all branches of science, have added to the number of Blumenbach. Bory de St. Vincent, in his treatise entitled, "L'Homme," has extended them to fifteen; t. i. p. 82, 3; see also Brewster's Journ. v. v. p. 39..1. Dumeril, in his Zoologie Analytique, divides the human species into six races or varieties; the Caucasian, or "Arabe Européenne;" the Hyperborean, probably a mixture of the Caucasian and the Mongol; the Mongol inhabiting Australia, China, and Tartary; the native Americans; the Malay inhabiting the Pacific and Oriental Isles and Malacca; and the Ethiopean; this last, the author remarks, may be almost considered as a different species of the genus. Beclard nearly agrees with Blumenbach, Elem. d'Anat. p. 110, 1. Rudolphi reduces the number of varieties to four, omitting the Malay; Physiol. b. 1. ch. 2. In the third vol. of Buffon's Nat. Hist. sect. 9. p. 302 et seq., we have many interesting observations on this subject, delivered in the animated style which characterizes the works of this author. See also remarks by Lacepede, art. "Homme," in Dict. Scien. Nat. t. xxi. p. 382 .. 392; Virey, Histoire Naturelle du Genre Humain, in his first section, t. i. p. 119 et seq., and the art. "Mazology," in Brewster, by Prof. Muirhead, v. xiii. p. 477; see also Adelon, Physiol. t. iv. p. 526 et seq. Mr. Mayo's 17th chap. contains much interesting information; he concludes generally, that the varieties may be referred to the influence of accidental causes operating on an original species; p. 451, 2. We have some valuable observations on various points connected with this subject in Monro's Elem. ch. xii. v. i. p. 194 et seq.

ferent varieties, as they have been termed, the European and the African for example, are derived from one common stock, the present differences being merely the result of circumstances, operating on them through a long course of ages, or whether they have sprung from different parents, each possessed of the characters of their respective descendants. In technical language, are they varieties only of the same species, or are we to regard them as distinct species[1]?

Although the division of the objects of natural history into the different gradations which constitute what is termed a scientific arrangement is too often altogether arbitrary, with regard to the case now before us, we are indebted to Blumenbach for an accurate conception of the object of the inquiry. According to his definition, animals may be considered as belonging to the same species, when they agree so far in their form and habits, as that those points in which they differ may be referred solely to the effect of what he terms degeneration[2]; while, on the contrary, when these differences cannot be referred to any source of degeneration, they must be considered as belonging to different species[3]. The causes of degeneration which he points out are temperature, climate, modes of life, diet, and some other circumstances of an analogous nature, and he examines at length the operation which these several circumstances may be supposed to have had in altering or modifying the human form and constitution[1].

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[1] Many persons are disposed to regard this discussion as altogether useless, or even to denounce it as impious, alleging that the question is decided by the account given us in the commencement of the book of Genesis, of the creation of the human race. But I conceive it to be a legitimate object of inquiry. We do not find that the writer of this book lays claim to any super-human source of information with respect to natural phenomena, while the whole tenor of his work seems to shew, that on such topics, he adopted the opinions which were current among his contemporaries. We may respect the feeling which produces this zeal in the cause of religion, but we must lament the indiscreet mode in which it is exercised. It might have been expected, at least in a Protestant country, that the example of Galileo would have proved to us the danger of identifying the truth of our theological creed with the correctness of our philosophical speculations, and that the liberty which we are compelled to allow to the astronomer, might have been extended to the inquirers into the other departments of natural knowledge. But some recent examples show us that this period is not yet arrived. "Well, indeed, it is for us," to borrow the expressive language of a writer in the Quarterly Review, "that the cause of revelation does not depend upon questions such as these: for it is remarkable that in every instance the controversy has ended in a gradual surrender of those very points, which were at one time represented as involving the vital interests of religion. Truth, it is certain, cannot be opposed to truth. How inconsiderate a risk then do these advocates run, who declare that the whole cause is at issue in a single dispute, and that the substance of our faith hangs upon a thread, upon the literal interpretation of some word or phrase, against which fresh arguments are springing up from day to day." v. xxix. p. 163. I may also refer my readers to the Lettres Provinciales of Pascal, No. 18, p. 330..2, where the same sentiment is clearly expressed, and is delivered in that forcible language for which this writer is so eminently distinguished.

[2] I may remark, that the term degeneration is not used by Blumenbach in its popular sense, as being equivalent to deterioration, but is employed, in conformity with its derivative meaning, to signify any deviation which takes place from the primary type, or original condition of the species.

[3] On the constitution of a species we have the following remarks in Dr.

Upon the first view of the subject, we might be tempted to pronounce, without hesitation, that we are acquainted with no natural causes, which possess sufficient power to effect so great a metamorphosis as we actually find to exist. It is also alleged, that as far as we can judge, since the first records of history, the same differences have existed that we observe in the present day[2]. Nor do we find those shades and gradations which might have been expected, were the varieties the mere result of external causes operating on the human body, which necessarily act upon different individuals in different degrees. Besides, there are instances where tribes, belonging to different varieties, have, for a long space of time, lived in the same country, and under the influence of the same circumstances, yet where no approach to a common nature has been observed to take place, but each

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Prichard's "Researches;" "..the term *species* must be solely applied to those collections of individuals which so resemble each other, that by referring merely to the known and well ascertained operation of physical causes, all the differences between them may be accounted for, so as to present no obstacle to our regarding them as the offspring of one stock, or, which is the same thing, of races precisely resembling each other;" v. i. p. 92. Dr. Fleming defines the word species to be "a term universally employed to characterize a group, consisting of individuals possessing the greatest number of common properties, and producing, without constraint, a fertile progeny;" Phil. of Zool. v. ii. p. 148. Cuvier observes, on this subject, "On est obligé d'admettre certaines forms, qui se sont perpetuées depuis l'origine des choses, sans exceder ces limites; et tous les êtres appartenans à l'une de ces formes constituent ce que l'on appelle une *espèce;* ses varietés sont des subdivisions accidentelles de l'espèce;" Regne Animal, t. iv. p. 19. It might seem that this question is decided by the criterion that was proposed by Hunter and other eminent naturalists, whether the offspring be prolific. It is sufficiently proved, that in most cases where the experiment has been tried, a hybrid, produced from parents who are admitted to be of different species, is unproductive. This consideration certainly affords a strong presumption in favour of the common origin of mankind; but it appears that we are not yet in possession of a sufficient variety of facts to allow us to draw the general conclusion; see Prichard's Research. v. i. p. 95..8; Lawrence's Lect. p. 265..9. Hunter's "Observations to show that the wolf, jackall, and dog, are all of one species," even if we allow them to be conclusive as to the point for which the experiments were instituted, can scarcely be considered as bearing upon the general question; Anim. Œcon. p. 143 et seq. Dr. Edwards has lately published an essay, in which these various circumstances are well illustrated; "Des Caractères Physiologiques des Races Humains."

[1] De Gen. Hum. var. nat. § 23. p. 66, 7.

[2] This was remarkably exemplified in the Egyptian tomb, for an exact copy of which we are indebted to the skill and perseverance of the enterprising traveller Belzoni. Among the figures that were painted on the walls, the difference between the negro and the Arab was as clearly marked as at the present day. Respecting the exact age of this interesting record of antiquity, I do not profess to give an opinion; but there can be no doubt that it is sufficiently remote for the purpose of my argument.

has retained its peculiar traits[1]. It must be admitted that the different circumstances referred to above may have considerable power over the vital functions; and indeed it is, in a great measure, to their combined operation that we must ascribe the changes which are produced in the inferior animals by what is termed domestication. Yet, I conceive, that if we carefully notice the facts that fall under our observation, we shall scarcely be able to point out that decided and unequivocal influence, which might be supposed necessary to produce the effects that are actually perceived.

One of the most obvious marks of distinction between the different varieties of the human race is the colour of the skin; and as Blumenbach assumes the white variety to be the standard or type of the species[2], it is necessary, in order to establish the hypothesis, to show how any of the supposed causes of degeneration can produce the black colour of the Æthiopian. He accordingly attempts to account for this colour by supposing, that the heat of the climate gives rise to an excessive secretion of bile, and that, in consequence of the connexion which there is between the action of the liver and the skin, an accumulation of carbonaceous matter takes place in the cutaneous vessels, and that this process being continued for a succession of ages, the black colour of the skin becomes habitual[3]. But upon this hypothesis we may remark, that although the inhabitants of colder climates, when they pass into the torrid zone, are frequently affected with bilious diseases, and consequently acquire a sallow complexion, this is merely the effect of disease; and as we do not find the natives of these countries to be liable to such affections, we are not entitled to account for the peculiar colour of the skin upon this principle. Besides, we do not find that the children of those who have acquired this sallow complexion, by a residence in warm climates, provided they are themselves healthy, and are born in the temperate zone, inherit, in any degree, the complexion of the parents[1].

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[1] This appears to be the case with the inhabitants of some of the Oriental Islands, where we have the Aboriginal Malays, with a mixture of the Chinese and the Negroes; the two latter, as it would appear, having been settled there for some centuries, but each retaining all their distinctive characters. In those countries the climate and the state of society produce a considerable similarity in the habits of the different classes of people, so that all of them are exposed to nearly the same physical and moral influences.

[2] Blumenbach assumes the Caucasian as the type of the rest, partly from its possessing the specific characters of man in the most marked degree, and partly in consequence of the form of the head being intermediate between the other varieties.

[3] De Gen. Hum. var. nat. § 44, 5. p. 122 et seq. The following positions contain the fundamental parts of the hypothesis: "Causam equidem proximam adusti aut fusci coloris externorum cutis integumentorum, in abundante carbonaceo corporis humani elemento quærendam censeo, quod cum hydrogenio per corium excernitur, oxygenii vero atmospherici accessu præcipitatum, Malpighiano muco infigitur." p. 124, 5. "In universum autem carbonaceum istud elementum maxime in atrabiliis prævalere videtur; manifeste etiam officinæ bilis cum integumentis....consensus." p. 126. "Tum autem ingens climatum in hepatis actionem potentia, utpote quæ intratropicos cœli ardore mirum quantum excitatur et augetur." p. 126... "Æthiopes vero indigenæ diutissime jam et per longas generationum series climatis istius actioni obnoxii fuere,.." p. 127. An hypothesis very similar to Blumenbach's is adopted by Dr. S. S. Smith, of New Jersey: he remarks, "it appears that the complexion in any climate will be changed towards black, in proportion to the degree of heat in the atmosphere, and to the quantity of bile in the skin." Essay on the Variety of Complexion &c. p. 30. See the observations of Prichard, v. ii. p. 528..0; also the art. "Temperament," in the Cyc. of Med. p. 161 et seq.

Blumenbach is disposed to refer the dark colour of the Æthiopian, in some measure also, to the direct effect of exposure to the sun's rays, conceiving it to be analogous to that browning or tanning of the skin which takes place, from the same cause, among Europeans. But I conceive that this eminent physiologist has, in this case, been misled by a false analogy. There is, in fact, no relation between these two affections: their seat is different; the one being in the epidermis, the other in the more vascular part of the cutis. The former is a temporary or transient effect, which is, in a great measure, removed when the immediate exciting cause is withdrawn; while, as far as we can perceive, the colour of the negro is not in the least changed by the removal into a colder climate, and is transmitted unimpaired to his posterity[2].

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[1] Dr. Prichard enters at length into the consideration of the structure of the parts on which the variety of colour depends, and of the different circumstances connected with it; Researches, ch. iii. sect. 2, 3, 4. p. 131..156. He reduces the varieties of colour to three only, which he technically names the melanic, the albino, and the xanthous. The second of these terms is employed in its ordinary acceptation, while by the first and third the author designates the great mass of the human race, without reference to their situation on the face of the globe or their other peculiarities. The first includes all the dark complexions, from the negro to the European brunet; the latter, "all those individuals who have light brown, auburn, yellow, or red hair." I am, however, disposed to think, that the more popular arrangement of the shades of the skin into white, black, yellow, copper-coloured, and tawny, corresponding to the five varieties of Blumenbach, will be found more discriminative, and more applicable to our physiological investigations. The articles "Complexion," and "Gypsies," in Brewster's Encyc., contain many valuable remarks that bear upon this question.

[2] The best authenticated narratives of travellers prove to us, that although, as a general fact, the Æthiopic variety is found in the hottest regions, yet that there is not an exact proportion between the heat of the climate and the blackness of the skin; see Blum. de Gen. Hum. var. § 43. p. 121, 2. This was particularly noticed by Humboldt, as applicable to the different parts of the American continent, where the natives all belong to the same variety, and are therefore a proper subject of comparison. In the same way we find, from the narratives of Cook and other navigators, that the inhabitants of Otaheite, and of others of the Pacific Islands, that are situated not far from the Equator, are fairer than the generality of the Malays, and it also appears that the Chinese are fairer than the Esquimaux, both of them being derived from the Mongolian stock; Elliotson's Blumenbach, p. 412, with the note. We have a copious collection of facts on this subject, accompanied by many judicious observations, in Dr. Prichard's Researches, v. i. books 3, 4, and 5. His general conclusion, as expressed in the following paragraph, appears to me

With respect to the other circumstances, in which the different tribes of the human species differ from each other, such as the form of the bones and of the soft parts; and even, in some cases, a difference in the structure of certain organs, we are still less able to conceive how they can have been produced by any of the external causes which may be supposed to operate upon the living body. The influence of temperature and of climate generally, of food, and of the occupations and habits of life, has been frequently made the subject of inquiry both by medical and by physiological writers, but without our being able to arrive at any very precise results. It would seem, however, to be pretty clearly established, that the same animal, when suffered to live at large in different countries, acquires different characters, and we can often perceive that the character which it has acquired is peculiarly well adapted for its new situation. We are, however, for the most part, altogether unable to assign any probable cause for this alteration, and we refer it to the effects of climate and diet, merely because we know of no others which can be supposed to operate [1]. One of the most remarkable examples of

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to be authorized by the premises: "The influence of the climate on the colour and organization of mankind is another inquiry, which the history of the great insular races might be expected to elucidate. With respect to the Polynesian tribes, it has been remarked by Mr. Marsden and Mr. Crawford, that the heat of climate seems to have no connexion with the darkness of complexion. The fairest nations are, in most instances, those situated nearest to the equator. If we inquire into the history of the Papua and Australasian tribes, with relation to this point, we shall find that the complexion does not become regularly lighter as we recede from the intertropical clime; for the people of Van Dieman's land, who are the most distant from the equator, are black; but we observe, that the occasional deviation to light hues chiefly displays itself in temperate regions, as in New Holland, among the tribes in the neighbourhood of Port Jackson." p. 489, 0. We have many interesting remarks on the colour of the human species in Mr. Lawrence's Lect. ch. ii. p. 271 et seq. See also Dr. Prichard's remarks on the complexion of the various tribes of what he terms the Indo-European nations, v. ii. p. 204, 5; also further remarks on the connexion between the darkness of the complexion and the heat of the climate in v. ii. p. 531, 2. On this and other analogous topics, we find much interesting matter in Forster's observations made in his voyage with Cook, ch. vi. p. 212 et seq. Camper, however, in an essay written expressly on this topic, "De l'Origine et de la Couleur des Negres," adopts the popular opinion, that the blackness of their skin depends upon the direct effects of climate, and that the brownness occasioned by exposure to the sun's rays, is transmitted to the offspring, and becomes increased by a number of successive generations; Œuvres, t. ii. p. 451 et seq.

[1] Prichard's Research. b. 9. ch. i. sect. 7. "On the relation of particular varieties of the human species to climate," contains many valuable facts and judicious remarks. I may refer, in this connexion, to some remarks of St. Hilaire, on the variation in the size of the human subject; this he states to be greater than in other species of animals. For the most part the least size is found in the N. hemisphere, and the greatest in the S.; Ann. Sc. Nat. t. xxvii. p. 85 et seq. Dr. Edwards, in his essay "Des caractères physiologiques des races humaines," discusses at length the effect of external circumstances, in altering or modifying the original characters of the human frame. The general tendency of his remarks is in favour of the permanency of the character, and he adduces various considerations in proof of his opinion, from

the influence of external circumstances, upon both the physical and the intellectual powers, is the production of what is termed cretinism in certain parts of Switzerland. It consists in a state of mental imbecility, combined with, and probably depending upon, a mal-conformation of the bones of the head; it appears to be generated by something peculiar to the atmosphere of confined valleys, and does not seem to be hereditary [1].

These various considerations afford a powerful argument in favour of an original difference in the varieties of the human race [2], but there are others, equally or perhaps more powerful, which lead to the opposite conclusion. The analogy of the inferior animals strongly supports the doctrine of the common origin of mankind. The different kinds of dogs, for example, which exhibit so many forms, sizes, and colours, are supposed to have all proceeded from one source, yet they remain as permanently distinct from each other as the European and the negro, and are apparently as little affected by external circumstances [3].

This formation of varieties, which afterwards become permanent, seems to depend upon some natural tendency in the animal constitution, which it is not easy to explain, but of which we often meet with curious illustrations. A remarkable instance was lately related by Sir A. Carlisle, with respect to an American family, where a female had two thumbs on each hand, and six toes on each foot. She married and had several children, who, in their turn, became parents, and at the present time a considerable number of her descendants possess the supernumerary thumbs and toes [4]. A similar series of facts has occurred in the

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the present state of the inhabitants of the various countries of Europe. The familiar example of the Jews, who retain, in a great degree, their original characters, in all climates and parts of the world where they are dispersed, confirms the opinion of Dr. Edwards; the same remark may also be applied to the Gypsies. For an account of this race I may refer to the article in Brewster's Encyc. by Dalyell.

[1] Saussure, Voyages dans les Alps, ch. 47. § 1050..6 et alibi; Reeve on Cretinism, in Phil. Trans. for 1808, p. 111 et seq.; Alison's Physiol. p. 343; Prichard on Insanity, p. 318 et seq.

[2] The difficulty of assigning the common origin to mankind is forcibly, although perhaps insidiously, urged by Kames, in the preliminary discourse to his Sketches of the History of Man; v. i. p. 3 et seq.

[3] It is scarcely necessary to remark, that the question respecting the origin of the dog itself, whether it be a primary species, or derived from the wolf or jackall, does not affect the point under discussion; no one imagines that all the existing varieties of the dog were produced by the union of a number of different primary species.

[4] Phil. Trans. for 1814. p. 94 et seq. It appears that this peculiarity has now gone to the fourth generation, and has been propagated both by the male and female parents. Many cases of a similar or analogous kind are related by Morand; Mém. Acad. pour 1770, p. 137 et seq., where various members of the same family had the supernumerary finger, and we find the same circumstance noticed by Pliny; Lib. 11. cap. 43. We have a very ample and learned account of these peculiarities in Is. St. Hilaire's "Anomalies de l'Organization," p. 681 et seq. His remarks on the causes of these irregular-

family of the individual who obtained the name of the Porcupine Man, one of whose descendants, in the third generation, was lately exhibited in this metropolis, possessing exactly the peculiarities of his grandfather[1].

We may, upon this principle, partly explain the mode in which the varieties of the human race were originally produced; but we can scarcely suppose that it was the sole principle which was called into action; it is more probable that the effect produced is the result of the co-operation of many causes, than of any single accidental occurrence. And this is more analogous to the changes that take place among the inferior animals, where we see the remarkable effects of domestication in producing these changes. The dog, in its wild state, always exhibits nearly the same characters; it is covered with hair of the same colour, its ears and tail are of the same shape, its limbs of the same form, and it manifests the same powers and instincts. Yet, into what numerous varieties do we behold it transformed, when it becomes the guard and companion of man. Its size and disposition varying from the formidable mastiff to the puny lap-dog; its hair all colours, sometimes short and smooth, at other times long and curled, the shape of its face, ears, and tail, exhibiting every shade of difference. These differences we account for from the joint operation of the two principles which have been described above; the production of what we term accidental varieties, similar to the supernumerary fingers and toes of the American family, or the protuberances of the porcupine man, and the more gradual and continuous action of domestication, by which an equally remarkable change is brought about, and is transmitted to the descendants of the animals so changed. In many of the inferior animals we can distinctly perceive the most unequivocal proofs of the operation of both these causes; and with respect to the first, at least, we

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ities, and the mode of their production are peculiarly interesting, and enable us to refer to certain general principles a series of facts, which, at their first view, would appear to be incapable of generalization. See also the art. "Monstre," in Dict. Class. d'Hist. nat. by G. St. Hilaire.

[1] The origin of this family peculiarity is satisfactorily ascertained in the account which is given by Machin, in the Phil. Trans. No. 424, p. 299, who describes the boy, Ed. Lambert, then fourteen years of age. In the Phil. Trans. for 1755, p. 21 et seq., we have a further account by Baker, of the same individual, then forty years of age, and the father of six children, all with the prominences on the epidermis like himself. In 1802, Tilesius published a description of one of these children, then an adult, with engravings; and in the year 1821, an individual of the third generation was publicly exhibited in this metropolis, whose skin exactly resembled the original description in the Phil. Trans. and the plates of Tilesius. See also Buffon, Hist. Nat. t. iii. p. 570, 1. We have an account by Mr. Humphries, of the origin of a new variety of sheep, which lately occurred in America, and which offers a series of facts precisely analogous to the above; Phil. Trans. for 1813. p. 88 et seq. See Prichard's Resear. v. ii. p. 550. In Brewster's Journ. v. 8. p. 24, 5, we have an account of a native of Ava, entirely covered with hair, who had a daughter in the same state.

have equally clear evidence that it applies to the human race. With respect to the second, we are not able to adduce any facts of so direct a nature as applicable to man, but still we have sufficient evidence, that the effect of refinement and a high state of civilization on the human race, is analogous to that of domestication on the inferior animals[1]. In those countries where the difference of habits between the higher and the lower classes exists in the greatest degree, and where, from moral and political causes, they are kept the most distinct, an obvious difference may be observed in their form and organization, although they both of them belong to the same variety[2].

If then we admit the common origin of mankind, we may inquire, whether any of the varieties, as they now exist on the earth, is similar to the first created pair, and if so, which of them it is which bears this resemblance. In the prosecution of this inquiry we may derive some faint light from historical records, and some perhaps still fainter from analogy, but we are left almost entirely to the uncertain guidance of conjecture. Now we may remark, that it is more probable, that the changes induced upon mankind, have been in consequence of a progress from a state of barbarism to one of refinement, than the reverse; and hence we are led to regard that variety to be the primary one, which, through all the vicissitudes of human affairs, has remained in the most degraded state, and which, in its structure, differs the most from that variety, which has uniformly enjoyed the greatest degree of civilization. Upon this principle we must regard the Æthiopian as the type of the original pair, from which have sprung the Mongolian, the Malay, the aboriginal American, and lastly, the Caucasian, which we are entitled to regard as the most perfect specimen of the human race[3].

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[1] Dr. Prichard enters into a minute examination of the state of the different varieties of the human species, and compares the general laws of the animal œconomy as they are manifested in each of them. His conclusion is, that "it does not appear, from a review of the principal facts in physiology, as they have been traced among the different races of men, that these races are distinguished from each other by any of those broad outlines, which generally, perhaps uniformly, separate particular species of animals. The great laws of the animal œconomy are the same in their operation on all. There are deviations in some respects, but these deviations are not greater than the common degree of variety in constitution which occurs within the limits of the same family." Researches, v. i. p. 125. We have a very judicious recapitulation of the arguments upon which this opinion is founded in the second chapter of the ninth book, v. ii. p. 584 et seq.

[2] This is particularly observable among the inhabitants of Hindostan, where, in consequence of the division into castes, the same condition of life, and the same occupation are continued, without any change, through many successive generations. The superior orders, who are employed as artisans, are of a decidedly lighter complexion than the agriculturists; in many of the Pacific islands the same difference exists between the different classes as in Hindostan.

[3] Dr. Prichard conceives, that what he terms the melanic variety, "may be looked upon as the natural and original complexion of the human race."

As it appears to have been among the Egyptians that the first great progress was made in the arts of civilization, it has become an interesting subject of inquiry, to what race or variety this people ought to be referred. There are certain passages in the writings of the ancients, which seem to prove, that individuals possessed of the negro character existed in Egypt, and that a dark complexion was generally regarded as characteristic of its inhabitants. We may presume, however, that at an early period, earlier than that to which our historical records extend, the original character had been considerably modified, or perhaps entirely changed, so that the form and complexion of the Egyptians more nearly resembled that of some of the Asiatic tribes of the Caucasian variety, than any belonging to the Æthiopian race.

The inquiry has been pursued with much learning and industry by Cuvier, principally by the examination of the skulls of mummies, and the result appears to warrant the conclusion that, as far as regards the form of the skull, the ancient Egyptians resembled the Caucasian variety, at the period of their highest advance in civilization[1]. Dr. Prichard has examined this question, with his accustomed ingenuity and accurate research, and concludes that the ancient Egyptians did not resemble the negroes as they exist in the western parts of Africa, where their peculiar traits are the most strongly marked; but that they partook, in certain respects, of the African countenance and complexion[2]: the opinion of Blumenbach may be considered as not essentially different from Dr. Prichard's[3]. We have not sufficient data to enable us to determine, whether the original negro race was gradually metamorphosed into the state which is indicated by the existing remains, or whether the change was effected by some political revolution.

It has been a favourite object with many naturalists to establish a regular gradation among the different classes of animals, so as to form the whole into one chain; the contiguous links of which are closely connected with each other, and carry us on from the least perfectly organized to that which

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Researches, v. i. p. 139. We are told by Dr. S. S. Smith, that the negro population of North America is gradually acquiring a lighter hue, and that the peculiarities of their features are likewise diminishing, p. 91, 2 et alibi; see also Prichard, v. ii. p. 365, 6. Hunter remarks, that in the inferior animals, the alteration which is produced in them by what may be termed civilization, by shelter from the inclemencies of the seasons, by nutritive food, cleanliness, and other circumstances of this description, consists in changing their colour from a darker to a lighter shade; Observ. on the Anim. Œcon. p. 244.

[1] Lawrence's Lect. p. 339..348.

[2] Researches, ch. v. sect. 9. p. 316 et seq.

[3] Phil. Trans. for 1794, p. 177 et seq. We have an account of a minute examination that was made by Dr. Granville, of a female mummy: from accurate measurements of the different parts, especially of the skull and the pelvis, it appeared to correspond with the most perfect specimens of the Caucasian variety; Phil. Trans. for 1825, p. 279..1.

is the most so, by almost insensible degrees. This has been applied to the human race; and it appears that, if we arrange the skulls of the different varieties according to the forms, the most perfectly characterized European will stand at one end of the scale, and the African at the other; while the intermediate space will be filled up with the Mongolian, the American, and the Malay. It also appears that, if we continue the scale to some of the inferior animals, the gradation proceeds with a certain degree of regularity, indeed so much so, that in some species of the simiæ, the skull resembles the Æthiopian, nearly as much as the Æthiopian resembles the European. The Grecian sculptors were so sensible of this effect, that without any reference to, or probably knowledge of the fact, as far as regards the different varieties of the human race, in the heads which they formed to represent the gods, they exaggerated the characteristic trait of the Caucasian skull, and by bringing forwards still farther the upper part of the head, they gave to the countenance an expression of superior intellect, which is always associated with the peculiar configuration of the head.

Camper endeavoured to establish a method of ascertaining the exact proportions which the different parts of the head bear to each other in different individuals, from which we might derive an indication of the state of the intellectual faculties. It consisted in drawing a horizontal line through the meatus auditorius, and another line along the profile of the face, so as to touch the most projecting parts of the forehead and the upper jaw. These two lines, by their intersection, make what he terms the facial angle, the size of which is increased by the prominence of the forehead and the recession of the jaw [1]. We cannot hesitate to admit the correctness of Camper's observations, and we can scarcely refuse our assent to the conclusion that he deduces from them. Cuvier, however, who has given a correct summary of Camper's dissertation, conceives that the method is imperfect, as affording a view of the form of the head in one direction only. He conceives that we obtain a more correct idea of the relation between the cranium and the face, by viewing the head vertically, and we find that, by this method, we obtain the same gradation of form from the European to the Ourang-outang, through the Mongolian and the Æthiopic varieties [2].

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[1] Dissertation sur les différences des traits du visage, pars 1. ch. iii. p. 34 et seq. In the fifth chap. p. 51 et seq. we have the results of his measurement of different skulls, illustrated by a series of engravings; tab. 1, 2. See also Cloquet, Anat. de l'Homme, t. i. pl. 28.

[2] Leçons d'Anat. Comp. No. 8. art. 1. t. ii. p. 1..12. He estimates the facial angle of the European at 80°, and remarks, that the angle of the negro, on the one hand, is 70°, while that of the antique statues, on the other, is 90°, p. 7. We have some judicious remarks upon these measurements in the seventh and eighth of Sir C. Bell's Essays on the Anatomy of Expression; also in the fifth sect. of Dr. Pritchard's Researches. See also on this subject the remarks of Dr. Leach, art. "Craniometry," in Brewster's Encyc.

Blumenbach has formed a most extensive and well authenticated collection of skulls, procured from different parts of the world, by which the characteristic forms of the different varieties are fully established. The most important points in which the heads of the Caucasian and the Æthiopian differ from each other are, that the former is more round and altogether more capacious, the forehead is broader and more prominent, and the upper part of the head generally is larger in proportion to the face [1]. There are likewise other parts of the body, in which the negro differs anatomically from the European, occurring both in the bones, the muscles, and the soft textures. They are of a kind which cannot be fairly attributed to any thing in the habits or modes of life of the individual, but appear, like the colour of the skin and the texture of the hair, to be transmitted from one generation to another, independent of external circumstances. These differences, although considerable and permanent, appear to be exactly analogous to those which occur among the inferior animals of the same species; and therefore, although we are unable to account for their production, they do not oppose the conclusion, that the whole of the human race are derived from one pair [2].

So far we proceed upon the basis of fact; but we enter upon more doubtful ground, when we inquire whether there be any innate difference of intellect or general character connected with these variations of the external form. The data by which this question is to be decided lie equally open to the judgment of every one; yet our opinions have been so biassed by considerations of a collateral nature, connected with our moral and political speculations, as to have led to the most opposite conclusions. But such considerations, however important in themselves, should not interfere with the pursuit of truth in our scientific researches. In the present instance, I conceive that both the evidence of historic testimony and the deductions from anatomy and physiology will lead to the same conclusion, that the Æthiopian is naturally inferior to the European in his moral and intellectual powers. It may be conjectured, that while the other varieties of the human race have had, from various causes, their organization improved and their faculties elevated, the African has remained stationary, and nearly resembles, at the present day, the state of man at his first creation [3].

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[1] Blumenbach, Decades collectionis suæ craniorum, cum tabulis; also De Gen. Hum. var. nat. p. xxii..xxxiv. tab. I, 2.

[2] We have a judicious summary of the facts, as far as regards the form of the bones, in Dr. Gibson's dissertation, "De Forma Ossium Gentilitia;" and on the general differences of structure and organization in White, "On the Gradation of Animals," a treatise which contains many curious observations, although I conceive that the author has failed in establishing his fundamental position. Spix's interesting and beautiful work, entitled, "Cephalogenesis," may be referred to in this place, as indirectly connected with this subject.

[3] The remark of Hume on this subject appears to me to be the just deduction from the accumulated and unvarying experience of ages; Essays, v. i.

SECT. 2. *Of Temperaments.*

A temperament may be defined a peculiar state of the system, depending on the relation between its different capacities and functions, by which it acquires a tendency to certain actions. I have frequently had occasion to remark, how accurately the different powers are all adjusted to each other, so as to produce one harmonious whole. If the disproportion be too great, disease ensues; but there are many gradations, compatible with health, where yet this disproportion is very observable. A human body, in its most perfect state, should have a certain degree of contractility of the muscular fibre, and a certain degree of sensibility of the nerves. The digestive organs ought to prepare a certain quantity of chyle, and from this a due supply of blood ought to be elaborated; the lungs should be sufficiently capacious to act upon the blood, and all the other functions should proceed in their proper course, so as to form the due balance of the whole of the system.

The ancients paid considerable attention to the subject of temperaments, and pointed out various peculiarities in the constitution and actions of the living body, which have been seen so far to coincide with general observation, that their nomenclature has continued in pretty general use, even to the present day, although the hypothesis on which it was founded is universally discarded. They described four temperaments, corresponding to the four qualities of Hippocrates—hot, cold, moist, and dry: these were supposed to give the specific characters to the four ingredients of which the blood was thought to be com-

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p. 21, note M. p. 512. We have, indeed, some rare instances brought forward of negroes who have made a certain proficiency in the liberal arts and sciences. But the actual advance in these cases is inconsiderable, and the admiration with which it is received is a strong confirmation of the truth of the doctrine which is maintained in the text. Those authors who endeavour, upon such a foundation, to establish the equality of the intellect of the negro, might, upon the same principle, argue that the ass is as large as the horse, because an instance may be adduced in which an unusually large ass has exceeded the size of a small horse. We have some judicious remarks by Mr. Lawrence, upon the permanent intellectual superiority of the Caucasian variety, in the eighth chapter of his "Lectures." We have some interesting remarks on the intellectual capacity of the negro, by Jefferson. A very full, and, we may conclude, a very correct account of the anatomical differences between the body of the European and the negro is contained in Sœmmering's treatise, written expressly on this subject; we have a copious abstract of it appended to White's work on the Gradation of Animals. There are two observations made by Sœmmering, which bear immediately upon the present question; that in the negro the size of the skull bears a smaller proportion to the face and organs of sense than it does in the European, and that the brain is smaller in proportion to the aggregate of the nerves which proceed from it to the organs of sense. In both these respects the negro recedes from the characteristic features of the human race. An observation of an analogous kind was made many years ago by Daubenton, that the position of the head on the spinal column differs in man from that in all other animals, and that the peculiarity exists in a less degree in the negro; Mém. Acad. pour 1764, p. 568 et seq.

posed—the red part, the phlegm, the yellow, and the black bile respectively; and hence were derived the names of the sanguine, the phlegmatic, the choleric, and the melancholic temperaments, as indicating an excess of each of these substances[1]. After the revival of letters this division was adopted in its essential parts by the most eminent physiologists: Stahl ingeniously adapted it to the modern doctrines of the humoral pathology[2]; and even Boerhaave, although he increased the number of temperaments to eight, and relinquished the erroneous opinions of Hippocrates and Galen respecting the constitution of the blood, yet he still derived the characters of his temperaments from the principles of the humoral pathology, and supposed them to be formed merely by different combinations of the four cardinal qualities[3].

Haller appears to have been the first who decidedly opposed the ancient doctrine, not only by showing that there was no foundation for the varieties of the temperaments in the peculiar nature of the fluids, but by substituting in their place the vital actions of the system. But his ideas, although to a certain extent correct, are to be regarded rather as an indication of the plan to be pursued, than as comprehending a complete view of the subject[4]. Darwin proceeded upon the principle of Haller, in endeavouring to establish the temperaments upon the vital actions of the system; and in conformity to the hypothesis which he adopted, of reducing these actions to the four heads of irritation, sensation, volition, and association, he formed four temperaments, in which these qualities were conceived respectively to prevail[5].

Perhaps, upon the whole, we may find it convenient to revert to the arrangement of the ancients, which appears to have a real foundation in nature[6], although on this, as on other occasions, the father of medicine blended false theory with correct observation. If to the four temperaments of Hippocrates we add, after the example of Gregory [1], a fifth, the nervous temperament, and bestow new appellations upon the other four, we shall have the leading varieties of the constitution under the denominations of the nervous, the sanguine, the tonic, the relaxed, and the muscular temperaments [2].

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[1] The doctrine of Hippocrates on this subject will be found in his treatise "De Natura Hominis;" Op. a Fœsio, t. i. p. 224 et seq., and that of Galen in his two books, "De Elementis" and "De Temperamentis."

[2] Theor. Med. Vera; sect. de Temp. p. 232. He very elegantly describes the state both of the corporeal and the mental powers, as connected with these supposed conditions of the fluids.

[3] The eight temperaments of Boerhaave are respectively denominated warm, cold, moist, dry, bilious, sanguine, phlegmatic, and atrabilious; Instit. Med. § 889..896.

[4] El. Phys. v. 4. 1..6.

[5] Zoonomia, v. i. sect. 31. p. 354..0. He defines a temperament, "a permanent disposition to certain classes of diseases;" but this, I conceive, is restricting it within too narrow limits; it ought to be extended to the ordinary, as well as to the morbid state of the system.

[6] Cullen admits the four temperaments of Hippocrates, and remarks concerning them, that it is probable they were first founded upon observation, and afterwards adapted to the theory of the ancients, since we find "they have a real existence." Lect. on Mat. Med. p. 18. Dr. Prichard remarks that, "This division of temperaments is by no means a fanciful distinction;" Researches, p. 169. He restricts the number to four, and designates them by the original names. See also his article "Temperament," referred to above, for their origin, and their connexion with the varieties of the human species.

The different states of the system may be conceived to depend partly upon a difference in the original conformation of the body, and partly upon a difference in its powers and functions. The nervous temperament obviously owes its peculiarities principally to the sensibility of the nerves existing in an undue proportion to the contractility of the muscles. The sanguine temperament would appear to depend chiefly upon the organization of the body, and the nature of its composition; the vessels are capacious and the solids distensible, the proportion of the fluids is large, and all the actions, which depend especially upon chemical changes, seem to proceed with an unusual degree of facility. We have, therefore, much activity, but the strength is soon exhausted, while the functions are all disposed to excessive action, and are liable to be deranged from slight causes. The tonic temperament is perhaps the one which must be regarded as the most perfect state of the human frame, that in which the different powers are the most nicely balanced, and where we have the greatest capacity for action, combined with the greatest strength of resistance. The body is spare, but hardy; capable of long-continued exertion, rather than any great degree of physical strength, while the mind is firm and ardent, and exhibits that happy combination of genius and industry, which gives rise to the best directed efforts of human intellect. In the relaxed temperament we have the capacious and distensible fabric of the sanguine constitution, but with a deficiency of the vital powers, and an imperfect development of the functions [3]. The nervous and muscular systems are feeble in their operations, and the various processes, both chemical and mechanical, are of course imperfectly performed. The muscular temperament is that of mere physical strength; there is a great share of contractile power, with a defect of nervous energy; the body is capable of great exertion, but the functions are with difficulty excited into action, while the perceptions are blunt, and deficient both in strength and accuracy. The state of the mind corresponds to that of the body; the feelings are not easily roused, but when the mind is once excited, it obstinately retains the impression, and perseveres in its object with unshaken resolution[1].

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[1] Conspectus, v. i. p. 517..8.

[2] We have a long article on this subject in the Dict. Scien. Méd. by Hallé and Thillaye, t. lvi. p. 458 et seq.; it is, like most other parts of that work, very diffuse, and the list of references very miscellaneous. We have many good remarks on temperaments in Cabanis' Rapports; see particularly t. i. p. 54..64, and 6e Mem. t. i. p. 404 et seq. on the influence of the temperaments upon the formation of the ideas and the moral affections. I may also refer to the art. "Tempéraments", by Adelon, in Dict. de Méd. t. xx. p. 285 et seq.; and to his Physiol. t. iv. p. 490 et seq. We have a separate work by Hallé on temperaments.

[3] In relation to the relaxed temperament, I may refer to the accounts which have been received from various quarters, of the remarkable insensibility to pain, which is manifested by the natives of the East Indies, as indicating an original difference in the state of the nervous system. This insensibility, I conceive, cannot be attributed to any effect of education, or to any acquired condition of the mind or feelings; see Dr. Kennedy's account of the Indian penance of Gulwayty, or Churuh Pooja, in Calcutta Med. and Phys. Trans. v, ii. p. 293 et seq.; and in Brewster's Journ. v. viii. p. 44 et seq. The same

It is admitted that few individuals possess these characteristics in an extreme degree; and even where they have been the most strongly marked by nature, education, climate, habits, and many other causes, may modify them in various ways. They are also capable of being combined together, by which intermediate shades are produced; so that it is often difficult to determine under which temperament many individuals, as we see them in society, ought to be classed. We are, however, warranted in asserting, that different temperaments actually exist, that these differences are innate, and that they attach both to the corporeal and to the mental part of our frame.

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insensibility to pain is stated by Crawford in his account of Ava, to occur with respect to the Burmese, v. i. p. 407, 8.

[1] In Hunter's translation of Lavater, v. i. p. 254, and v. ii. p. 93, we have descriptions of the temperaments, illustrated by very characteristic engravings.

CHAPTER XXI.

OF SLEEP AND DREAMING.

THE physiology of the human frame, as I have hitherto described it, supposes that all the functions proceed in a regular course, without interruption or deviation. But this we are aware is not the case. Even under the most favourable circumstances, the body is of very limited duration; many years elapse before its powers acquire their perfect state, and after a short period they show symptoms of decline. And besides this, which may be regarded as the regular process, we are subject to innumerable irregularities, which give rise to diseases of various kinds, that either accelerate the decay of the system or destroy some of its functions.

The doctrine of diseased action forms the science of pathology, and does not come within my province; but the functions are subject to one kind of partial interruption, which, as it does not constitute disease, will fall under our consideration. I refer to the phenomena of sleep.

In the language of poetry, sleep and death have been compared to each other; and could we conceive of a human being, created in the full possession of all his powers, we might imagine, that when he was first seized with an irresistible inclination to sleep, it would appear like the commencement of dissolution. The resemblance is, however, chiefly apparent. Sleep is a state in which all the vital functions retain their full activity[1], and which is absolutely necessary to the support of our existence. In considering the nature of the phenomena of sleep, two subjects of inquiry especially present themselves; first, in what does the state of sleep differ from that of waking, or in what does sleep essentially consist? And, secondly, what physical change takes place in the brain or nervous system, which can be supposed to be the efficient cause of sleep? In connexion with these topics, I shall make some remarks upon the nature and cause of dreams.

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[1] It may, indeed, be questioned whether this be literally the case: in very profound sleep it is probable that all the vital motions are diminished to a certain degree, although none of them are affected so far as to interfere with the due exercise of the functions to which they are subservient; see Haller, El. Phys. xvii. 3. 3. and Blumenbach, Inst. Phys. by Elliotson, sect. 20. § 320. p. 177. This latter physiologist has, however, gone much too far, when he defines sleep to be a "function, by which the intercourse between the mind and body is suspended." § 318.

SECT. 1. *State of the System during Sleep*[1].

In order to ascertain what is the condition of the system during sleep, it will be proper to examine what takes place in the approach to this state[2]. The first indications are a general languor and an incapacity for exertion of any kind, either mental or corporeal. The impressions made by external objects are scarcely perceived, and our voluntary actions are performed with difficulty. After some time the eye-lids close, the muscles of voluntary motion are relaxed, and we become insensible to what is passing around us. In the mean time the vital functions continue their operations nearly in their usual manner; the heart beats, the muscles of respiration act, and the glands continue to produce their respective secretions. Nor does the mind become inactive, although it no longer preserves its connexion with external objects. The ideas often flow with perhaps greater rapidity than in our waking hours, while imagination, memory, association, and many of the passions seem to exist with peculiar vivacity. These observations upon the state of the system at the approach of sleep, lead us to conclude, that it consists especially in two circumstances; in the suspension of certain of the functions, which act through the medium of the nervous system, and of the power of receiving impressions by the external senses.

Of the sensitive functions which are suspended during sleep the most important is that of volition. We find that all the muscles of voluntary motion lose their power, and it is upon this circumstance that the complete relaxation depends. Darwin[3] and Stewart[4], who have offered many ingenious observations upon sleep, regard the suspension of the power of the will[5] and the absence of impressions on the external senses, as the essential characteristics of sleep. But at the same time that the exercise of the external senses and of voluntary motion is either altogether or nearly suspended, the body appears to retain its susceptibility to the usual internal stimuli; and thus not only the vital functions, as was before observed, proceed nearly in their accustomed manner[1], but we are sensible to the feelings of pain and uneasiness of various kinds.

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[1] On most of the questions that are discussed in this section, as the essential nature of sleep, its efficient cause, and its immediate effect on the several functions, I may refer to Adelon, Physiol. t. ii. p. 292 et seq., and to the art. "Sommeil," Dict. de Méd. t. xix. p. 348 et seq.; also to Bourdon, Princ. de Physiol. liv. 6. par. 2. p. 785 et seq.; to Dr. Philip, Phil. Trans. for 1833, p. 73 et seq. and to Mr. Mayo, Physiol. p. 208 et seq.

[2] See Haller, ubi supra.

[3] Zoonomia, v. i. sect. 18. The remarks of Brown on this part of Darwin's works are sensible and correct; sect. 11.

[4] Elements, v. i. c. v. pt. 1. sect. 5. p. 327 et seq.

[5] Prof. Stewart, indeed, supposes that the will is not actually suspended during sleep, but that it loses its influence over those faculties which are subject to it during our waking state; Elem. p. 330, 1. But I confess that I do not perceive any essential difference between these two cases, while the considerations which he offers in order to prove his opinion, only show that the suspension is not complete.

SECT. 2. *Nature and Cause of Dreams.*

This idea of the state of the system during sleep will assist us in explaining the curious phenomena of dreams. Dreams consist of a succession of ideas, that pass through the mind, with various degrees of rapidity and vividness, without any regard to congruity or consistence. They may generally be traced to some actual occurrence; but these are so perverted and mixed up with imaginary transactions, as to produce the most strange and singular combinations. Dreams differ from our waking thoughts principally in the following circumstances. They are often more vivid, so that we mistake our ideas for perceptions, and conceive that what is only passing through our minds is the representation of what actually exists. In dreams we have little or no conception of time and place, and sometimes we crowd a long series of events into a few moments, and fancy ourselves conveyed to any distance with the most perfect facility. We perpetually fall into the grossest inconsistencies; we suppose persons to be both living and dead at the same time, imagine ourselves to be in two places at once, and we even confound our notions of personal identity. Our passions and feelings, which are occasionally strongly excited during sleep, bear no proportion to the cause which produces them; we are overwhelmed with joy or with grief, without knowing why, and we are even sometimes aware of the unreasonableness of our transports, without being able to check them. It is observed, that surprise is seldom experienced in dreams, and that we pass through all the adventures of an oriental romance, without being conscious of their singularity.

These circumstances, which are among the chief characteristics of dreams, are all explicable upon the principles stated above, that in sleep, the senses are not capable of receiving external impressions; that, although many of the mental powers retain their activity, the exercise of volition is suspended[2]. While we remain in a state of rest, our waking thoughts are directed, in a great measure, by association, but at every moment our senses convey to us the impression of external objects, and, by the agency of the will, we are perpetually directing the train of our ideas into some channel different from that into which it would flow of its own accord. However vivid any of our ideas may be, still we never mistake them for perceptions, and thus we immediately become sensible of the difference between them. Our notions of time and space are affected by the events that are passing around us, and indeed essentially depend upon the comparison which we establish between these events and our internal feelings. From these causes we may explain why our dreams are formed of such a farrago of inconsistent ideas, and why we so seldom experience any surprise at the unnatural combinations that are formed in them. And in the same manner, from the suspension of volition, and consequently from not comparing our ideas with each other, but suffering them to proceed with their natural impetuosity, we may deduce the reason why our passions and exertions are so often disproportioned to the causes exciting them.

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[1] This remark, as will afterwards appear, is to be received with certain limitations; in profound sleep, the action of some of the physical functions is certainly diminished; its effects on animal temperature is noticed by Dr. Edwards, "De l'Influence," &c. p. 478.

[2] Dr. Carmichael endeavours to explain the phenomena of dreams by assuming the existence of separate organs for the different faculties, and by supposing that some of these are liable to be in the state of sleep while others are not so, the different organs of sense being likewise incident to the same irregularity; Trans. of the College of Phys. in Ireland, v. ii. p. 48 et seq.

Dreams then appear to consist of a long train of associated ideas, seldom interrupted, as our waking thoughts are, by the intervention of external impressions, or by the voluntary efforts which we make to alter the course of our ideas, by comparing them together, dismissing some and introducing others at pleasure. The commencement of the associated train seems often to depend upon some feeling excited in a part of the body, or upon an impression made upon an organ of sense; but it is frequently difficult to account for the direction which our ideas afterwards assume. Although we sometimes dream of those events, which have most fully occupied our minds during our waking hours, this is not always the case; on the contrary, our dreams often turn upon the most trifling occurrences, or upon circumstances which had been totally forgotten. This may be explained upon the principles that have been stated above. The mind may have been steadily chained down to one set of ideas for many hours, yet just as we are falling asleep, we may experience a sensation in some part of the body, which calls up a new train of ideas, that retains possession of the mind, and completely excludes the former. Prof. Stewart relates a case, which very aptly illustrates the manner in which an impression made upon the body during sleep calls up a train of associated ideas, and thus produces a dream. A gentleman who, during his travels, had ascended a volcano, having occasion, in consequence of indisposition to apply a bottle of hot water to his feet when he went to bed, dreamed that he was making a journey to the top of Mount Ætna, and that he found the heat of the ground almost insupportable [1].

When the impression made upon the body becomes very powerful, we are generally awaked by it; but it sometimes happens that a great degree of pain or uneasiness is excited, and yet that sleep continues. In this case, we fall into one of those painful dreams to which the name of incubus or night-mare has been given. We feel the uneasiness acutely, and our dream is composed of some association that has been formed with this uneasiness, or one of a similar kind; we are even aware of our situation, and know, probably from former experience, that, could we speak or move, our painful dream would be interrupted. It must be confessed, that there is something obscure, both in the cause producing incubus, and in the relative condition of the physical and mental powers. It would seem as if the mind and body were in different, or even in contrary states, the body in the most profound repose, and the mind peculiarly active, and this disproportion existing in such a degree as to constitute something approaching to disease.

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[1] Elem. v. i. p. 335. We have a good summary of opinions and various original observations in the art. "Dreams," in Brewster's Encyc., by Stevenson. See also Dr. Abercrombie, p. 258..288; and the remarks of Dr. Philip, Phil. Trans. for 1833, p. 36, 7.

There is a peculiar kind of dreaming, which sometimes occurs, called somnambulism or sleep-walking, where the body is still more incapable of receiving impressions than in ordinary sleep, and yet the will has a certain degree of power over the organs of speech and voluntary motion. The individual walks about his apartment, utters sentences, and performs some of his usual occupations, yet he remains so soundly asleep,that it is impossible to awake him without employing a considerable degree of violence. The state of the organs of sense in somnambulism is singular, and almost incomprehensible. At the same time that it is difficult to produce any effect upon them by the usual stimuli, they appear to be sensible to certain actions, but these are exercised in a very limited manner only, and at the pleasure of the individual. Thus there are well attested accounts of somnambulists, who have procured the implements for writing, and have actually transcribed a copy of verses, in such a manner as to prove that they must have used their sight; yet they were, at the same time, incapable of perceiving the brightest light when held close to their eyes. A circumstance in which the state of mind in somnambulism differs very much from that in common dreams is, that the train of ideas is always intently fixed upon one object. It is also remarked, that, notwithstanding the vividness of the imagination, and the firm possession which it acquires of the mind, when the person awakes spontaneously, or is forcibly roused from his sleep, he has frequently no recollection of what has happened to him. The state of somnambulism, in some respects, resembles what has been called a trance, a condition of the body, which, if it ever exists, has probably been much exaggerated by the credulity or superstition of the narrators[1].

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[1] Darwin's Zoon. v. i. sect. 19. § 2. Prichard on the Nervous System, ch. 12. p. 399 et seq., and on Insanity, p. 434 et seq.: also Abercrombie, p. 288 ..305. In connexion with this subject I may refer to a brief but comprehensive account of Animal Magnetism in Prichard on Insanity, ch. 12. sect.

SECT. 3. *Cause of Sleep.*

We must now proceed to the next branch of the inquiry, what is the proximate cause of sleep, or what is the state of the brain and nerves which immediately precedes it? The hypotheses on this subject, although numerous, are not very satisfactory, and Prof. Stewart expressly declares, that the investigation is beyond the reach of the human faculties[1]. We may readily admit, that it still remains involved in much obscurity, but it appears to be a legitimate object of inquiry, and one which is not more beyond our grasp than the other functions of the nervous system. The common opinion among the earlier physiologists was, that sleep depends upon the exhaustion of the animal spirits or nervous fluid[2], but it is sufficient to remark, that the existence of the nervous fluid itself is quite a gratuitous supposition. Haller[3], Hartley[4], and many of the most eminent physiologists of a later date, have conceived that sleep depends upon an accumulation of blood or other fluids in the vessels of the head, pressing upon the brain, and thus impeding its functions. This opinion derives some plausibility from the effects of pressure arising from various morbid causes, which brings on a lethargic state, that finally ends in an abolition of the faculties. The well known case of the Parisian beggar has been often cited in support of this hypothesis. He had a perforation in the skull, by which a portion of the brain was left exposed. When this part was pressed upon, it produced a state of drowsiness, and this might be increased by increasing the pressure, until at length he became completely apoplectic.

In opposition to this hypothesis, it may be observed, that the state produced by pressure upon the brain, although it resembles sleep in the partial abolition of the faculties, yet it differs from it in some essential particulars, and it would be difficult to comprehend how some of the circumstances, which are known

2. p. 410..421, also in the art. "Somnambulism and Animal Magnetism," in Cyc. of Med. See also Rostan's Articles, "Somnambulisme," Dict. de Méd. t. xix. p. 363 et seq., and "Magnetisme Animale," Ibid. t. xiii. p. 421 et seq.

[1] Elements, v. i. p. 327.

[2] This opinion is elegantly detailed by Willis, in his treatise, "De Anima Brutorum;" Opera, t. ii. p. 128 et seq. Boerhaave supposed that the proximate cause of sleep consisted in a deficiency of animal spirits being carried to the brain, but that this deficiency might arise either from the exhaustion of the spirits, or from the pressure of the blood upon the brain not permitting the spirits to be conveyed to it; Prælect. § 593..5. t. iv. p. 254, 5. The section "De Somno," although in many parts not correct, according to our present opinions, affords a good specimen of Boerhaave's perspicuous method of analyzing and abstracting the more obscure parts of the animal œconomy; § 590..600.

[3] Although this may be regarded as the fair inference from Haller's observations, I must remark, that he states his opinion with certain limitations, and with his usual circumspection; El. Phys. xvii. 3. 9.

[4] On Man, Prop. 7. p. 45..8.

to promote sleep, can act in producing an accumulation of blood in the vessels of the brain. Besides, it seems to confound the natural with the morbid state of the system, for all those cases in which sopor is produced, by pressure upon the brain, are regarded as indications of some of the most dangerous diseases, whereas the state of sleep is a regular process of the animal œconomy, which cannot be supposed, either the cause or the effect of any morbid action [1].

The following remarks, which are principally deduced from the speculations of Cullen [2], may tend to throw some light upon the subject. We are led to regard the different functions of the animal œconomy as producing their ultimate effect by a kind of mutual action and re-action, one serving, as it were, to counterbalance another, so as to form an harmonious result from the combined operation of the whole. In this way the sources of expenditure are adjusted to those of supply, and we shall always find that there is some method of providing for the regulation of any excess or defect that may take place. Many facts lead us to conclude it to be a general law of the nervous system, that it is incapable of acting, for any length of time, without being exhausted, and requiring an alternation of repose. This applies equally to the organs of sense, to the muscles that are under the control of the will, and to the intellectual powers. Now, during our waking hours, a variety of actions are going on, which tend to produce this exhaustion, and sleep is the period when the nervous functions are recruited [3].

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[1] Blumenbach conceives the proximate cause of sleep to consist in a diminished or impeded flow of arterial blood to the brain, a conclusion which he deduces, partly from a consideration of the remote causes of sleep, and partly from the effects which are known to be produced upon the functions of the brain by the abstraction of this fluid; Instit. Physiol. by Elliotson, § 322. But, upon this hypothesis, it may be remarked, in the first place, that it would be very difficult to show how some of the remote causes of sleep can produce the effect which is contemplated; and, secondly, that sleep, which is a natural and salutary process, has any real resemblance or analogy with the morbid state which is produced by a deficiency of arterial blood.

[2] Physiol. § 124..133. In the following observations, I have not employed the phraseology of Cullen, which is encumbered with the speculation of the nervous fluid; but the existence of this fluid is not essential to the hypothesis. Bichat's theory of sleep is essentially the same with Cullen's; he lays it down as a general law of the animal functions, that they have all alternations of action and repose; this intermission, if long continued, and especially if extended to any number of these functions, constitutes sleep; for the most part, the action of the animal functions is partially suspended, and it takes place in some of them only, and in proportion to the degree so is the sleep more or less profound; Sur la Vie, &c. art. 4. § 3, p. 27..9.

[3] That sleep is an affection of the functions that depend upon the nervous system, is proved by the fact, that those animals require the most sleep whose nervous system, and especially the central part of it, exists in the greatest perfection. We have some judicious observations on the causes and effects of sleep in Reeve's Essay on Torpidity, p. 136..146. Magendie's remarks on sleep are contained in his Elem. Phys. t. ii. p. 460..5; his opinion respecting the relation of the nervous system are somewhat different from those

If we examine into the nature of the causes which tend to produce sleep, it may serve to illustrate this doctrine of the alternate process of exhaustion and reparation. These causes may be arranged under two heads; those which diminish the nervous sensibility, and those which prevent the sensibility from being excited into action. The most important of the first class of causes is fatigue, both of body and of mind, by which the nervous power is suspended, and the brain rendered insensible to the accustomed stimuli. The action of opium and of other narcotics may be referred to this head; inasmuch as, by diminishing sensibility, they render the brain incapable of receiving impressions. In the same manner, we may explain the effect of those causes which prevent the blood from experiencing the proper change in its passage through the lungs; such as the inhalation of carbonic acid. When death is produced by suffocation from fixed air, a profound sleep first comes on; and, if the process be not too rapid, the functions are gradually abolished, without pain or uneasiness of any kind.

The second set of causes that produce sleep, are those which act by preventing the sensibility from being excited. Every function requires for its continuance a certain force of impression or stimulating power, without which all action would cease. Could we withdraw from the system every thing which stimulates the muscles and the nerves, we should no longer have either motion or sensation. If, therefore, all external impressions are carefully removed, and the mind is prevented from dwelling upon its own ideas, sleep generally ensues. But it is obvious that the effect will take place with more ease and certainty, when the nervous energy has been previously diminished by the first set of causes; as it will, in this case, require a greater degree of stimulating power to produce the same effect. Hence the most favourable combination of circumstances for producing sleep are previous fatigue, not carried to excess, freedom from pain, absence of light and noise, a regulated temperature, a posture in which all the muscles are relaxed, and a tranquil state of the mind. Perhaps the state of the mind is the most important circumstance; for it is repeatedly found, that an interesting train of thought will completely banish sleep, when every thing else seems favourable for its approach. It is observed, that persons in a rude and savage state of society, those that are engaged in mere manual occupations, and young children, fall asleep imme-

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stated above. The article "Sleep," in Rees's Cyclopædia, may be perused with advantage. Dr. Carmichael, in the essay to which I referred above, after remarking that sleep depends upon something more than mere rest after fatigue, conceives that it is essentially connected with the process of assimilation, and particularly with the deposition of new matter in the brain; Trans. of Irish College, v. ii. p. 48 et seq. His first position is correct, but I must acknowledge that I see no evidence for the truth of the speculation on which he builds his hypothesis of the proximate cause of sleep, and still less his mode of accounting for the partial state of repose of the mental powers.

diately when the body is at rest. It is upon this principle that we account for the effect of any monotonous noise in producing sleep; as the humming of bees, the murmur of a fountain, or the reading of an uninteresting discourse. "If we examine this class of sounds," Prof. Stewart observes, "we shall find that it consists wholly of such as are fitted to withdraw the attention of the mind from its own thoughts, and are, at the same time, not sufficiently interesting to engage its attention to themselves."[1]

There is a state of the system which has been thought to be allied to sleep, but which is related to it rather in appearance than in reality; this is reverie. In the most profound reverie, as in sleep, external objects make but little impression upon the senses, or, at least, if the impression be made, the perception is not excited in the brain. But reverie differs from sleep in one essential particular, that in the former the faculty of volition is in its full exercise, and indeed it is upon the activity of this principle that the abstraction from surrounding objects essentially depends. The power of directing the thoughts at pleasure, of dwelling upon certain ideas, of excluding others, of preventing the intrusion of external impressions, and of turning the attention immediately from one object to another, constitutes the most perfect state of the human intellect, and one which enables the mind to reach the highest departments of science and philosophy. But, according to the usual order of nature, advantages are balanced by corresponding inconveniencies, and what is most anxiously sought after, and appears the most worthy object of desire, is often attended with some necessary evil. The state of the mental faculties, which has been described, is peculiarly liable to induce a habit of abstraction from the impression of external objects, which constitutes reverie, and which, if it be permitted to go beyond certain limits, disorders the functions both of body and mind, the body becomes languid and inactive, and the mind falls into derangement[2].

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[1] Elements, p. 329. I may remark, that the whole of sect. 5. p. 327 et seq. deserves an attentive perusal. Haller gives us some useful observations on the phenomena and remote causes of sleep, although combined with incorrect hypothesis; El. Phys. xvii. 3. 4..8. See also Blumenbach's Instit. Physiol. by Elliotson, § 321. Dr. Philip, as I conceive, is disposed to generalize too much, when he states that sleep is, in all cases, caused by the exhaustion of the brain and spinal cord; Phil. Trans. for 1833, p. 73 et seq.

[2] See Parry's Pathol. § 650. We have many just and philosophical observations on reverie in Darwin; Zoon. v. i. sect. 19; yet, in one point, he appears to me to be incorrect. He ascribes the state of reverie, either to the sensations of pleasure, or to the efforts of volition being so powerful, as to render us insensible to the ordinary impressions of external objects. But, I conceive, it is the latter case only which constitutes reverie; the former is merely to be resolved into the general principle, that a more powerful impression always renders us, to a certain extent, incapable of attending to a weaker. Perhaps, indeed, what I regard as an inaccuracy in Darwin, may depend rather upon his phraseology than upon his actual conception of the subject; as when he speaks of sensations of pleasure, it may be that he means to express the ideas of these sensations, which so exclusively occupy

the mind, and are forcibly retained there. That this is the case may be conjectured from his "Definition or Character of complete Reverie;" § 10. I must remark, that in the nomenclature of Darwin, the terms irritation and sensation are nearly equivalent to what I have denominated respectively nervous action and perception. We have occasionally very singular examples of long protracted sleep, where no other obvious derangement exists in the animal œconomy; a remarkable example of the kind is narrated in the Quart. Journ. v. i. p. 121.

CHAPTER XXII.

OF THE DECLINE AND DISSOLUTION OF THE SYSTEM.

I HAVE endeavoured to show, in the last chapter, that the final cause of sleep is to afford rest to the nervous system, by which its functions may be recruited, after the expenditure of power, which necessarily occurs during our waking hours. It has, in like manner, been shown, in former parts of this work, that all the components of which the body is composed, as well those which serve for the exercise of the contractile, as of the sensitive functions, receive a regular supply of matter, for the purpose of repairing the losses that are continually going forwards, from the different actions that have been described. Provided the due proportion of rest be obtained, and an adequate supply of matter be afforded, the process of reparation nearly keeps pace with that of expenditure, but still, under the most favourable circumstances, a gradual tendency to ultimate decay is engrafted in our system; and although we may escape the shocks of disease, and the various accidents to which we are at all times exposed, still age makes its gradual advances, and brings with it the inevitable destruction of our corporeal frame. This process may be observed in all our organs, and will be found to affect every function. I shall briefly trace its operations as they manifest themselves in the different parts of the animal œconomy, beginning with the membranes, the bones, the muscles, and the nervous matter; then proceeding to the functions which depend upon contractility; and, lastly, those which are more connected with the exercise of the sensitive and intellectual faculties[1].

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[1] These progressive changes are elegantly detailed by Boerhaave, Prælect. § 434..480, t. iii. p. 291..374, and Aphor. No. 55. and 128, with Van Sweiten's copious commentary; by Haller, in the thirtieth book of the El. Phys.; and by Blumenbach, Inst. Physiol. sect. 44. A number of important observations on this subject are contained in Bichat's treatise "On Life and Death," a treatise which, I think, displays more marks of original genius than his longer and more elaborate performances. It is in the first part of this work, that his remarks on the natural progress of the system to dissolution will be found; see particularly art. 10. p. 108 et seq.; the second part refers to the cause of death when produced by violence, or by any morbid action. There is no part of Adelon's physiology, a work to which I have so often referred, which is more deserving of a careful perusal, than those portions of his 4th volume, in which he traces out the gradual development of the fœtus, and the progressive changes which the system afterwards experiences.

SECT. 1. *Changes in the Structure and Functions of the Body.*

The natural progress from youth to age is strongly marked, both in the texture and the composition of membrane. In infancy it is soft and relaxed, and contains a large proportion of jelly and water; but as age advances, the jelly gradually disappears, or is much diminished, and it loses a considerable portion of its water. For some time its elasticity seems to be increased, with the increase of solid matter, but it gradually acquires a greater degree of firmness, which not only renders it less flexible and less extensible, but finally less elastic[1]. This change is to be observed in all those parts which principally consist of membrane, as the ligaments, the cartilages, and the tendons, and more especially, in the vessels of all kinds. As affecting the blood-vessels, it was made the subject of an interesting train of experiments by Winteringham. He not only established the fact, that this change takes place in the vascular system generally, but he found that it took place at a different rate in the arteries and in the veins, its progress being more rapid in the former than in the latter[2], and observations of a similar kind were made by Haller[3].

The muscles undergo a change in their state as age advances, partly in consequence of the change in the state of the membrane, which enters so largely into their composition, and partly in consequence of the alteration of the fibres themselves. They become generally less contractile, while those that serve for the voluntary motions are less under the control of the will, or are less able to execute its commands. In some instances, parts that are originally muscular become tendinous, the muscular fibres being gradually absorbed, and tendinous matter deposited in their room. In some cases, the muscular, or other soft parts, become rigid, from a quantity of bony matter being deposited in them.

A very obvious alteration is induced upon the bones; they contain a considerably greater proportion of phosphate of lime, and are thus rendered harder and more brittle; some parts, which are tendinous or cartilaginous in infancy, become gradually converted into bone as age advances, and from this cause the body becomes less moveable in its different parts, the motion of the joints is diminished, and the contraction of the muscles is impeded[1].

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[1] Haller, El. Phys. xxx. 1. 12.

[2] Experimental enquiry; see especially ex. 6, 7, 8, 9, 10, 11, and remarks, p. 31, 35..7. Winteringham's work may be regarded as a very valuable series of statical experiments, performed, as it appears, with great minuteness, and leading to many important results. The direct conclusions that he draws from them are, in most cases, the legitimate deduction from the facts, but the indirect conclusions are frequently incorrect, in consequence of his viewing the actions of the animal œconomy too much as mere mechanical operations.

[3] Ubi supra; also xxx. 2. 1..4, and xxx. 3. 1.

The circulation is affected by the change which takes place in the relation between the arteries and the veins, the arteries being rendered less distensible and less contractile; a larger proportion of blood is therefore deposited in the veins, which consequently become overloaded and distended. Another, and perhaps a still more important change in the state of the sanguiferous system depends upon the smaller proportion of fluids generally, and of blood in particular, which exists in the body as age advances. Every one of the textures, membrane, bone, muscular and nervous matter, and consequently, all the various organs, contain a greater proportion of fluids in the early periods of life; and, under ordinary circumstances, we may perceive a regular gradation from a more fluid to a more solid consistence, until, as it would appear, the quantity of solid matter becomes incompatible with due performance of the functions.

From both these causes there will be a diminution in the relative quantity of the arterial blood, a circumstance which must materially affect all those operations which are more directly connected with the circulation. A less quantity of blood will pass through the pulmonary vessels, so as to give less opportunity for it to be acted upon by the air; all the secretions therefore which are furnished from the arterial blood will be diminished in quantity or deteriorated in their quality. Hence the digestion will be impaired, and thus the supply of materials will be cut off for the immediate support of life. The changes that have been described are so connected together, that any one deficiency obviously induces a derangement of the whole system. Thus the diminished quantity of arterial blood decreases the velocity of the heart, and by causing it to propel its contents with less vigour, the blood stagnates in some of the minute vessels, and thus lays the foundation for still further derangement. Among these is a diminution of temperature, which is occasioned by several concurring causes, and which again, in its turn, increases the evil, by not supporting the contractility of the muscular fibre.

The changes that take place in the nervous system are no less remarkable than those in the muscular. The composition of the brain is altered; it is gradually rendered firmer, and, as in other parts of the body, the quantity of blood transmitted to it is considerably diminished[2]. The sensitive functions, all of them, experience a corresponding change; the different organs of sense become less adapted for receiving the impressions of external objects, the nerves transmit them less readily to the sensorium commune, and the perceptions which they excite there are less vivid.

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[1] The mechanical form of the bones in the fœtal state was made the subject of a separate work by Albinus; Icon. Oss. Fœtus.

[2] We have an interesting series of comparative experiments, on the state of the brain in old age, by Desmoulins, Journ. Phys. Juin, 1820.

The state of the intellectual faculties undergoes, at the same time, a gradual progress, which bears a relation to these alterations in the physical powers. In infancy the brain seems principally adapted to receive those impressions which are connected with the contractile functions. During childhood the mental powers gradually unfold themselves, and the different faculties rise up in succession, according to the operation of external objects, modified by the peculiar organization and innate propensities of the individual. In youth the impressions upon the senses are the most vivid, there is a greater rapidity of conception, and from this cause there is a greater tendency to form powerful associations. This, therefore, is the season of fancy and imagination, while the power which the mind possesses of associating its ideas with facility gives strength to the memory, and enables us to retain the knowledge which is acquired. As age advances, impressions on the organs of sense produce less effect, new associations are less easily acquired, and in general the mental faculties have more difficulty in undergoing any kind of alteration. While former habits are retained with greater force, and while old associations are recollected with peculiar correctness, recent events are forgotten, and new modes of life are with difficulty had recourse to. At length the powers of the system entirely fail; the external senses become callous to the impression of surrounding objects, and the mental faculties become irregular and uncertain in their operation. Although the decay of the physical and of the intellectual powers does not always proceed with equal rapidity, yet the respite which may be granted to either is not of long continuance.

SECT. 2. *Causes of Dissolution.*

The successive stages of growth, maturity, and decline, are necessarily connected with our constitution, and must therefore depend upon some invariable law of the animal œconomy. The changes which take place in the constitution and structure of the body, as described above, may be considered as sufficient causes of its necessary and inevitable tendency to decay. Physiologists have not, however, been satisfied with this general conception of the subject, but have endeavoured to point out more minutely the intimate nature of the alterations which were observed to take place, and thus to discover what may be technically termed the proximate cause of natural death. Boerhaave is the first writer whose speculations on this subject were so far matured, or deduced from correct observations, as to entitle them to any detailed examination. His hypothesis was founded, in a great measure, upon mechanical principles, in connexion with the doctrines of the humoral pathology. It proceeded upon the idea, that the motion of the fluids through the vessels, and the friction which must be thus necessarily produced, will tend to destroy the texture of the parts; at the same time all the fluids of the body are gradually converted from a bland into an acrimonious state, while the elements of which they are composed are transformed from those which constitute the fluids, to such as enter into the composition of solid substances. He, however, principally insists upon the physical changes in the vessels, upon their partial obliteration, upon the increasing rigidity of their texture, and upon the greater proportion which the solids generally bear to the fluids.

The opinions of Boerhaave were, most of them, adopted by Haller, and were considerably amplified and supported by new facts and arguments. He also made a very important addition to the hypothesis, by introducing the consideration of the vital properties of the system, which had before been scarcely taken into account. Haller supposes, that not only the texture of the body is rendered more solid and less flexible and elastic, but that the powers of contractility and sensibility are essentially diminished, and that the physical change which the body experiences is derived in part from the diminution of these powers, as well as of that of the organs through which they operate[1]. Although it might be very difficult to decide absolutely upon the question, in what degree the structure and functions of the body primarily influence each other, or to adduce any absolute proof, that the functions may be deranged without any derangement of the organs, yet, I conceive it to be more agreeable to the general analogy of the animal œconomy to suppose that this may be the case, than that the powers of vitality, in all instances, bear an exact ratio to the condition of the organs.

This view of the subject was adopted by Cullen, who, according to his usual custom, has compressed into a short compass an elegant summary of his doctrine. He proceeds upon the three principles, that there is a different distribution of the blood in the different periods of life, that the vessels offer a greater resistance to the entrance and transmission of the fluids as age advances, and that the excitability of the system is gradually diminished. In youth the quantity of blood is the most considerable; the arterial system is always in a state of overdistention, and, from the greater contractility and sensibility of the system, has a tendency to increased action. On this depends the growth of the body; the functions are all in an active state, a large quantity of blood is formed, and this is deposited by the arteries in the different glands or organs of secretion, from which the materials of the body are composed. This addition of new matter, and the force of the circulation, distend the different parts and add to their bulk. After some time the addition of matter, and the degree of extension, resist the further continuance of the process, and the power of the arteries is so balanced to the condition of the system as to enable it to retain its present state[1].

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[1] El. Phys. xxx. 3. 3, entitled, "Vis insita et nervosa minuitur."

But this balance is soon destroyed by the diminished action, both of the muscular fibre and of the nervous matter, in consequence partly of the decline of their powers, and partly from the diminution in the quantity of arterial blood that is sent to them. At the same time the veins being more distensible than the arteries, and having experienced less alteration in their texture, and partaking also less of the vital actions of the system, the blood is more disposed to accumulate in them. There are many facts in pathology which appear to countenance Cullen's hypothesis of the arterial plethora in youth, and the venous plethora in old age, and it seems likewise to coincide with the state of things, which might be expected to ensue, from the actions of a system so arranged and so organized. From calculations that have been made, combined with anatomical observations, it is found that the growth of the heart does not keep pace with that of the sanguiferous system generally[2], while, at the same time, we learn from the experiments of Winteringham, that the arteries become firmer, and would consequently require a greater force to preserve them in the same state of distention. The veins, being the less active part of the circulating system, and being chiefly of use as reservoirs, to contain the blood and suffer it to return to the heart, after it has performed all its functions, and is reduced to what may be regarded as an inert state, thus become surcharged with blood, and it appears, as a matter of fact, that their relative capacity is increased[3].

The deficiency of the force of the arterial circulation necessarily produces another effect on the body, to which we have already alluded, and which must materially assist in explaining the changes that take place in the functions. When the blood is propelled with less force than ordinary, the deficiency will be first experienced in the minute or capillary branches of the arteries, and these are in fact found to be much diminished, and many of them to be even entirely obliterated. Anatomists are well acquainted with this circumstance, and have frequently occasion to observe, that certain parts of the body, which are vascular in youth, as age advances become entirely solid[4].

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[1] Many parts of Cullen's hypothesis may be regarded as deduced from, or at least as confirmed by, the experiments of Winteringham; this was particularly the case with the change in the distribution of the blood, and in the relative strength of the arteries and veins; see Exp. Inq. p. 29, 30, 32, 35..7. 187, 8.

[2] Cullen's Physiol. § 298. p. 249.

[3] Winteringham's Inquiry, p. 29, 0.

[4] The gradual obliteration of the capillary arteries, during the progress of life, was a subject to which Ruysch particularly attended, and which, in consequence of the minuteness of his injections, he proved to take place to a degree that had not been previously suspected; Advers. Decas 2. pars 4. Op. t. ii.; Haller, El. Phys. xxx. 1. 12. The dexterity of the modern anatomists

I have remarked, in a former part of this work, that all the changes which the blood experiences, and all the actions which it produces, take place in the capillaries of the arteries, and that the large trunks are to be regarded principally as tubes, by which the blood is conveyed to the extremities. The diminution of these capillaries must obviously and directly impair the functions of every part of the system, and will explain the diminished activity of both the mental and the corporeal powers.

Another physical cause of decay in the system is a want of due correspondence or co-operation between the different functions, and especially between those of assimilation and of absorption. I have formerly explained the nature of that constant change which is going forwards in the system, by which the particles of matter are undergoing a kind of circulation, the old ones being removed by the absorbents, and new ones deposited in their place by the secreting arteries [1]. It sometimes happens, that these two sets of actions do not correspond, or proceed with a due relation to each other; at one time secretion goes on too rapidly, and the body becomes bulky, while, at another time, an opposite state produces emaciation. These are not indeed to be regarded as constituting disease, when existing within certain limits, but when excessive they lead to derangement of the machine, and ultimately prove inconsistent with health. And besides an excess or defect in the quantity of action, the functions occasionally seem to acquire a wrong direction; in some cases an unnatural formation of adipose matter takes place, which oppresses and impedes many of the operations of the system, and, in other instances, still more serious evils arise from the deposition of bone in membranous parts, especially when they are connected with the vital functions, as, for example, in the valves of the heart or the large arteries [2].

From the combined operation of all these causes, we may easily conceive how all the functions will be liable to become deranged, and that this derangement is not merely the effect of accident, but is the natural progress of the constitution, and the inevitable fate of animal existence [3]. After the view which I have

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has made us familiar with an occurrence which was formerly regarded with much astonishment, or even with a degree of scepticism.

[1] It is from this circumstance that Haller styles the body, "Machina, quæ se ipsum et destruit et instaurat;" El. Phys. xxx. 2. 6.

"Nostra quoque ipsorum semper, requieque sine ulla,
Corpora vertuntur: nec, quod fuimusve, sumusve,
Cras erimus." Ovid, Metam. xv. 214..6.

[2] Haller treats of this cause of decay in El. Phys. xxx. 3, 7; but many of the facts which he adduces, of solid concretions in the soft parts, are of a different kind from the bony indurations incident to old age; they are morbid calculous depositions. On the causes of what he styles natural death, see the remarks of Adelon, Physiol. t. iv. p. 466 et seq., also the paper of Dr. Philip to which I have referred above. On this subject the student may peruse with advantage the art. "Age," by Dr. Roget, in the Cyc. of Medicine, and by Dr. Symonds in the Cyc. of Anatomy and Physiology.

[3] What we may style natural death is a very rare occurrence, so that al-

taken of the laws of the animal œconomy, and an investigation of its wonderful mechanism, with all its adjustments and its contrivances, it may appear remarkable, that so admirable a structure should be intended to last for so short a space of time; and we might be tempted to regret, that what is so beautiful should not be more permanent. But the present state of things appears to be the general order of nature with respect to all organized bodies. Perpetual change is an essential quality of their constitution, and this system of change is experienced not only in the component parts of each individual, but extends to the individuals themselves. We have found that it does not depend upon any accidental circumstances or any partial imperfection, but that it is interwoven with our nature; and that as maturity succeeds the period of growth, so is maturity necessarily succeeded by dissolution.

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though the process which I have described may be supposed to be always going forwards in every individual, yet it is to a very few that it is granted to reach its termination. Haller estimates the average probability of human life, under the circumstances in which mankind are ordinarily placed, and deduces the conclusion, that only one individual in about 15,000 reaches the 100th year; El. Phys. xxx. 3. 15. There is, however, reason to suppose that in this country at least, the average length of human life is increased during the last thirty or forty years. See the art. "Longévité," by Rullier, Dict. de Méd. t. xiii. p. 277 et seq.

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———— opinion respecting the Action of the Absorbents...............
———— ———— the formation of the Voice ....

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Zinn's observations on the Iris ....
Zoognosis, Serres' account of the laws of..............................

THE END.

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